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PHILOSOPHICAL
TRANSACTIONS

From the Year 1741, to the Year 1750

A BRIDGE.

AND

OF THE GREAT HEAD

OF THE PAPER, by which the Paper

JOHN WATSON, Esq.

Is now printed by the University of Cambridge.

THE TEXT

OF THE

Part II. The Axioms and Medical Papers

AND

Part IV. The History and Medical Papers

AND

Part VI. The History and Medical Papers

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THE
PHILOSOPHICAL
TRANSACTIONS

(From the Year 1743, to the Year 1750)

ABRIDGED,

AND

Disposed under GENERAL HEADS.

The *Latin* PAPERS being translated into *English*.

By JOHN MARTYN, F. R. S.

Professor of BOTANY in the University of *Cambridge*.

VOLUME THE TENTH.

CONTAINING,

PART III. The ANATOMICAL and MEDICAL PAPERS.

AND

PART IV. The HISTORICAL and MISCELLANEOUS PAPERS.

L O N D O N :

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THE
Philosophical Transactions.

ABRIDGED.

PART III.

CONTAINING THE

Anatomical and Medical PAPERS.

CHAP. I.

ZOOLOGY, and the *Anatomy* of *ANIMALS*.

I. **E**XAMINING one day a number of the eels in four paste, and having a mind to view a single one, I placed the largest I could find in a small drop of water, under the microscope; in doing of which I found I had wounded him in the belly; a long slender tube proceeded from the wound, doubled in the form of an intestine, which I then took it for.

The next day I communicated this to Mr *Turbeville Needham*; and having a mind to see the *viscera*, as we then thought, he cut one in two, near the middle; when, to our great surprize, we found this part had shot out from each of the divided ends, and a number of seeming *ova* issued from them; but we soon found, that these were really live eels, included in their proper membranes, though of different degrees

Concerning the minute eels in paste being viviparous; in a letter from Mr James Sherwood, Surgeon, to M. Folkes, Esq; P. R. S. No. 478. p. 67. Jan. & Feb. 1746. Read Feb 13. 1745 6.

VOL. X. Part iii.

of maturity. Some moved but slowly their head and tail, others coiled and uncoiled themselves pretty briskly ; and indeed it was pleasant to behold the most mature making many efforts to disengage themselves from their enveloping membranes, frequently varying their position, being sometimes spiral, then like a figure of 8, till at last they were entirely at liberty, and swam about like the parent eel.

By this it appears, that what I at first took for the intestine, we now found to be the *uterus*, which shews, in the large eels, to be full of dark spots, and are the young eels. These dark spots are also observable in the young ones, as soon as delivered from their parent.

To be sure of this experiment, I repeated the operation on numbers of these eels in presence of Dr *Parsons*, and several other Gentlemen, and always found young ones ; some so mature (*viz.* those towards the middle of the parent) as to disengage themselves before I could get them under the microscope.

In cutting one of these eels, I happened to divide a young one exactly in two ; but, notwithstanding the globular appearances, as in the parent, there issued nothing from the wounded extremity of either part ; which must be owing to the immaturity of the young ones.

From this it is plain, that these eels are viviparous ; and consequently cannot favour the common opinion among Naturalists, That all kinds of animalcules are produced by minute eggs floating in the air, and falling into the different *matrices* and *pabula*, that sustain each kind.

*A letter from
Mr H. Baker,
F. R. S. to the
Pres. concern-
ing a new
discovered
Sea-Insect,
which he calls
the Eye-suck-
er. No. 472.
p. 35. Jan.
&c. 1744.
Read March
8. 1743-4.*

Fig. 1.

Fig. 2.

II. I was lately favoured with a couple of small sea-insects, by a Gentleman, who told me, they were found fixed by the snout to the eyes of sprats ; that they are often observed sticking there, and may consequently be supposed to suck their nourishment from thence.

As I do not remember this insect has been yet mentioned by any body, I hope I shall be excused for laying before you a short description of it, with a drawing thereof, as seen by the naked eye, *Fig. 1.* and another as magnified by the microscope, *Fig. 2.* I shall likewise beg leave to distinguish it by the name of *eye-sucker*, as that name conveys an idea of the manner how it lives.

The length of this little creature from end to end is near 3 inches, whereof the head is about $\frac{1}{4}$. It's body is somewhat thicker than a hog's bristle, and of a pleasant green colour. A gut seems running through it, and terminates at the *anus*. The head is light-brown, twice the thickness of the body, and of an oblong figure tapering towards the snout. It has a pair of fine small black eyes, and a couple of holes, at some distance forwards, which probably are it's nostrils.

But the most remarkable part of the head is it's *proboscis* or snout ; which is nearly $\frac{1}{2}$ it's length, and does not end in a point, but spreads at it's extremity with a considerable aperture. This snout appears of a horny substance, and has, on every side, several large knobs or protuberances ;

tuberances; whereby, when once insinuated into the fish's eye, it must necessarily be fixed there, so as not easily to be removed. But this the figure will more expressively demonstrate.

As the insects, whence I take this description, were dead and dry, it cannot possibly be so exact and particular; and is more liable to mistake, than if they had been living. But I hope it may serve to give some idea of this odd creature, till a better can be obtained from some curious observer, who may get a sight of it alive.

III. 1. We find, in divers places, upon water-plants, and other bodies in the water, a whitish substance, that looks at first only like a sort of mould: we sometimes see plants, sticks of wood, snail-shells, and the like, that are entirely covered over with this substance. But if we take any of these, put them into a glass of clear water, and then examine with a magnifying glass what is upon them, we soon discover, in the little bodies, that, by their assemblage, form this whitish substance, such motions as give sufficient reason to look upon them as living animals; and this will appear yet more sensible, when they come to be observed with a microscope. We then find them to be minute bodies, severally fixed to the extremities of small stems, or pedicles, many of which are often so united, as to form together a sort of branches, or *clusters*; and this sort of appearance determined Monsieur De Reaumur to name the *animalcula* that appeared so fixed, *clustering polypi: des polypes en bouquet*. These clusters are larger or lesser, according to the species of the *polypi* that form them, and according to the concurrence of many other circumstances. To get a clear idea of the figure of these animals, it is best to observe the smaller clusters; as, in the larger, the great number of the *polypi* upon the several stems, are apt to hide one another. There is a case, I shall mention presently, where the *polypi* are single; and it is proper to observe them in that case; and the rather, because that is the way to discover how the clusters are formed.

I shall now describe one of these single *polypi*, to give a general idea of the form of the animal: and I shall herein chiefly endeavour the description of that species which I have most particularly made my observations upon. These are not in length above $\frac{1}{40}$ of an inch, and are of a shape nearly resembling that of a bell: this may be seen in the figure, where one of them is represented exceedingly magnified *Fig. 3.* The anterior part, *ac*, generally appears open, when it properly presents itself; the posterior part *ib* is fixed to a stem or pedicle *be*; and it is by the extremity *e* of this pedicle, that the *polypus* fastens itself to any other sort of body. The *polypus* of this sort generally appears to the microscope of a brownish colour, excepting at it's smaller end *b*, where it is transparent, as well as it's pedicle *be*. When the anterior part *ac* is open, one may perceive about it's edges a very lively motion; and when the *polypus* presents itself in a certain manner, it discovers, on either side of these edges of it's anterior part, somewhat very

Observations upon several newly discovered species of fresh-water Polypi; by Mr Abraham Trembley. No. 474. p. 171. Read Nov. 22. 1744.

much resembling the wheels of a little mill, that move with great velocity.

These *polypi* are able to contract themselves; and they do so often, and suddenly. They may be brought to contract at any time, either by touching them, or by moving the body to which they are fixed. When they contract, the edges of their anterior parts are drawn quite into their bodies; and when they resume, which they do soon after, their former posture, one may distinctly see those edges come forth again, and put themselves in motion, as before.

When one looks about the anterior parts of these *polypi*, which are open, and whose edges are in motion, one may frequently have an opportunity of remarking a number of very minute bodies swimming in the water, that seem to be forced down with velocity into these openings of their anterior parts, and that sometimes are thrown out again from thence.

To make this observation the most sensible, it is best not to look at a single *polypus*, but a cluster of some numbers of them together.

I have taken notice, that the *polypi* of the sort in question, appear of a brownish colour when viewed with the microscope; I should now add, that having left some of them for several days in the same water, they by degrees lost their brown colour, and became transparent; excepting only that a few grains or spots of brown or black, still continued to be discernible in their bodies: but, having afterwards removed these *polypi* into other water, newly taken out of a ditch, they in a little time resumed the same brown hue which they had before.

It may commonly be observed, that when the *polypi* are in water newly put to them, there fall upon their anterior parts far greater numbers of the above-mentioned minute bodies, than when they have been left for any time in the same water. It is very probable, that these minute bodies are exceedingly small animalcules, upon which the *polypi* feed; and that, consequently, the opening which they have in their anterior part, serves them for the purposes of a mouth.

The *polypi* that have become transparent, and been left some time without the addition of such water as would make them recover their brown colour, have also, at the same time, left off multiplying. But I have observed that others of them, to which I have afterwards given new water from the ditch, have soon after begun to multiply again.

These *polypi* are capable of swimming about; and when they swim, they are no longer in clusters, but always single; and they do not then appear in the same form as when they are fixed, and open at their anterior ends. It is by swimming that they leave the place to which they first appeared fixed, and that they go and fix themselves to any other body that they find in their way.

One should begin to observe a *polypus* soon after it has fixed itself singly, in order to see regularly in what manner the clusters form themselves, and in what way these small creatures multiply.

The

The stem or pedicle of a *polypus* that is yet single, and which has but lately fixed itself, is at first very short, but it lengthens itself in a little time. After that, the *polypus* multiplies; that is to say, it divides or splits itself into two lengthwise. One first observes the lips to be drawn into the body, whose anterior part closes, and becomes round: the motion that was to be seen before the lips were drawn in, no longer appears; yet may one see, by looking with attention, a slow motion within the body, during all the time that the *polypus* remains closed. The anterior part of the *polypus* flats itself afterwards by degrees, and spreads in proportion, becoming broader as it shortens; it then gradually splits down through the middle, that is, from the middle of the head to the place where the posterior end joins to the pedicle: so that, in a little while, there appear two separate round bodies joined to the extremity of the pedicle that just before supported but one.

The anterior part of each of these bodies then opens by degrees; and, as they open, the lips of the new *polypi* shew themselves more and more. Then is the time of observing these lips with attention, and of forming an idea of their true form, and of their motion already spoken of. This motion is at first very slow, it quickens as the *polypi* continue to open; and, as soon as they have done, it becomes as swift as that which appeared in the lips of the whole single *polypus*, before it began to divide: and then these new *polypi* may be looked upon as entirely formed.

They are, at first, less than the *polypus* from which they were formed; but they grow to the same size in a very little time: A *polypus* is an hour, or thereabout, dividing itself.

To have a tolerable notion of this operation, one must have seen it divers times, and in *polypi* placed and situated in divers different ways. The lips of these *polypi* appeared to be composed of 4 or 5 transparent stripes, all which have an undulating motion. Whilst the *polypi* are opening, and the motion of their lips is yet but slow; one sees on either side, when they are in a certain position, what one is inclined to take for the wheels of a mill, in the *polypi* that are quite formed, and whose lips move very fast: but one now sees, while they are opening I say, what may be rather taken for 4 or 5 fingers on either side of their mouths; which alternately bend down and extend themselves every instant, and to which the transparent stripes above-mentioned appear to be fixed. This should be observed often, and in as many various attitudes as possible, to avoid being deceived by the taking of appearances for realities; which happens more or less to every observer, more especially when he first begins to observe. Before I venture to explain myself more particularly upon this last article, I shall endeavour to repeat and to pursue further, if I am able, some observations that I have at present only begun.

When the first *polypus* is thus divided, and the two new ones produced by this operation are thus completed; one sees on one pedicle two *polypi*, joined to it's extremity by their posterior ends, and that shew themselves on the sides of each other, as in *Fig. 4.*

Fig. 4.

The ordinary proportion between the length of the body of one of these *polypi*, and the length of their pedicle, is pretty exactly observed in the drawings. Soon after the separation is compleated, each of the new *polypi* begins to shew a pedicle of it's own.

I have often had occasion to take notice, that each of the new *polypi* had, the day after their separation, a pedicle of a tolerable length; and that these new pedicles united at the extremity of the first pedicle, as the branches of a tree unite at it's trunk. Several of the *polypi*, upon which I have made continued observations, have multiplied at the latest 24 hours after their first separation. The new cluster has then consisted of 4 *polypi*, each of which had it's own pedicle; as every one has also had, that was afterwards produced by a new separation.

Fig. 5.

Fig. 5. represents a cluster of 8 *polypi*; and by this figure it may be apprehended in what manner the pedicles of the *polypi* become disposed, as their numbers increase. These several pedicles become so many branches of the cluster or sprig.

This figure particularly represents a cluster, whose progress I followed in *Sept.* last, 1744. It consisted, on the 9th, but of one single *polypus*, which was placed as at *b*: this *polypus* divided itself that evening, and at 8^h. 30' there were to be discovered at *b* two perfect *polypi*, whose pedicles or branches, *b d*, *b d*, continued lengthening till the morning of the next day, at about 9^h. 15' that morning, these two *polypi*, which were then at *d*, *d*, began also each to divide; so that at 11^h. 15', there were at *d* and *d* 4 compleat *polypi*, whose several pedicles *d i*, *d i*, *d i*, *d i*, formed themselves soon after. On the 11th, about 7^h. 30' in the morning, I found that these 4 last *polypi* had already again divided themselves; that is, there were at *i*, *i*, *i*, *i*, 8 distinct *polypi*; and this cluster is here represented as it appeared upon the 12th, between 10 and 11 in the forenoon.

The *polypi* are not always ranged as they are disposed in this fig. for it often happens, that the pedicles and the *polypi* are behind one another, so as to form a groupe, in which some of the *polypi* may chance to be hidden or covered by others, either entirely, or in part. This fig. represents the *polypi* and pedicles as magnified to the same degree as those already exhibited in the former figures. I have taken notice of clusters, the numbers of whose *polypi* have constantly gone on doubling, from 2 to 4, from 4 to 8, from 8 to 16, from 16 to 32: after which I have no longer been able to count exactly the number of the *polypi*. I have said enough, to shew how the clusters are formed, and how fast these small animals multiply: indeed the number is prodigious of those that are sometimes found in the water.

I have large glasses by me at this time, *Nov.* 1. 1744. *N. S.* in which they have exceedingly multiplied; there is particularly, in one of them, a cluster composed of several lesser united clusters, which is above an inch over every way. There detach themselves from time to time single *polypi*, which go swimming about till they fix each upon some body or other;

other; and from these there again arise new clusters, in the manner above spoken of. The branches, from which *polypi* have detached themselves, still remain fixed to the cluster, but they bear no more *polypi*; and after all the *polypi* of a cluster have thus detached themselves from it, the assemblage of the branches still subsists, but is of no further use.

I know of 4 other species of *polypi*, that all increase in the same manner as those I have been already speaking of; that is, split and divide themselves according to their length. Those which come the nearest to the first, are somewhat more slender, and the branches of their clusters are transparent; yet do they appear, when there is a number of them together, of a changeable violet colour: the clusters of these bear a good resemblance to a sprig of spun glass. When these last animals are completely formed, it is not so easy to see distinctly in them the motion of their lips, as it is in the other species before-mentioned; yet may it be observed in these also, whilst they are still opening, and completing their formation: for at such times, this motion is but slow, whereas it becomes afterwards very quick in those that are entirely perfected.

The *polypi* of the other species that I have observed, are yet less than the last: they are shorter, but more open and hollowed at their anterior ends. These have a character that sufficiently distinguishes them from all the other species: their stems and branches have a motion that is not to be found in those of the other *polypi*. These stems draw themselves up, and shorten all at once, taking the form of a spiral wire or screw; and a moment after they again resume their former shape, stretching themselves out straight as before.

These several species of *polypi* I have been speaking of, all multiply in vast abundance; but they have also enemies that destroy immense numbers of them, and that in a very little time.

I have also this summer observed regularly other small *polypi*, of a different sort from those that are found in clusters. These are nearly in shape like a tunnel, pretty long in proportion to the opening of their larger larger ends. For this reason, M. De Reaumur has thought proper to distinguish them by the name of *tunnel-like polypi*. I am acquainted with 3 species of these last *polypi*, which are respectively, green, blue, and white. These must also be observed often, and in various attitudes, in order to obtain a tolerably exact notion of their proper structure. Their anterior end particularly, is of a far more compounded shape, than one would at first imagine. There may be discovered, round the edges of this part, a sensible motion, much resembling that of an indented wheel, or rather of an endless screw, that is turned very fast about. These *tunnel-like polypi* form no clusters, like the others.

I have remarked, that the little bodies, that pass swimming near the anterior parts of these insects, are in some manner drawn into the mouths of their tunnels; and I have sometimes seen a considerable number of very small round *animalcula* fall one after another into these openings.

Some

Some of these were indeed afterwards let out again, at another opening, which I am not yet able particularly to describe: but I could plainly see, that many of these little round bodies remained within the bodies of the *polypi*; and it is therefore apparent, that these little bodies, so taken in, became their food. These *tunnel-like polypi* do also multiply by dividing themselves into two, but they divide themselves otherwise than the *clustering polypi*: they neither divide longitudinally nor transversely, but sloping and diagonal-wise. Of two *tunnel-like polypi*, just produced by the division of one, the first has the old head and a new posterior end; and the other the old posterior end, with a new head. I shall call that which has the old head, the *superior polypus*; and that which has the new head, the *inferior* one. The first particulars observable in a *tunnel-like polypus* that is going to divide, are the lips of the inferior *polypus*; I mean those transparent edges that are so conspicuous in the *polypus* when entirely formed. These new lips first discover themselves upon the *polypus* that is going to divide, from a little below the old lips, to about $\frac{2}{3}$ of the length of the *polypus*, reckoning from the head: but these new lips are not disposed in a straight line, according to the length of the *polypus*, but run sloping near half way round about. These lips are known by the motion in them, but which motion is at first very slow. That portion of the body of the *polypus*, that is bounded by these new lips, then gathers up itself, the new lips insensibly draw together and close; whereby there forms itself, at the side of the *polypus*, a swelling, that is soon found to be the head of the new one, bounded by the new lips first discovered. Before this swelling is grown very remarkable, one begins to distinguish the two *polypi* which are forming themselves; and when that swelling is considerably increased, the two *polypi* will be discovered, no longer joined but by a small portion to each other. The *superior polypus* no longer adheres to the inferior one, but by its posterior extremity, which is still fixed on one side of the *inferior polypus*: the *superior polypus* then begins to make motions that seemingly tend to the separating of him from the other; and in a little time he becomes quite detached, swims away, and fixes himself elsewhere. I have seen one come and fix at the side of the *inferior polypus*, from which he was just before separated. The *inferior polypus* remains fixed in the same place, where the *polypus* was that is now divided, and of which he was only the half, before the division took place. I am not, at present, able to enter into a further detail of the manner in which these *tunnel-like polypi* divide and multiply themselves. I could not do it, without the assistance of many figures, nor without the mention of several other facts, that I have not yet sufficiently satisfied myself about, nor observed so often as I think it necessary to do.

I shall also endeavour to carry on further the Natural History of all the several *polypi* of which I have yet spoken, and, perhaps, that of some other sorts besides; as I find, that the experiments I make upon
infects

insects of one species, facilitate in several respects those I have to make upon others; and that these last often throw a new light upon observations and experiments already made.

As all these little animals are exceedingly minute, I have hardly been able to observe any of the several facts above-mentioned without the assistance of the *microscope*; but, if I was to take such small objects out of the water, in order to expose them to my glasses in the common way, I should both risque the losing of them, and hazard the putting them out of a condition of performing their natural operations. I am therefore forced to observe them with the magnifiers of my microscope, without taking them out of the glasses I keep them in. For this purpose, I contrive to get them so near the sides of those glasses, that the *foci* of my magnifiers may reach them from without: I then fix, by the sides of my glasses, a jointed arm I have fitted for that purpose, into the socket of which I can conveniently screw the different magnifiers of my microscope, and retain them fixed at their due distances, by which I am able, with great ease, to keep the *animalcula* in sight as long as I have occasion for them: and I use for the most part the light of a wax taper, to illuminate my objects.

2. I have since my last missed no opportunity of pursuing my inquiries concerning these insects, and whilst I have been searching for them in different waters, I have occasionally discovered several other sorts, to which I was before a stranger; but have not been able to refuse some share of my application to these also. The relations I found these new species of *polypi* had to those I was already acquainted with, and several particulars in which they greatly differed, equally determined me to observe them all, with as much care as I was able. I believe it will not be improper, to give the description of that *apparatus* I have made use of, and by which I have been enabled to make my experiments. This description will render what I have further to say the more intelligible; and will perhaps contribute to make others more capable of judging, what degree of credit should be given to the several facts, I may have occasion to relate. The principal benefit I received from it myself was, that I was thereby enabled to observe with the several magnifiers of my microscope, small water insects, whilst in a glass containing water sufficient to let them live therein, much in the same way as they would have done, had they still been in the ditches or other waters from whence they were first taken.

I have accustomed myself to keep great numbers of the small insects I make observations upon, in large glasses: and it is by observing what passes in those glasses that I endeavour to discover the more general facts, relating to the natural history of these animals. After which, I have found by many repeated experiments, that it is necessary to remove into glasses of a lesser size, like that represented in *Fig. 6.* such of the insects as are to be set apart for more particular and curious microscopical observations. I put water into these glasses, from the same ditches, out

— continued
by the same.
N^o. 484.
p. 627. Oct.
&c. 1747.
Read May 21.
and June 18.
1747.

out of which the insects I am observing have themselves been taken; and I shift this water more or less often as the circumstances may require.

It is easy to conceive, that to observe a small insect in one of these glasses, with a magnifier of a short *focus*, it is necessary that insect should be placed very near to one of the sides of the glass; and that it ought also to be kept steadily in the same place. The insect ought therefore to be either fixed to the side of the glass itself, or to some other body that may be conveniently so fixed. I chuse, for this purpose, substances that are slender and supple, such for example as the small branches or twigs of divers species of the *equisetum palustre*, or water horsetail. The clustering *polypi* are often found upon these twigs, and they may be made to settle upon them from elsewhere, as I shall take notice, by and by. Having chosen a small slip, upon which there is one or more of the clustering *polypi* or the like, I take a piece of a peacock's feather, longer or shorter, according to the diameter of the glass I am then to use. I cut away all the lateral branches or beards of the feather on both sides, excepting one at one of it's extremities; upon this one I make a knot near it's insertion, but do not at first draw it close. I then bring this open knot to the small slip of the horsetail that is floating in the water of the glass, and I get one of it's extremities into the knot, which I then draw close; and the slip of the horsetail is thus joined to the piece of the feather. I next take hold of the feather, and bending it near the middle, I force it's two ends *b, f*, into the glass, I then let go the feather which I before held, and it's elasticity forces it's two ends against the sides of the glass *A*, by which means the small twig *dl* of the horsetail I have been speaking of, and which was already fixed to the extremity of the feather *fd*, becomes also fixed close to the side of the glass; the consequence of which is, that the *polypus* that is sticking to the horsetail is obliged to remain in such a situation, as to be within the reach of a magnifier that is but of a short *focus*.

Nothing more is now wanting, but to place the magnifier before the object: for it would be both difficult and very inconvenient to hold it like a reading-glass in the hand. In the instrument *k, i, b, g, e*, I make use of, it is screwed into a ring fixed to a small branch *n, g*, which has a ball *g* at it's other extremity; this ball fits a socket, and so makes a joint, by which the first branch is joined to a second *b, i*, and that again in like manner to a third *i, k*, or fourth, if there is occasion. The foot of the whole is fitted near the edge, into a small board or tablet that holds the whole *apparatus*. By the means of these joints, the magnifier *e*, may be turned any way, and may be conveniently brought near to it's proper distance from the object, yet as the branch which holds it, cannot well be without some spring; it will be still difficult to adjust the object exactly to the *focus* of the magnifier when it is short, if only the magnifier was to be moved for that purpose; and it will therefore be found easier, when the magnifier is once right against the object,

to move gently the glass in which that is contained, till it is found to be precisely in the *focus* of the magnifier: and for this purpose the small board upon which the glass is placed, ought to be well smoothed. The light that comes in at a common window will be found sufficient, for observing in the water such objects as are to be seen with the bare eye, or with a hand magnifying glass; but such as must be examined with a *lens* of a shorter *focus*, must be viewed by the light of a taper, placed beyond the glass, and whose flame is so ordered as to be upon the level with the object. A magnifier thus once adjusted may remain in the same place before the object, for several days together, without being disordered; so that, to observe the progress of the insect during all that interval; no more will be necessary, than to place from time to time a taper behind the glass, and to apply the eye to the already fixed magnifier. Several of these *apparatus's* may be placed upon the same board by one another; and thus at the same time observations may be carried on upon different sorts of insects, or upon several insects of the same species; in order to come sooner and with more certainty at the knowledge of the facts one is enquiring about.

I could never have discovered the manner in which the clustering *polypi* are multiplied, but by the help of the expedient I have just described: and before I had the use of that *apparatus*, I only knew in general the figures of those *polypi*, and of the clusters that contained them. I had taken notice that those clusters grew, and I had reason to suspect, that a whole cluster came from a single *polypus*; but I still wanted to see this increase, and to find the moment of their multiplication; for I had reason already to suspect, from what I had seen with a glass I held in my hand, that these clusters did not grow insensibly like plants; but that on the contrary, the operation I wanted to see was performed in a short portion of time. To come therefore at that moment, I resolved to observe regularly for some time *polypi* of this sort with my microscope, whilst they should remain in circumstances, nearly as easy and as natural to them, as those they were in in their proper habitation. This it was that gave me the first thought of the above-described *apparatus*. And when I had prepared and fixed every thing, I set myself continually to watch for the moment of the multiplication of the clustering *polypi*; and I then found this moment, which I had so much wished to discover, the very same morning that I began to make use of my *apparatus*.

It was in that species of *polypi*, some of which are represented in *Fig.* 3, 4, 5, that I first discovered the manner in which these small animals are multiplied: and it is indeed among several species that I am now acquainted with, one of those in which this fact is the most easy to be observed. It is also in the same species easy to see that very odd motion, which they exhibit at their anterior extremity. This same motion, which has also place in other species of clustering *polypi*, is not in them so easy to be remarked; both on account that they are lesser, and also on ac-

count that this motion itself is swifter, than in the sort above-mentioned.

There is also to be observed at the anterior extremity of several other small insects, a sort of motion which has drawn the attention of all such as have happened to see it, and who have almost all been curious to inquire and satisfy themselves, whether those little wheels, which appear to turn with so swift and so regular a motion, are really wheels turning upon an axis or not. This has determined me now to mention that motion, tho' it is not my design to treat fully of it in this place, or to determine very precisely what I think about it: as I shall be very cautious how I assert any thing positively upon so nice a matter, until I shall have repeated again several experiments I have already made, and until I shall have tried several others. In order to discover what this motion might really be, I have applied my self not only to observe it in the same animal placed in different attitudes, but also in different species of water-insects in which it is seen, and I have compared the *phenomena* of all these several motions one with another.

All I have yet learned from these comparisons, and all the other observations I have made, seem to concur in proving to me, that there is some deception of the sight in the present case, and that the motion in question is not really what it at the first appears to be, a rotatory motion round an axis. And I even know some species of *polypi*, in which this motion is, comparatively speaking, but slow: and in these it is distinctly seen, that this motion, tho' in general resembling that observed in the others, is not a revolving or rotatory motion: such, for example, is the motion which is taken notice of in that species of *polypi*, which Mr *Leewenhoeck* has described in the 295th N. of these *Transf.* This is one of those insects whose motion is the most to be admired, and it is besides exceedingly curious upon many other accounts.

I have already said, that the motion in question is very slow in the clustering *polypi*, just when they are opening again after their division, and I am greatly mistaken, if it may not then be seen very plainly that this motion is not a rotation. The same remark may also be made on the tunnel-like *polypus*, and that, almost during all the time that it employs in it's separation.

I made use of an expedient, whilst I was observing the clustering *polypi*, whereby I was able to retard the quickness of their motion. I poured by little and little a small quantity of spirit of wine into the glass wherein they were kept. It immediately either abated the velocity of their motion, or took it quite away, according to the quantity of it that I poured in. That which follows both in the one and in the other of these two cases, is of use, and gives light to the present question. Sometimes the spirit of wine forces the *polypus* entirely to draw in it's lips within it's body, and at other times even to detach itself intirely from it's pedicle also.

Another way to take off the celerity of this motion, is to remove the insects into a water which furnishes them much more sparingly with food; fasting probably weakens them, and from their weakness arises an abatement in the quickness of their motions. This last expedient is of use and conveniency for the observing of this motion whilst it is slower, for several days consecutively. And afterwards upon returning the *polypi* into water stocked with food for them, the motion will soon be restored to it's former briskness.

I remarked also the last winter, that cold deadened the motion of the clustering *polypi*: and these animals in all probability are less voracious, and eat less in winter than they do in summer.

When the motion in the clustering *polypi* has been retarded, either by fasting or by the cold, they become whiter or of a paler colour than before, they also then cease to multiply.

I shall not here enter into the detail of the several observations I have made, on the feeding of these clustering *polypi*, and on the relation I have found between that and their generation; as these are particulars more properly belonging to a regular and distinct account of their natural history. But what I now propose, is to describe, in a few words, the manner in which the clusters are formed of a certain species of *polypi*, which multiply in the main like those represented in *Fig. 3, 4 and 5.* which differ chiefly from them in the form of their clusters.

My chief end in describing here this species of *polypi*, is to enable myself afterwards, to convey, by comparison, a distinct idea of a difference well deserving attention, that is to be observed between the manner of multiplying these, and that of another species of clustering *polypi*, which I happened to discover the last year.

The reader will please to recollect what I have said in the preceding paper, concerning the general manner in which clustering *polypi* are multiplied. These little animals are nearly of a bell-like form. Their anterior extremity, in which is their mouth, and which may be looked upon as their head, is that which is hollowed inwards, and resembles the open end of the bell. Their other extremity terminates in a point, and to this point is fixed a stalk or pedicle.

The *polypus*, when it is ready to divide, first draws in it's lips into the body. It then by degrees puts on a round form, and presently after the little spherical body so formed, divides itself into two other like spherical bodies. These last in a few moments again insensibly open, they then lose their spherical form, and put on that of a bell, or of a *polypus* as perfect and as compleat, as that by the division of which it was formed. This is the manner in which several species which I have observed of clustering *polypi* are multiplied: the whole operation is performed by that sort, of which I have spoken in my former paper, in $\frac{3}{4}$ of an hour, or an hour by those I am now going to speak of.

The *polypi* of this sort are lesser and whiter than those others, which are represented greatly magnified in the above-mentioned figures. The

cluster which they form rests upon a stem easy to be remarked: this stem is fixed to some other body at it's lower extremity, and from it's other arise branches, making obtuse angles with the stem itself; other branches again set out from these in different places, and from these last other new ones, and so on. At the extremity of each branch may be seen a *polypus*: and as all these branches are not of an equal length, so neither is every *polypus*, as in the other species, at the top of the cluster, or at an equal distance from the base of the stem, but on the contrary, there are here *polypi* to be discovered at all heights in the cluster. The assemblage of all these branches forms, together with the *polypi* at their extremities, a very pretty cluster or groupe, much resembling a tuft or a garland of flowers.

Fig. 8.

The stem, which carries all the cluster, and every branch in it, is capable of a remarkable sort of motion. Each will contract suddenly when it is touched, when the glass containing the cluster is moved, and even sometimes when no reason is to be perceived for their so contracting (Fig. 8. a). The stem and the branches contract and shorten, by disposing themselves into spirals, all whose rings nearly touch each other. Every branch is by itself capable of contraction, independently of the rest: tho' it but rarely happens that any one branch does contract itself quite alone, for commonly in the action of contracting it happens to touch some other branch, and then that other immediately contracts with it. When the main stem, which bears the whole cluster contracts itself, then all the branches of the cluster contract together also; and the whole becomes intirely closed. A moment after, the branches and the stem again extend themselves, and the whole cluster thereby recovers it's ordinary figure. But when the cluster is considerably advanced, the stem then ceases to contract itself any more.

I shall now attempt to describe the manner in which this cluster forms itself.

A single *polypus* detaching from the cluster, swims about in the water till it meets with some proper body to fix itself upon. It then has a pedicle but which is not longer than the *polypus* itself. In the space of 24 hours this stem becomes 8 or 9 times as long as it was at the first: and it is this pedicle which is to become the main stem of the new cluster. About a day after the *polypus* has been thus fixed, it divides itself into two. Ten or 12 hours after, these two *polypi* again divide themselves each into two more: they soon after put out branches, and thus retire to a greater distance from each other. It is now necessary to take notice, that when two of these *polypi* are thus formed by the division of one, the one is ordinarily much larger than the other: this larger one remains at the extremity of the branch where it was, but which branch lengthens itself more, whilst the other puts out a new branch which seems to proceed from the first. The larger of these *polypi* again divides itself generally before the other; and all I have been describing is reiterated several times. Thus a principal branch is formed, pro-
vided

vided with several lateral ones. These lateral branches become principal, with regard to those which in their turn seem to spring from them, when the *polypi* at their extremities come to divide. All the *polypi* of a cluster do not detach themselves from it at the same time: those which are nearest to the origin of the branches usually detach themselves first. And every *polypus* so detached, goes and fixes itself elsewhere, every one thus becoming at last, if not prevented, the principal of a new cluster.

I have often kept *polypi* of this sort, in glasses of the size of that which is represented in *Fig. 6*. And the first cluster I had placed in it to observe it's growth and progress, continued still well provided with *polypi*, when there were already numbers of other clusters formed in the same glass, all which owed their being to those that had detached themselves from the first cluster. I have seen sometimes, portions of the peacock's feather in the water, entirely covered with these clusters: and I was well assured that all these clusters came from the first I had lodged in the glass. Nay I have even carried my experiments so far as to be well assured, that every *polypus* of a cluster, as soon as detached and fixed elsewhere, became the principal of a new cluster. I mention this fact particularly, because I shall make some use of it hereafter, when I come to take notice of a difference, between this species of *polypi* I am now treating of, and another species I shall have occasion to say somewhat about by and by.

When a cluster is already in good part stripped of it's *polypi*, the branches are no longer able to contract with the same quickness and readiness as before. When there remain but a very few *polypi*, none but those branches to which *polypi* are still fixed continue to exert this power; which *they* also lose as soon as they are stripped of their few remaining *polypi*, after which they shew no further capacity of moving. From all which particulars it seems to result, that this motion in the stem and in the branches of a cluster, is entirely derived from the *polypi*, which are fixed upon the branches. Notwithstanding which it must be acknowledged, that an observer, attending to the appearance only of this motion, can hardly help persuading himself at the first, that they are the branches, which draw and give motion to the *polypi*. The resemblance and the analogy, which the figure of a cluster of *polypi* bears to the figure of a plant, would induce any observer, for some time to imagine, that the *polypi* which he sees fixed to the branches of the cluster, do really proceed and spring from those branches, in the same manner as the leaves, the flowers, and the fruits of a vegetable, spring from the branches of the same. The contrary of all this however is true. The branches, composing the clusters of the *polypi*, spring from the *polypi* which are at their extremities. These *polypi*, which at the first appear to be the fruits of the clusters, may more properly be considered as their roots: and of the truth of this any one may easily satisfy himself, who will be at the trouble of examining regularly, and for some

continu-

continuance, the whole progress of a cluster of these *polypi*. What further proves that these branches do really spring from the *polypi*, and that they derive their nourishment from the same, is, that the branches constantly cease to grow, whenever the *polypi* at their extremities are detached from them, either naturally or by any accident.

The *polypi* of another species I am now going to speak about, form also a groupe resembling a cluster, or more properly an open flower. This flower or cluster is supported by a very distinct stem, which is by it's lower extremity fixed to some of the aquatic plants or extraneous bodies that are found in the water. From the other extremity of this stem set out 8 or 9 branches, quite differently disposed from those of that species of *polypi* I have been last describing. These branches are perfectly alike, but it may be noted, that what I here call by the name of a branch, is indeed the assemblage of several other lesser branches, whose collective form much resembles that of a leaf. Every one of these assemblages is composed of one principal branch or nerve, which makes with the main stem of the cluster an angle somewhat greater than a right one. From either side of this principal nerve others again set out, and these lateral ones are the less extended in length, the nearer their origin is to the extremity of their principal branch. There is a *polypus* at the extremity of this principal branch, and another at the extremity of every one of the lateral twigs. There are others also on both sides of those lateral twigs, at different distances from their extremities, and these are more in number or fewer, in some proportion to the length of the twig itself. These *polypi* are all exceedingly small, and of a bell-like figure, and they discover about their openings a quick motion, very difficult to be seen with any distinctness.

Fig. 9.

There may also be observed in several places, upon the branches of these clusters of *polypi* (Fig. 9.) certain round bodies, which I at the first took for insects preying upon the *polypi*, because I was acquainted with some such, nearly of that shape and size: but I shall presently give an account of what those round bodies really are.

Every cluster has, as I have said, 8 or 9 of these branches or leaves such as I have just described. They do not all of them set out from the same point; but the points from whence they do set out are not far asunder: each of these leaves is a little bent inwards, and they all form together a sort of a shallow cup. If the eye is placed right over the basis of this cup, the appearance of the whole 8 or 9 branches is like that of a star with so many rays proceeding from the same center.

When the cluster is touched, and even frequently without it, all the branches fold together inwards, and then constitute a small round mass. The stem, which carries all the cluster, contracts also at the same time, folding itself up like a workman's measuring rule, that consists of 3 or 4 different joints.

I saw for the first time the *polypi* I have now been describing, on the 30th of May 1746. They were upon a water-plant, which I had taken from a ditch, and disposed in one of my large glasses. They imme-
ately

ately struck me by their beauty, and I could not help being curious to know, in what manner such clusters were formed. The relation they bore to the species first above described, and to some other species which I had before observed, gave me reason to believe that the cluster must have sprung from a single *polypus*, by the means of several successive divisions. I was not however contented with judging of them from analogy only; I was desirous to be actually an eye-witness of their operations; and the observations which I therefore made upon them, discovered to me a new fact, which I should never have suspected, and which I could never have come to the knowledge of, if I had contented myself with the judgment I made of them from analogy only.

I supposed, when I began to observe, that every cluster in question came from a single small *polypus*, like to those with which the clusters were so plentifully provided. I therefore began by endeavouring to get one of these *polypi* single, and fixed upon such a body as I could well dispose in my glass, so as to keep it within the reach of a magnifier of a short *focus*; and I pursued for this purpose my ordinary method.

I took some clusters of these *polypi* well advanced, I put them apart in a glass filled with proper water to afford them sustenance; I put also into the same glass a slip of water horsetail, after I had carefully examined it, and so assured myself that there was no *polypus* upon it. I expected that some *polypi* would soon detach themselves from the clusters, and that some of those *polypi* would fix upon the horsetail, whereby I should be enabled to set them apart, and to observe in other glasses the progress of the clusters, which would, as I made no doubt, be soon produced from them. It was on the 30th of *May*, that I set the clusters apart in the glass; on the 31st I could discover nothing new, and on the 1st of *June* I had no opportunity of observing; but on the 2d in the morning I found against the sides of the glass several small clusters of *polypi*, of the species I am now treating of. I was surprized to find them so far advanced, for they could not have begun at the soonest before ten at night, on the 30th of *May*. I saw on the 2d of *June* in the afternoon upon the slip of the horsetail, which I had placed in the same glass with the clusters of the *polypi*, a small body, which, as I had all reason to believe was newly fixed upon it. I then took out the slip of the horsetail, and I lodged it with the small body that was upon it in another glass; after which I examined it with my microscope, by the help of the *apparatus* first above described.

I then found that this body was much larger than any of the *polypi* of the present sort, and of a figure very different from them (*Fig. 10.*) *Fig. 10.* This made me suppose that this body was not of the species of the *polypi* now before us, and that it was not from any thing of this sort that I was to expect the production of a cluster of this species of *polypi*. I resolved however to continue my observations upon this minute body; which was oblong, and had a pedicle 3 or 4 times longer than itself.

It was on June 2. at 5 in the evening that I put it apart in a glass, and at 8 $\frac{1}{2}$ the same evening, I perceived that it began to split from the top towards the bottom. When the separation was accomplished, each of the two bodies, formed by this division, was nearly of the same shape as the first (*Fig. 3.*). I then thought, judging still by analogy, that it would be some time before either of these bodies would again be ready to divide; but a very little after, I saw that they both became round, and that they disposed themselves precisely as if they were again going to separate. This novelty drew all my attention, and it again came into my mind, that this body which I had but just concluded not to contain the principle, from whence I was to expect the production of one of the clusters I was looking after, might possibly still be the very thing I was seeking for.

I now imagined that perhaps these bodies would again divide and subdivide themselves, till they should come both to the shape and to the size of the *polypi*, which I had seen upon the clusters: I however looked upon this idea but as a mere conjecture. The two little bodies did in effect divide presently after; but the 4 which resulted from this division (*Fig. 9.*) had neither yet the form nor the minuteness of the *polypi* in question. I now wanted to know whether these 4 bodies would again proceed to divide without interruption; and I saw them a little after again prepare for another division: this division was completed at 11^h 20', and at midnight the 8 bodies which were formed by this third division were again almost compleatly divided. The cluster was then composed of 16 *polypi*; and I from that moment no longer doubted, but these were clustering *polypi* of the species I have been last describing. Among these 16 *polypi*, there were some which had already the perfect form of those I had observed upon the more advanced clusters: and these were such as were nearest to the origin of the branches.

Few of these 16 *polypi* were of an equal size, those which were the most distant from the origin of the branches, were the largest, and their form also was the least like to that of a bell. I found at three in the morning on the third of June, that the number of the *polypi* in the cluster was considerably increased; they were 16 at midnight, and I could now tell 26, tho' I could only see part of the cluster, the rest of it being beyond the *focus* of the microscope: and at 7^h 30' in the morning, I counted at least 40 *polypi*, in that same part which I could see of the cluster.

In order to judge with more certainty of the progress of the multiplication of these *polypi*; I counted also those of another cluster, which was so situated as to be entirely within the reach of one of my magnifiers. This cluster began to be formed about 8 in the evening of the 2d of June; I mean that it was then, that the round body first began to split itself into two. At 11 the same night, that cluster consisted of 8 *polypi*, at 7^h 30' the next morning of 64, and before night of 110 at the

the least. So that in about 24 hours there were formed, by repeated divisions of one single round body, no fewer than 110 *polypi*.

The cluster I first spoke of continued to increase from the 2d of June 8^h 30' at night, when it first began to form itself, till the 13th, when the *polypi* began to detach themselves from it; and there remained no more upon the cluster on the 15th.

The *polypi* which are at the extremities of the principal branches are constantly the largest, they are those which divide themselves the most frequently, and one of the 2 *polypi* resulting from this division is generally larger than the other. The largest remains at the end of the principal branch, whilst the lesser serves to form a lateral branch, and is itself the principal of all the *polypi* which that lateral branch is to bear.

One can hardly now be without curiosity to know, what those round bodies really are; those sort of bulbs which contain in themselves the principle, from whence these whole clusters we are speaking of are to be produced. What gives origin to these bulbous bodies? Are they produced in the clusters by divisions and subdivisions, as the *polypi* themselves are, which in other species are themselves the principles of the clusters? In these other species, every *polypus* may become the principle of a cluster and of a groupe of *polypi*, as soon as it has detached itself from the cluster where it had it's origin. When one of these has once fixed alone any where and divided itself, it no ways differs either in shape or in size, from any of the *polypi* that were in the cluster it is now parted from, or from any of those others that will be formed in the cluster, it is by it's own future division and subdivisions to produce. But how is it with the new species we are now considering? Does every *polypus* among these, as soon as detached from the cluster, fix itself also elsewhere, and there give origin to a new cluster? Or are they only the bulbous bodies abovementioned, that have this prerogative, of being capable to produce a new colony?

These questions and doubts greatly raised my curiosity, from the time I first began to see the progress of a cluster of *polypi*, formed by the division and the subdivisions of one of these round bulbous substances: and that which now follows, is what I have been able to collect from the various observations, and from the several experiments, which I made, whilst I was endeavouring to give myself some satisfaction with relation to the same doubts and questions.

To know, whether the *polypi* which detach themselves from these clusters do each of them contain in themselves the principles of other new clusters, I took all the precautions I had taken in other cases, and such as I had found easily to succeed with the clustering *polypi* of other sorts. But all was to no effect, and I could never find that any thing was produced by these *polypi* so detached. I have therefore all reason to presume, that these *polypi* do not contain the principles of new clusters, and it seems to me the most probable, that they all perish without ever producing any thing whatsoever.

When I first began to seek for the origin of the round bulbous bodies I have been speaking of, I immediately recollected those other round bodies I had before taken notice of, and which I at the first suspected to be insects preying upon these *polypi*. I therefore again sought for them in the clusters already formed; I soon found several of them, and I perceived that they neither attacked the *polypi* nor changed their situation. I then concluded that these round bodies were really the very bulbous ones in question, and whose origin I was now seeking for: I applied myself therefore to observe several of them, and these are the facts which I then discovered.

Some days after the clusters had begun to form themselves, I saw come out, not from the extremities of the branches, but from the bodies of the branches themselves in different places, small round buds, which grew very fast, and which arrived at their greatest size in 2 or 3 days. These bodies much resembled the galls which grow on the leaves of oaks; they were placed upon the branches of the clusters, just as those galls are usually placed upon the fibres of the leaves: and these bulbous substances do really contain the principles of the clusters.

Two or three days after these bulbs have begun to form, they detach themselves from the branches out of which they sprung, and go away swimming till they can settle upon some body, which they meet withal in the water, and to which they immediately fix themselves by a short pedicle. The bulbs are then nearly round only a little flatted on the under side, the pedicles continually lengthen themselves by degrees for about 24 hours, and during the same time the bulbs also change their figure, and become nearly oval. There are in a cluster but few of these bulbs, in comparison of the great number of *polypi* that are upon the same; neither do these bulbs all come out at the same time.

It is now easy to judge of the remarkable difference there is between the two sorts of clustering *polypi* that are described in this paper.

The clusters of the first species of *polypi*, and those of several others which I have also observed, do all come from *polypi* detached from the clusters already formed. But the clusters of the *polypi* of the second species here described, do not arise from *polypi* detached from other clusters, but from round bodies or bulbs, larger than those *polypi*, and of a form very different from them.

These bulbous bodies are not formed like the *polypi*, by the division of others like themselves, but they spring from the branches of the cluster, as the flowers and the fruits of a tree spring from the branches of the same.

In diverse other species of *polypi*, there are considerable intervals of time between their divisions. In the bulbous kind, if I may call it so, the first divisions are consecutive and follow hard upon each other, nor is there any interval of time between them, until the bodies which are to divide have already acquired the shapes of *polypi*.

The clusters of the bulbous sort have an origin entirely different from those of the other sorts of clustering *polypi*. Yet do these clusters in- large, and the *polypi* upon them multiply, in the same manner as those of the other species which I am acquainted with.

As I relate facts that are new, and as I am also, if I may so speak, under the necessity of mentioning new relations and analogies, I find myself under great difficulties, to find proper terms to express those relations and analogies.

I shall not here enlarge upon the analogies which may be found, between the origin of the minute animals I have been speaking of, the origin of plants, and the production of those other animals we have been hitherto more acquainted with. We shall better be able to judge of those analogies, and to compare them together, when we shall come to know more both of plants and of animals, and when we shall have made observations upon greater numbers of them.

The new and the surprizing facts, which the study of Natural History lays before us more and more every day, are fully sufficient to convince us, that the nature both of plants and animals is as yet but very imperfectly known to us, and indeed much more imperfectly than many have been apt to imagine. All we do know is but very little, in comparison of what yet remains to be known: and this consideration should prompt us, still more assiduously and more diligently, to inquire after truth; as it should at the same time also make us exceedingly circumspect, and very cautious how we venture to make judgments upon the nature of things, or how we form to our selves general rules, from so few principles as we are at present masters of.

Fig. 6. represents the necessary *apparatus*, for observing commodiously and regularly a *clustering polypus* with the microscope. In the glass *A*, is the end of a peacock's feather *b, c, f*, bent at *c*, and whose extremities are by the spring of the feather, kept close against the sides of the glass. At one of the ends *f* of the feather one of it's beards is left on, which is long enough to fasten to it in *m* a slip of water horsetail *d l*, upon which is a *polypus*, which is by this means kept so close to the side of the glass, as to be within the reach of a magnifier of a short *focus*, such as *e*. This magnifier is screwed on to a ring whose arm *n g* has at it's extremity *g* a ball playing in a socket so as to make a joint; there are again other like joints at *b* and *i*, and by the help of these the magnifier may be moved every way, and be conveniently brought near to the object. The foot *i k* is stuck into the board upon which the glass is placed. The light of a window in the day-time is sufficient to observe an object so placed within the glass, either with the bare eye, or with an hand-magnifier: but if a magnifier of a short *focus* is necessary, the shutters must be closed, and a wax light must be placed behind the glass, at such a height as to have it's light fall directly upon the object; and a magnifier so placed

*Explanation
of the figures
referred to,
in the fore-
going paper.*

placed may remain if there is occasion for several days in the same posture without any inconvenience.

Fig. 7. exhibits a cluster of *polypi*, of the first of the two species described in this paper, and which is here considerably magnified.

Fig. 8. shews another cluster of *polypi* of the same sort; the number of the *polypi* here shewn is but small, because the cluster was drawn as it appeared within 2 or 3 days after it had first begun to form itself. One of the branches of this cluster is partly contracted, and they may be seen in this situation, when a branch after contracting itself is again expanding to it's ordinary state. This cluster is yet considerably more magnified than that exhibited in *Fig. 7.*

Fig. 9. represents one branch of a cluster of *polypi* of the second species described in this paper. There may be seen upon this branch, besides the *polypi* which are of a bell-like form, some of those round bodies from which the clusters of this kind of *polypi* do first spring; and which remarkably distinguish it from many other species.

Fig. 10. represents one of these round or globular bodies, after it has parted itself from the cluster, has fixed itself to some other body, and after that the globule itself and it's pedicle have begun to lengthen. It was in this condition *June 2. at 5 p. m.*

Fig. 11. exhibits the 2 bodies, that were formed by the parting of that represented in *Fig. 10.* This parting began at 8^h 30', and was completed at 9 the same evening.

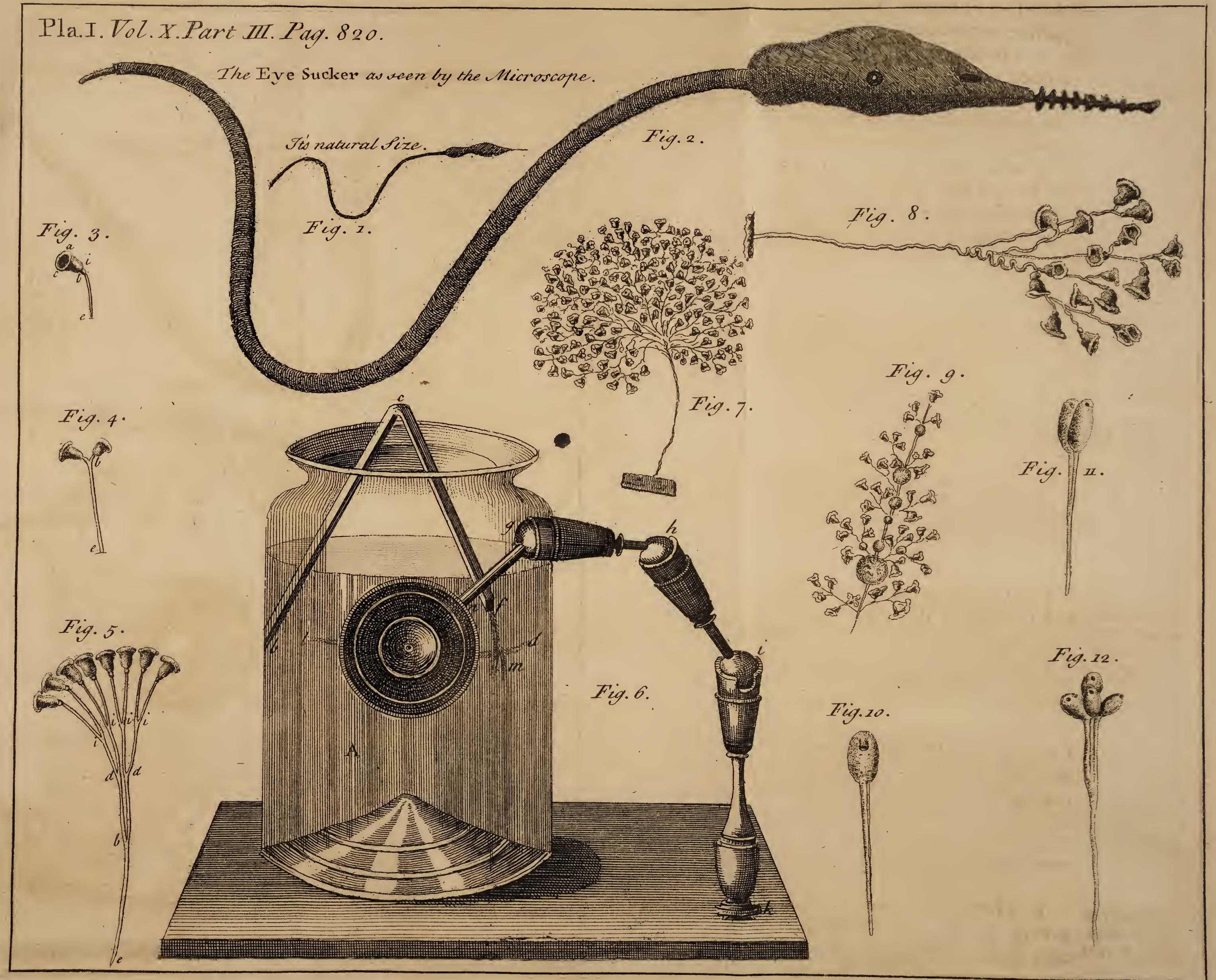
Fig. 12. represents the four bodies, which were formed from the 2 represented in *Fig. 11.* and these 4 bodies were also formed before 10^h.

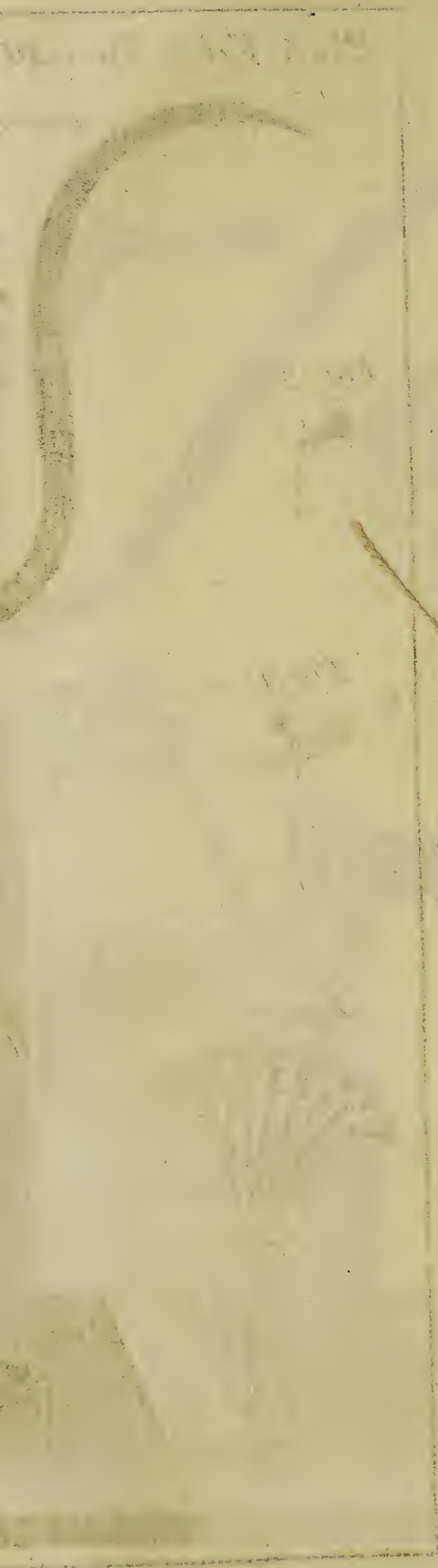
*A supposition
how the white
matter is pro-
duced, which
floats about in
the air in au-
tumn; in a
letter from
Mr W. Ar-
deron to Mr
Baker. N^o.
482. p. 428.
Jan. & Feb.
1747. Read
Feb. 26.
1746-7.*

IV. Having lately a large spider in my hand, by chance I let it fall, and it hung by it's thread, as they very commonly do. On holding my hand very still it readily ascended up it again; and thus, by giving it a shake, and then holding my hand still, the spider ascended and descended a great many times. I thought, at first, it had spun a new thread at every descent, and was desirous to have measured how long an one I could cause it thus to spin; but, upon a stricter examination, I very plainly perceived, that whenever it ascended, it wound it's thread with it's feet into a sort of coil, and when it descended only unravelled it out again. As these coils of thread are exactly like those floating in the air towards the end of summer, I think it is not improbable those are made in the same manner, when spiders have a mind to direct their course in the same direction their threads lie.

*A letter from
Mr H. Baker,
F. R. S. to the
Pres. concern-
ing the grubs
destroying the
grass in Nor*

V. Having seen some letters lately sent from *Norfolk* and *Suffolk*, giving an account, that prodigious numbers of what one letter calls *grubs*, and another large *maggots*, full as thick and almost as long as a man's little finger, are dispersed over the fields, and do abundance of mischief in those counties, I immediately imagined (tho' the accounts were very imperfect, being sent by people wholly ignorant of





of Natural History) that they must be the *aureliæ* or *chrysalides* of some folk. N^o.
 species of beetle: and desiring to get what farther information I could 484 p. 576.
 concerning them, I wrote with that intent to Mr *Arderon*, whose an- Oct. &c. 1747.
 swer (with some additions of my own) I shall beg leave to lay before Read Nov. 5.
 you, after first intreating your patience to read a description of these 1747.
 insects, published in the *London Evening Post* of *October* the 29th, as it
 is the same in substance with the private letters that put me on enquiring
 after them.

“ I forgot to tell you in my last of the grubs that are in many parts *Extract of a*
 “ of our country. They attack the corn-fields sometimes, and spoil all *letter from*
 “ the crops, but haunt chiefly the richest meadows, where they work *Norfolk, to*
 “ between the turf and the soil, eating the roots of the grass to that *a friend in*
 “ degree, that the turf rises and rolls up, with almost as much ease as *London.*
 “ if it was cut with a turving-spade; and underneath the soil is turned
 “ to a soft mould, like a bed in a garden, for about an inch deep; in
 “ which lie the grubs, in a curved posture, upon their backs, all and
 “ every one of them with only the tips of their two ends in sight, the
 “ rest of their bodies buried in the mould. They are in general about
 “ 1 $\frac{1}{2}$ inch long, and as big as the stem of a tobacco-pipe, near the
 “ bowl; they have red heads, white shining bodies, a little hairy on
 “ the back, and the rump end is ready to burst with a dirty looking
 “ stuff, easily seen through the transparent skin; they have 6 hairy
 “ legs, 3 on each side, all near the head, two *forceps*, or jaws, like a
 “ hornet, with which they cut asunder the roots of the grass, and de-
 “ stroy whole meadows, without any remedy yet found out to destroy
 “ them; they have no eyes, as can be seen. Whether they are in
 “ their ultimate state, or are to be flies, I know not, for we could find
 “ no *aurelia*. The first news we heard of them was about 2 years ago,
 “ by a Gentleman who lives near *Norwich*, and then were supposed
 “ to be new comers. This summer they have been much in *High*
 “ *Suffolk*, to the Farmers and Graziers great loss; and we now hear
 “ they are likewise in some part of *Essex*; they are often discovered by
 “ hogs, who, as I am informed, are greedy of them at first, but ha-
 “ ving once had their bellies full, never care for them after.”

Of these grubs Mr *Arderon* gives the following information.—They
 are, says he, a species of insects but too common about *Norwich*, and,
 to my own knowledge, have been more or less numerous in this county
 for these 20 years past. They are the *erucæ* of the *scarabæus arboreus*
vulgaris major of Mr *Ray*, that is the tree-beetle, or blind beetle, vul-
 garly in *Norfolk* called the *dor*.

In different parts of *England* they are called the brown tree beetle,
 the blind beetle, the chafer, the cock-chafer, the jack-horner, the jef-
 fry-cock, the may-bug, and the dor. By the *Dutch* they are named
Baum-kaefer, *Roub-kaefer*, *Koren Worm*, or corn-worm, because they
 destroy the roots of corn; and in *Zealand*, *Molenaers* or *Millers*, as
Goedartius says, c. 78. because they bite the leaves of several sorts of
 trees

trees into particles as small as if they were ground. In *England* I have likewise heard them called *millers*; but supposed it to be from a white mealy powder wherewith their wings are covered. The *French* call them *banetons*.

This insect has two pair of wings, one filmy, and the other scaly: The first pair fold together under the latter, and remain quite hid, unless when spread out for flight. The *elytra* or case wings are of a reddish light-brown colour, and seem sprinkled with a white powder that may easily be wiped off; the legs and pointed tail are whitish, the rest of the body brown, except at each joint on the sides of the belly, where there is an indented line of white. The circles round the eyes are yellowish, and so are the *antennæ*, which are short, and terminated by lamellated spreading tufts, capable of being opened more or less.

It is probable the females make holes in the ground with their sharp tails, and there deposit their young: but whether at first they are small *erucæ*, or eggs from whence such *erucæ* are hatched, I do not pretend to say: 'tis certain however, that these *erucæ* are extremely mischievous, by devouring the roots of almost every thing where they come, and in some grounds they are to be found in such numbers as is scarcely credible.

I have seen, says Mr *Arderon*, whole closes of fine flourishing grass, in summer-time, become withered, dry, and as brittle as hay in a few weeks, by this vermin's eating off the roots; in doing which they are so dextrous, that many yards of this withered grass might be rolled up in one piece, all the fibres that fastened it to the ground being gnawed away.

Closes of turnips often undergo the same fate from these devouring insects, which one would think designing to do as much mischief as possible (could we suppose them capable of any design); for when one of them fixes upon a turnip, he eats only the middle small root, which soon causes it to wither and die, and then moves on to the next. In like manner they destroy the roots of wheat, rye, &c. and almost every other useful vegetable that happens in their way. And what makes this pest the more deplorable, is the long time of their continuance in their *eruca*, or most mischievous state, which, according to *Goedartius*, is 4 years at least: but *Mouffet* writes, that in *Normandy* they are observed to be most numerous every third year, which is therefore called *L'an des banetons*. And it is not improbable, that in the open fields where they are well fed, they may come to their perfect state a year sooner than those did which *Goedartius* almost starved in glass jars.

Mr *Arderon* says, he has frequently been told by people of credit and observation, that neither the severest frosts of our climate, nor the being immersed in water, will destroy these *erucæ*; some having been exposed for many days to the keenest frosts, and others covered with water for as long a time, which notwithstanding were found to revive, and become as vigorous as ever.

Crows and hogs devour these *erucæ* greedily ; but their numbers are too great to be much diminished thereby. The most effectual way, tho' very laborious, is to beat them off the trees in the day-time with long poles, and then sweep them together and burn them. On a farm at *Heatbal*, near 5 miles S. W. from *Norwich*, of 80 *l. per Annum*, belonging to *St Helen's* hospital in this city, in the occupation of *Mr James Ebdin*, these insects were so numerous last year, that the Farmer and his servants affirmed they gathered 80 bushels of them, the *erucæ* of which had so spoiled the produce of his farm, that the court of this city, in compassion of the poor man's misfortune, allowed him 25 *l.* The order for which I send you a copy of, dated *Dec. 5. 1746.*

In the day-time few of the beetles fly about, but conceal themselves under the leaves of oaks, sycamores, limes, &c. where they seem asleep till near sun-set, when they take wing and fly about the hedges, as thick as swarms of bees ; at which time they frequently dash themselves against people's faces with great violence, and by their so doing occasioned the common proverb, *As blind as a beetle.*

Mouffet tells us, it is recorded, that on the 24th of *February*, in the year 1574. there fell such a multitude of these insects into the river *Severn*, that they stopped and clogged the wheels of the water-mills : as to which I must take notice, that their coming so early in the year was no less extraordinary than their multitudes ; for the larger species seldom appear till *May* ; and a smaller sort, which come out in *July* and *August*, are seldom seen after the evenings grow cold.

We are told in the *Transactions* of the *Dublin Society*, that the country people in one part of that kingdom suffered so greatly by the devastation made by these insects, that they set fire to a wood some miles in length, which parted two adjacent counties, to prevent their dispersing themselves any farther that way *.

VI. The

* *Mr Arderon*, in a subsequent letter to *Mr Baker*, writes, that *Mr Ebdin* solemnly declared, before the Committee of *St Helen's* hospital, that the damage done to him that year amounted to 100 *l.* and upwards. Three Gentlemen, appointed to inspect into the truth of his complaint, visited the farm in the harvest-time, when, amongst other things, they found those ears of wheat, which had part of their roots eaten off, to look pale and wan, and when rubbed, to afford nothing but small withered corn, not fit for any use, and the straw came up with the least touch ; and that these *eruca's* do most damage where the soil is richest.

Tho' many burn these flies, when they have beat them off the trees, *Mr Ebdin's* method was to spread cloths under the trees, where he saw them hanging thickest, and when beaten off, to wrap them up in the cloths, and beat them to pieces with wooden beetles.

Mr Arderon says, he had the above remarks from *Mr Richard Humfrey* one of the Committee who inspected the farm ; and he adds, that some ingenious persons account for the large increase of these insects from the decrease of rooks in this county, which they say greedily devour them in their grub-state ; and the decrease of rooks is owing to most of the ancient rookeries having been destroyed by the large fall of timber-trees made of late years, which has obliged the rooks to remove into other parts of the kingdom.

Nov. 21. *Mr Arderon* dug up with his cane two of these *eruca's* within a foot of one another in the *Upper Close*, near the middle of the city of *Norwich*, surrounded with houses ; they were about half grown, $1\frac{1}{2}$ inch long, and $\frac{2}{10}$ in diameter.

A letter from the R. Hon. John E. of Orrery, to M. Folkes, Esq; P. R. S. inclosing an account of the Cornel-Caterpillar; in a letter from the Rev. Mr Philip Skelton to his Lordship. N^o. 487.

VI. The great honour which I have received from the R. S. makes me very ambitious of expressing my sense of it, in some manner that may at least be a mark of my gratitude, however unworthy of their attention. Inclosed in another packet I send you a letter I have received from the Rev. Mr *Skelton*, who, at my request, has drawn up the account of a sort of caterpillars, that appeared very numerous in these parts some years ago. He has likewise pasted on the first leaf a piece of their web *. He is my neighbour here, and a gentleman of sense and learning. The letter is so full and explicit, that I need say nothing on the subject: unless hereafter you are desirous to know further particulars.

p. 211. Apr. &c. 1748. Read April 21. 1747.

The Rev Mr Skelton's letter to the E. of Orrery, dated Monaghan, Mar. 29. 1748.

Fig. 13.

In the beginning of *May* 1737, the warmest season that any body now alive remembers to have felt, the cornel-trees, of which we have a good number about this place, appeared almost covered with small caterpillars of the size and shape in *Fig. 13.* and in general of a dusky green, resembling in colour the bark of the tree, although a few, considerably larger than the rest, were yellow. These worms were employed partly in feeding on the leaves of the cornel, which was their only nourishment, and partly in crawling (with a very swift motion for a worm) over the bark of the tree. As they crawled, they left each a fine thread, scarcely visible to the naked eye, sticking to the bark. These threads, being almost infinitely multiplied by the inconceivable number of worms employed in the work, formed the web, in which the threads are not interwoven, but cohere by some roughness or glutinous quality.

By the end of *May* there was not a leaf to be seen on any of the cornels, excepting a few, reserved for a very curious purpose, which I shall have occasion to mention presently. But the worms, in the room of the green cloathing they robbed those trees of, gave them one of white, so entire, that it covered the whole bark, from the ground to the points of the slenderest twigs, and of so pure and glossy a colour, that the whole tree shewed in the sun as if it were cased in burnished silver. The web was so strong, that if one disengaged it from the tree, near the root, one might have stripped it from the trunk, the branches, and the twigs, at one pull. As soon as the worms had covered all the cornel-trees, they removed from thence, and covered all the ash, beech, lime, crab-trees, and even weeds, that grew near them, with the same, but a thinner, kind of workmanship.

Many of them crawled along the ground, and over every thing in the way, still leaving a thread behind, and dispatching a part of their business as they went to a more convenient surface to finish the rest on. But I really imagined some of them took an easier and more ingenious way. I found many of them hanging by their own threads from the most

* To be seen in the *Museum* of the *Royal Society*.

most extended branches of the tree. While they were in this situation, a gentle puff of wind might, by exciting a pendulous motion, waft them to the next tree. This seems to be the method, by which those very minute spiders, whose threads are made visible by the moisture adhering to them in a foggy morning, transport themselves from one bush to another, although destitute of wings, sometimes across narrow paths, and even rivulets.

As the worms, neither while they were working it, nor afterwards, made any use of the web thus left on the bark of the trees, I take it for granted, they wrought for no other purpose than to rid themselves of that glutinous mass, out of which it was spun, and which, nature producing it that season in greater abundance than was necessary for the wrapping and stowing the worm in it's *nympha* state, prompted the creature to work off the redundancy the best way it could. The method it made use of for this purpose was very well judged. It fastened it's thread to some little eminence on the bark; and chusing, for the greater convenience of crawling, that even surface, kept continually in a brisk motion, till the troublesome superfluity of it's burthen was discharged. I can but guess at it's reason for removing from it's own native tree, to spin abroad upon the neighbouring ones. Perhaps it found the web too bright for it's eyes, or the threads, already layed, might have stuck to it's feet; for your Lordship may observe that the web is very apt to stick to the fingers, when it is touched.

About the beginning of *June* the worms retired to rest. Their manner of preparing for, and executing this, was very ingenious and curious. Some of them chose the under sides of the branches, just where they spring from the trunk, that they might be the better defended from the water, which in a shower, flowing down the bark of the tree, is parted by the branches, and sent off on each side.

Here they drew their threads across the angle, made by the trunk and branch, and crossing those again with other threads in a great variety of directions, they afterwards formed a strong tegument on the outside. Within this they placed themselves lengthways among the threads, and rolling their bodies round, spun themselves into little hammocks of their own web, while in the mean time they shrunk into half their former length. Those hammocks, being suspended by the transverse threads, did not press each other in the least. That they might take up the less room, they lay parallel to one another, and in the most convenient order imaginable.

Others, still more ingenious than these, fastened their threads to the edges of certain leaves, which, no doubt of it, they had saved from their stomachs for this very purpose; and with that slender cordage pulling in the extremities of the leaves, drew them into a kind of purse, in the inside of which they formed the same kind of work, and laid themselves up in the same manner as above. By this method they saved themselves a labour, which the rest were at the expence of; for the leaf served them

very well for an outward defence against the weather, and a place to fix their transverse threads to. It is probable they laid themselves up in great numbers together, not only because many were necessary to the work of providing a common covering, but also to keep one another warm, while nature was preparing for the great change, and also to confine some subtil vapour, issuing from their bodies, which might have been conducive to their reviviscence, and which had been easily dissipated, had they not lain close, and caught it from one another.

Between the worm, thus laid up, and the hammock, in which it was enclosed, a tough and pliant shell, of a dark-brown colour, was found. This I take to have been formed by the perspiration, or rather by some glutinous stuff, forced through the pores of the insect, while it was contracting itself, which being stopped by the close texture of the hammock, consolidated, and formed an interior covering for this delicate creature. As the worms themselves were of a pretty dark colour, this superficial tincture seems to have been in a great measure purged off into the shell.

For after the worms had continued in this state during the whole month of *June*, whether they gnawed their way through the ends of their shells and hammocks, or that exit was prepared for them by some corrosive matter ouzing from their mouths, I know not, but they came out almost all in the space of one morning, the most beautiful fly or moth that my eyes ever beheld. It's shape was extremely elegant; it's head, upper wings, body, legs, and *antennæ*, were of the purest white, and glittered as if they were frosted with some shining kind of substance. I rubbed some of this off, and upon viewing it thro' an ordinary microscope, it appeared like the points of very minute feathers, or like small cones of polished silver. The upper wings were regularly studded with small, round, black spots, and extended themselves from it's head somewhat beyond it's tail. The under wings, which were a little shorter, were of a dusky colour, and prettily fringed at the extremities.

This beautiful and surprizing work of nature seemed, after it's resurrection, to have no dependence on material food. The cornel had recovered a new set of leaves by the time the fly appeared, but it never touched them; and those that came out in my room, lived as long there, as the rest which enjoyed the open air, and the tree on which they were bred. If they did feed, it must have been on some other adventurer of the air, too minute to be visible to our eyes. Those that were confined to my room, discharged a small drop of brown liquor, in which I suppose their eggs were contained; but as they were not deposited in a proper receptacle, they did not produce worms the next year. As the tree is the peculiar habitation of the worm, and supplies it with it's only food, so it is certainly the only nurse of it's egg. It is likely the eggs were either inserted into the small crevices of the bark, or discharged into the little apertures, where buds are to spring the following season. In this situation, they might be most conveniently nourished

rished by the return of that genial juice, or spirit, with which the cornel is naturally fitted to cherish and raise them into life. The flies seemed to be of a most delicate constitution in respect to heat and cold. The former they could bear with difficulty; the latter, not at all. Hardly any of them survived the first of *August*. They loved rest, and did not care to flutter much about. While they were yet in their *nympha* state, I brought great lumps of them to my room, and those, which happened to be bruised in pulling them from the trees, produced flies, distorted either in the wings or other parts; but this distortion generally wore off, in a little time, and the pretty creature recovered it's own natural symmetry of shape.

In the beginning of *May* 1738, they began to work again in prodigious numbers, and having covered some trees, were stopped, and most of them destroyed by the foul weather that followed. Their web also was smutted and discoloured. I send your Lordship a piece of each year's produce. The whitest is that of 1737, the other of 1738.

In 1739 they appeared in small numbers, and much shrunk in their size, and wrought only sufficient covering for themselves.

They appeared again in the year 1740; but it was plain the great frost had destroyed most of their eggs, and checked the growth of those that escaped; for there were very few of them to be seen, and 12 of them were not larger than one in *May* 1737.

Every year produces more or less of them, with some small variation, as to the number and size.

The place where our cornel-trees stand, is surrounded with steep hills, and closely sheltered with a very thick plantation. This was probably no inconsiderable help to the prodigious increase of this puny reptile. I verily believe both an unusual warmth of air, and a deep shade, were equally necessary to it; for I observed, that those cornels, which stood more exposed to the cool air and the sun, abounded less with worms than the rest. I have been scrupulously exact in relating the above particulars, which I did not trust to my memory, but reduced to writing immediately after I had finished my observations.

This curious *phenomenon* naturally leads one to enquire, how these creatures came to breed on the cornel-trees, and what occasioned the prodigious increase of them at that time. Here fact gives us up to conjectures. I hope however that mine will not seem altogether unsatisfactory, but rather help to clear up those difficulties, and at the same time carry our eyes a little farther into nature, than merely to what concerns this species of insects.

There is not an animal, nor a vegetable, that may not be considered as a little world, in respect to the habitation and nourishment it affords to certain insects peculiar to itself. The scheme of life begins in vegetation; and wherever on the earth, or in the water, nature is able to produce vegetables, she always obliges them to pay for their elemental nourishment to certain insects, animals, or fishes, which she billets on them.

them. These again are forced to refund to others, to diet and lodge, each of them, a set of living creatures, assigned to them by the universal scheme of nature.

This traffick of life, this just community in nature, which suffers nothing to subsist merely for itself, is found not only every-where on the face of the earth, but also in all lakes, pools, rivers, and in the ocean. By microscopes we discover a prodigious variety of little creatures, all feeding either on the floating vegetables, which that element produces in a state of stagnation, or on one another. As to the sea in particular, we know only what happens about the shores, where we see vegetables of various kinds, on which a like variety of insects are bred and nourished. These, together with a prodigious number of others, bred in the mud, become the prey of the smaller kind of fishes, and they again of the greater. That this scheme of nature, in supporting life by death, found every-where else, dives into the depths of the ocean, may appear probable from the wise frugality of nature, which hath an useful end in every thing, and besides rejoices in filling the world with life and motion; and also from the wonderful kinds of fishes, which are now-and-then washed up by violent storms from the deep waters, or happen to pursue their prey, from the low lands of the ocean, to the higher grounds at the shores.

Franciscus Redi, in his curious treatise concerning the generation of insects, hath not only refuted the notion of equivocal generation, but also hath shewn us, that each animal and vegetable hath it's own peculiar insects to maintain; and *Eleazar Albin*, in his collection of various caterpillars, and the butterflies, into which they are transformed, hath given us a beautiful demonstration, from above an hundred instances, that each species hath it's own proper plant, to which it is by nature peculiarly adapted, and on which only it can feed, or live for any considerable time.

Now the cornel is the plant, on which alone the worms, we have been speaking of, can be propagated and fed. The specific qualities, with which the juices of this tree are impregnated, fit it for the propagation and support of this it's native insect. If these peculiar and distinguishing qualities reside, as the Chemists say, in the essential oil of the plant, it will follow, that this, as well as other insects, subsisting on vegetables, are by some means or other qualified to extract, in a nicer manner than any Chemist can do, the essential oil of their respective plants, nothing else therein being of a nature sufficiently peculiar either to assist the propagation, or supply the nourishment, of the insect.

As to the difficulty, how this plant came to receive the eggs of this fly, it is as great in respect to the propagation of any other insect on it's peculiar plant. The flies of every plant have continual access to those plants, and no doubt are prompted by the sight, smell, or other qualities of their native vegetable, which are congenial to them, to propagate their kind upon them. As this act is probably attended with
some

some degree of pleasure, it keeps them continually busy in the work of impregnating their proper plant. Hence it comes, that before the younger plants are removed to a distance from those that are more fully grown, they receive sufficient colonies from others, already peopled, which they extend again to their succours, the flies each year impregnating all the plants within their reach. Whether the thing happens in this manner, or that the eggs of insects so small are minute enough to be carried through the air, and so dispersed every-where, it is nevertheless a fact, that no vegetable is found without it's insects, tho' propagated by the seed.

As to the extraordinary increase of this insect in *May* 1737, the succession of 7 or 8 mild winters, which preceded that season, might, by preserving their eggs, give occasion thereto. As they are one of the earliest kinds, the excessively warm *May* that year so effectually hatched their eggs, that they all came to perfection: whereas the more ordinary worms and flies, that make a later appearance, meeting with the sharp easterly winds that happened that summer to blow during *July* and *Aug.* were in a good measure destroyed; otherwise it is possible they too might have had an extraordinary increase.

However I own this reason hath it's objections, and doth not fully satisfy me. There is scarcely a year that is not remarkable for some one kind of insects or flies, when no colourable reason can be assigned for it from any known temperament of the year, which might not as well favour a great increase of any other species. Insects, as well as fevers, are epidemical, and probably depend as much on a certain occult constitution of the air, water, or earth. Nay, it is an opinion received by some, that all pestilential disorders are nothing else than prodigious flights of invisible flies, of which each sort, according as the constitution of the year assists it, takes it's turn to multiply from worms proportionably little, bred in putrid carcases, especially after great battles, and being raised from thence into the air, are waisted not only from one body to another, but even to distant countries. *Sydenham*, and, if I mistake not, others, have observed, that the seasons immediately preceding those in which the plague raged, abounded unusually with all sorts of flies; which shews at least, that the constitution of the air doth at those times greatly favour the production of such creatures. This conjecture will seem the more probable, as the usual preservatives against infection, namely vinegar, tobacco, rue, wormwood, &c. are endued with very acrid and pungent particles, with which perhaps they sting and kill the invisible flies before they can lay their eggs, and by these means preserve us from contagion. It is certain however that there is such a constitution as we are here speaking of, in respect both to distempers and insects. But whence this proceeds, whether from the sun alone, or from the joint influence of other neighbouring planets, or the transudations of mineral vapours, or fermentations in the soil of the earth; and further, whether this sort of climacteric in the seasons be stationary
or

or casual ; I leave better Naturalists to judge. I only insist, that such a constitutional temperament there is, which, running thro' all nature, doth at certain times give more than ordinary energy to the prolific powers of such plants or animals as are of nature similar thereunto.

This plainly appears to us in plants of all kinds, even excluding the consideration of warmer or colder, of drier or moister seasons, which, it is manifest, have only their share in the casualties to which the vegetable world is liable. They frequently bear more blossoms and fruit in a bad, and less in a good season ; and what puts the matter beyond all question, that season which is favourable to one kind of vegetable is prejudicial to another ; whereas much heat and moisture together are equally indulgent to all. This, in my opinion, shews that each plant hath a specific vegetation of it's own, as well as one common to all, and that the former depends upon somewhat else than mere warmth and moisture.

The constitution therefore of the year disposes the vegetative spirit, whether residing in the air, the earth, the water, or in all, to supply sometimes these, and sometimes those vegetables, with a greater or less proportion of aliment. By these means a greater quantity of that juice, which distinguishes any one species of plants from all others, and enables it to give life and food to it's peculiar inhabitant, must necessarily be produced one year, than other ; and consequently the eggs, deposited in the cavities, or perhaps in the very perspiratory pores of it's bark, must be better cherished, and the worm more plentifully fed by the leaves, which in such a year contain a greater abundance of the specific juice, and that more perfectly elaborated.

From hence it may seem reasonable to rest in this conjecture, till somewhat more certain is found out, that the annual constitution being more indulgent to the vegetation of one plant than of another, promotes the growth and fertility of this, which is of a similar, and checks the increase of that which is of a dissimilar nature. The plants, thus differently supplied, furnish their respective insects accordingly. Hence again it comes to pass, that many species of insects, having been injured by some unknown disposition of the air or earth, seem almost extinct in one season, and swarm out again in another, as if there had been a new creation of them. One year, the wall-fruits are devoured by ear-wigs ; another, we are pestered every-where, and even in our closest chambers, with unusual multitudes of the common fly. One year the wasp predominates ; another, the gnat ; and a third, the cane-caterpillar. One year, the Farmer complains of a worm, hardly known to him before, that destroys his corn ; and the Gardener does the same another, in respect to an insect that falls greedily on his seeds, as soon as they are committed to the ground. The *African* locusts come some years into *Spain* in such swarms, that they cover the face of the earth ; and when they have devoured the whole herbage of the country, retire again to their own, and do not visit *Spain* in the like numbers for several years.

Large

Large old orchards are some years suddenly stripped of all their blossoms and leaves, by a prodigious increase of the apple-tree worm; and groves of oak have been served in the same manner by the caterpillar peculiar to that tree. This must needs give a check to the growth of the tree more than equivalent to the great increase promised at such a time by the extraordinary redundancy of the vegetative spirit.

VII. The paper lately communicated by the *Pres.* from M. Bonnet *An abstract of Geneva*, contains various experiments he has made relating to the respiration of caterpillars.

Malpighi first discovered, that those 18 openings or orifices, which are placed 9 on each side of the caterpillar, and which are called by the name of *stigmata*, serve to give respiration to this class of animals.

M. de *Reaumur* has repeated the experiments of *Malpighi*, and made several new ones upon this subject. And he has been of opinion, that these apertures served only for the inspiration of the air, which the caterpillar afterwards expired, through the whole superficies of its body. What he has written upon this subject is in the first tome of his memoirs, at the 131st and the following pages.

Mr *Bonnet* has had reason to think these caterpillars do both inspire and expire the air by their *stigmata*; and that they did not expire any of it through the pores of their body. This paper here shewn gives an account of 36 several experiments, made chiefly with design to discover this fact, whether indeed these insects did both inspire and expire the air by their *stigmata*, or only inspire it. These experiments, like M. de *Reaumur*, consist mostly in the plunging of caterpillars either into water, or some other liquor; some also they daubed or anointed over with fat and greasy substances, some quite over, and others only in some places.

Mr *Bonnet* is inclined to think, that the small bubbles of air observed all over their bodies, when they are immersed in water, do not come from the air included within them, and which they expired by the pores; but that they are formed by the air only lodged near the surface of the skin of the caterpillar, as it is about the superficies of all other bodies: he has endeavoured to contrive it so, as that no air might remain thus sticking to the skin of those insects upon which he has made these experiments. And for this purpose, before he plunged them in the water, he first washed them all over with a hair-pencil or brush; and these being afterwards immersed in the water, but very few bubbles of air have been discovered on the outside of their bodies; and fewer as it appeared than M. de *Reaumur* had found upon those, upon which he made his experiments; neither was this last of opinion that all those bubbles which he took notice of were formed by the air rushing out through the pores, but that some of them were also formed by the air sticking about the exterior part of the skin.

When a caterpillar is plunged in water, one bubble of air is almost constantly observed upon each of the *stigmata*. M. de *Reaumur* concluded,

An abstract of Mr Bonnet's Memoir concerning Caterpillars; drawn up in French by Mr A Trembley, F. R. S. here translated into English. Ibid. p. 300. Read April 27. 1748.

cluded, that the air was not expired by these *stigmata*, because he could never observe that any bubbles of air were ever driven out of these *stigmata*, as one would think there must have been, if the air was really expired by these apertures. Mr *Bonnet*, on the contrary, has some bubbles of air come out from these *stigmata*, and that has contributed to make him rather think that the air inspired was also discharged at these same orifices. But as these experiments are not decisive, he is unwilling absolutely to determine, but proposes the making more new experiments.

A caterpillar can remain several hours under water without perishing; it only falls into a state of numbness; but if again taken out of the water, it is not long before it again shews signs of life, and recovers. Mr *Bonnet* has sought by some experiments, to know, if some only of these 18 *stigmata* of a caterpillar might not be sufficient for the purposes of respiration: he has plunged some of them only partially in water, sometimes by the tail, and others by the head foremost; but always so that either 2 or more *stigmata* might be out of the water; and in these cases the caterpillar has not fallen into the torpid state above-mentioned, as it constantly did when intirely immersed. He has lifted out of the water some of the *stigmata* of caterpillars that had been quite immersed, and that were so become torpid and motionless; and these have also soon after shewn signs of life and motion. One of the caterpillars, upon which Mr *Bonnet* made experiments, lived 8 days, suspended in the water, and only exposing to the air it's posterior *stigmata*; that is to say, that only the 2 last *stigmata* were out of the water.

He during this time carefully observed his caterpillar; and he remarked, from time to time, when the insect moved itself, that little streams of bubbles came out of the anterior *stigma* on the left side. It appeared to him, by this and some other experiments, that amongst all the 18 *stigmata*, the two anterior and the two posterior ones are of a greater use for the respiration of the caterpillars than any of the others. He also found, that, upon the choaking up these *stigmata* with butter, the animal seemed to suffer much more sensibly, than when he so choaked up all the intermediate ones.

All these experiments of Mr *Bonnet*, and which are very particularly detailed in his paper, were made with great attention, patience, and sagacity. And it is to be wished that he may continue thus diligently to apply himself to the study of Natural History.

An abstract
of the Rev.
Mr Gould's
account of
English Ants;
in a letter
from the Rev.

VIII. I here send you a short abstract of an ingenious treatise on *English* ants, the perusal of which has entertained and instructed me not a little; and as the very industrious author has made more observations than any other person amongst us appears to have done, and has discovered several curious particulars not mentioned by other writers on the subject, I thought you would not be displeased to see a brief account of

of the performance, with a few remarks and emendations I have taken the liberty to make.

The book is intituled, “*An account of English Ants;*” which contains, 1. Their different species and mechanifm; 2. Their manner of government, and a description of their feveral queens: 3. The production of their eggs, and procefs of the young: 4. The inceffant labours of the workers, or common ants; with many other curiosities observable in thefe furprizing infects: by the Rev. *Wm. Gould*, A. M. of *Exeter-College, Oxon.* London: Printed for *A. Millar*, oppofite *Katharine-ftreet* in the *Strand*, MDCCXLVII. in large 12mo.

H. Miles,
D. D. F. R. S.
to Mr H. Baker, F. R. S.
N^o. 482. p.
351. Z z.
Jan. & Feb.
1747. Read
Jan. 15.
1746 7.

CHAP. I. Contains a description of ants in general, their various forts, colour, and ftructure of their parts.

Five fpecies of ants have occurred to the obfervation of our author. 1. The hill-ant, vulgarly called the horfe-ant. 2. The jet-ant. 3. The red ant. 4. The common yellow ant. 5. The fmall black ant.

Having defcribed the fize and colour of thefe, he proceeds to defcribe the ftructure and nice mechanifm of ants with great accuracy; obferving, that, befides the *viscera*, there is in the body of ants a bag of corroding fpirituous liquor, which they can eject to a confiderable diftance at pleafure. This particular has alfo been obferved by other writers.

He fays, he has met with a ligament in the red ant, which uniteth the breaft and body, confifting of two lobes fomewhat round; but in other ants there appears but one lobe, which rifes higher, and is broader, than the lobes in the red. It is this fpecies of red ants, which he has obferved to have a ftmg, of the fame contexture with that of a bee, in miniature: in other ants he has met with no ftmg; but they bite, or make a fmall incifion, with their faws, ejecting fome of the afore-mentioned corroding liquor, &c. The red ants, which are furnifhed with a ftmg, he obferves live more open, &c. and are more bold than any of the others; and therefore fuch a weapon is ferviceable to them.

The jet-ants, he informs us, have a peculiar difagreeable fmell, which he imagines may be a great prefervative to them againft an enemy; and that the fpirit which all ants eject is very ftrong, affecting at a fmall diftance in the fame manner as fpirits of hartfhorn.

CHAP. II. Treats of their colonies, cells, &c.

Here our author obferves, that though they unite in colonies, in fuch places and fituations as are moft agreeable to their different natures, &c. yet their refidence is not fo limited as to admit no variation; however 'tis worth obferving, that the feveral fpecies never fo intermix, as to affociate and breed together, though they will live near and good neighbours one to another.

Their architecture, he fays, is adjusted with remarkable curiofity and art, the whole ftructure being divided into a number and variety of cells, communicating all of them with one another by little fubterraneous

chanel, which are circular and smooth; but as for the incrustation, most *Virtuosi* have mentioned, in the apartments of ants, our ingenious author observes, that after the most careful observation he could never find any composition in their structures; the cells being formed in the mold itself, without any addition of glue, straws, &c. He acknowledges it may be otherwise in hotter climates, where sand is more apt to crumble.

Their works, as he informs us, are all carried on by the assistance of their double saws, and the hooks which are placed at the extremity of them, described by him in the preceding chapter. The process and manner of their work may easily be observed, he says, if you deposit some ants, with a lump of moist earth under a glass.

CHAP. III. Treats of their government; describes their several queens; the respect shewn them by the common ants, &c.

A colony, our author tells us, from the latter end of *Aug.* to the beginning of *June*, is usually composed of a large female, and various companies of workers. And besides these, in the latter end of *June*, all *July*, and part of *Aug.* of a number of winged ants commonly known by the name of Ant-flies. The government, he says, has been universally taken for a republic or commonwealth; and have been treated as a body consisting of males and females; the former being looked upon to be those which make their appearance with wings in the summer. But as, in the oeconomy of bees, the generality of them have no distinction of sex, but make it their whole employment to provide for the young laid them by their queen, so the same character is found to be maintained in the constitution of ants. The common ants therefore, which usually present themselves to our view, are, he says, like the common bees, of neither sex, but seem intirely destined to take care of, and educate the young, which the queen deposits in the cells.

In every perfect colony, our author says, there is at least one queen; who, in the space of 7 or 8 months, gives birth to a family, amounting, at a moderate computation, to 4 or 5000; except the red queens, who are not so prolific. The yellow ants being the most frequent, he gives a very particular and curious description of their queen; which, he tells us, is perhaps 5 times larger than any of her subjects; and that, moreover, in her front she has 3 eyes, in a triangular form, which are less than the two common ones on each side her head. I omit other particulars, as also his description of the other queens, for brevity's sake. The queen of the jets, he says, he never had the pleasure of seeing.

He has beautifully represented the obedience and respect the queen commands, in whatever apartment she condescends to be present. An universal gladness, he says, spreads itself thro' the whole cell, expressed by particular acts of joy and exultation: they have a particular way, it seems, of skipping, leaping, and standing upon their hind legs, and prancing with the others; which frolicks they make use of both to con-

gratulate

gratulate each other when they meet, and to shew their regard for the queen. Some walk gently over her, others dance around her, and all endeavour to exert their loyalty and affection. However romantic, says our author, this description may seem, it may easily be proved, by placing a queen, with her retinue, under a glass; for, in a few moments, you will be convinced of the honour they pay, and esteem they have for her.

In *Oct.* he tells us, ants and their queens begin to retire downwards; and, in the depth of winter, are to be found in the remotest apartments, incircled close with a cluster of attendants, and, as it were, benumbed.

CHAP. IV. The author gives a particular account of the time and manner in which the queens lay the eggs, &c.

And he says, he has been the more circumstantial in this point, to remove a mistake of Sir *Edmond King's*, who, not aware of there being a superior female, gave into the old opinion, that the small ants were the females, and supplied the colony with young: after a just description of the sperm or eggs, Sir *Edmond* observes, that he found that substance among the common ants; and that he gave the more credit to that opinion, because of the great care and tenderness with which they treat it. But our author does not allow this reason to be conclusive, inasmuch as the same is to be met with in the constitution of bees; adding, That having at all times of the year observed the common ants, he could never discern any alteration in their bodies but what was occasioned by food, or some accident.

The queen, he says, lays 3 different sorts of eggs, male, female, and neutral: the two first in the spring; the last in *July*, and part of *Aug.*

CHAP. V. Our author treats of the change of the eggs to vermicles, &c. and gives us an account of their surprizing continuance in that state.

The queen having furnished the eggs, he says, the common ants brood over them in little clusters, perhaps by way of incubation; and remove them to different parts of the colony, for the better advantage of moisture, and a just degree of heat and cold. The time of continuance in the egg-state is somewhat uncertain: but he says they seem to disengage themselves from the membranes that inclose the eggs in the same manner as silk-worms do.

The process of ant-vermicles, he tells us, is remarkable, and worth observation. The female eggs put on the form of worms some time in *Feb.* at farthest; the male by the latter end of *March*; the neutral by *Sept.* The first summer they grow very sparingly; the succeeding winter they seem at a stand: in the beginning of *April* of the second year they visibly augment every day; and in six weeks, or by the end of *May*, the male and female attain their greatest proportions, and are ready for another change. This long continuance of ants in a vermicular

lar state he thinks a great curiosity, hardly to be met with in any other class of insects, the female ant continuing above a year and quarter, the workers a twelvemonth, the males somewhat more.

CHAP. VI. Treats of a transmutation of ant-vermicles to nymphs or *aurelia's*, &c.

The vermicles, he says, weave in the manner of silk-worms, and in a few days infold themselves in a soft filken kind of tissue: they henceforth assume, and, whilst confined in this monument, continue the character of *aurelia's*. These are the small bodies which abound in the settlements in the summer-months, and are vulgarly reputed ant-eggs; but their largeness, and visible transmutation (as he justly observes), shew the mistake.

Our author takes notice of a remarkable variation in the *aurelia's* of the red ants. When the worms arrive at their period of transmutation, he says, they do not infold themselves in a tissue or shell, like the others, but lie motionless, and, to outward appearance, insensible; in a few days look whiter than ordinary, and in this manner gradually put on the form of ants. Thus Providence (remarks our author) is tied down to no particular laws; but can, by a surprizing variety, accomplish the same ends.

In the VIIth Chap. he proceeds to treat of the transformation of the several *aurelia's* to flies and common ants, with a description of their structure, duration, and other curiosities relating to the change. But the just progress of ants-eggs, vermicles, nymphs, &c. cannot, he says, be precisely stated; because they will not arrive at maturity under glasses, as *Swammerdam*, before him, had observed.

As soon as the ant-nymphs, surrounded with a tissue, are tending to life, he says, the workers give them air, by an aperture in the head-part of the covering; which aperture they gradually enlarge; and, after a day or two, take out the young, and expose it to the freer access of the sun-beams, which are of great force in promoting it's maturity.

Our author observes, that Philosophers have usually confounded the two different sorts of ant-flies, the large and small, looking upon them all under the character of males; tho' there be so wide and manifest a variance in the colour, size, &c. that the naked eye may easily distinguish it. On the contrary, therefore, he presumes they are of different sexes: the small ones he takes to be males, and the large females; and thinks it highly probable, that some of these females, afterwards, give birth to new colonies, and intitle themselves to the dignity of queens; there being, as he says, many strong experimental reasons to support so uncommon a curiosity; which he also recites, and answers the chief objection against it, taken from the number of these ant-flies: the principal thing of which his answer consists is, that the most obvious use of them is for the sustenance of other animals.

In the close of this chapter he annexes a few remarkable curiosities resulting from the change. The casting of their wings is an instance, he says, peculiar to the large ant-flies; these being to other insects their highest decorations; and the want of them lessens their beauty, and shortens their lives. On the reverse, a large ant-fly gains by the loss, and is afterwards promoted to a throne, and drops those external ornaments, as emblems of too much levity for a sovereign.

CHAP. VIII. Our author here treats of the incessant labours of the workers, the true method of collecting their provisions, and inquires into the truth of the opinion of laying up corn, &c. against winter, &c.

He says, The general subject of this chapter has been so largely treated of, and well illustrated, by some of the happiest favourites of *Minerva* and *Apollo*, that it is impossible to set it off with more beauty of thought, or elegance of style; but perhaps, (says he) in many circumstances they have rather shewn the Poet than the Philosopher; and rather indulged an extensive fancy, than strictness of inquiry.

I must here omit the account the author gives of the labour and industry of the common ants, which is certainly very curious, that I may avoid being tedious; observing only, in general, that the feeding the young is the most laborious exercise belonging to the working ants, and a part of their industry the most uninterrupted of any.

The juices of most sorts of fruit, insects, and honey, or any other delicious liquid, he says, are the repast which they nurture them with. These juices they extract, and first convey into their own *alvus*, and afterwards infuse into the bodies of the vermicles; which aliment, he supposes, may probably undergo some refinement in the repositories of the ants, and, being there meliorated, is properly tempered for the delicate structure of the worms.

It has been a dispute, says our author, amongst the inquisitive on this subject, whether ants have magazines of corn, and lay up a stock of provisions against winter. The generality of writers, he says, hold the affirmative; referring, in his margin, to *Solomon*, *Pliny*, *Virgil*, *Horace*, *Aldrovand*, *Swammerdam*, &c. Here I am obliged to do justice to *Swammerdam*; who, in his *Biblia Naturæ*, expressly says, that he never at any time observed them to get together any food against winter; and is of opinion, that, during the severity of the winter, they eat nothing; as is common with many insects, and some species of bees. His own words, in *Vol. I. p. 296.* are as follow: *Nèque etiam unquam observavi, quod cibaria quædam in hyemem sibi comparent: unde censeo ipsas, quousque vehementissima est hyems, nihil comedere; quemadmodum multis insectis, et apum quoque nonnullis speciebus, familiare est; quæ tempore brumali ab omni penitus cibo abstinent.*

Our author, with great deference to the writers who have held the affirmative, and with extreme decency, differs from them, offering a hand-

handsome apology for himself. He suggests, that in warmer regions they may not undergo the chill they do with us; and therefore may not pass the winter in a state of numbness. That, if this be the case, a store of food must be necessary to them, which is not to our northern ants, which live, as it were, entranced. He adds, that, upon the most impartial examination of authors, the opinion seems rather to be supported by it's antiquity, than reduced to a clear demonstration. He tells us, that, as upon the most exact and frequent examination of numerous settlements, in the winter, he could never trace out any reservoirs of corn, or other aliment; no, not in those of the hill-ants, which are the largest, and proportionably strong: so, to put this matter beyond all reasonable doubt, he had recourse to experiments; which, had the supposition been true, could not probably fail of succeeding. At the beginning of the spring, he placed, in several flower-pots, and other conveniencies, different colonies of yellow small black ants, &c. with their respective queens, attendants, and vermicles; in which position they continued summer, autumn, and winter, and carried on their operations as in other settlements, nourished their young, and brought them to perfection: from whence he concludes, that they would have laid up provisions, had it been their custom; but, upon carefully examining some of these pots, he found no appearance of magazines of corn, or any collected food: and that, upon his having frequently observed their excursions from, and return to their colonies, he could never find, that they ever returned with any wheat, corn, or any other vegetable feed; tho' they would with eagerness attack a pot of honey, or a jar of sweetmeats, &c. Many other experiments, besides these, our author made, which I forbear to give you, judging these to be satisfactory.

The most material argument in favour of ant-magazines, he thinks, is the authority of the Sacred Writings. *Solomon*, he says, has twice mentioned these extraordinary insects; and each time with an immediate reference to their sagacity in providing for the necessities of winter. For removing this difficulty, he has recourse to the former solution. The superior warmth of the climate he lived in, and, of consequence, the proportionable clemency of the seasons; whence he concludes the ants of those countries may vary from ours in this, as well as in other respects: or perhaps, adds he, it might have been a received opinion, as was the sun's motion; from whence this great prince might recommend it, as a worthy example of industry and wisdom.

If I might have leave humbly to offer my opinion, our author seems to have justified his conduct in departing from the commonly received opinion: and perhaps there is good reason to think that it has been handed down from ancient writers of reputation, and too easily received, without carefully examining into the truth of the fact; which persons might easily be led to do from a general observation of the extraordinary

nary industry of these little laborious animals in carrying things into their cells.

The most learned *Bochart*, in his *Hierozyicon*, has displayed his vast reading on this subject, as he usually does on all others; and has cited passages from *Pliny*, *Lucian*, *Ælian*, *Zoroaster*, *Origin*, *Basil*, and *Epiphanius*, *Jewish Rabbi's*, and *Arabians*, all concurring in the opinion, that ants cut off the heads of grain, to prevent their germinating: but he confesses, that the ancients *Greek* writers have made no such observation of the ants; nor any of them who lived before *Pliny*, as far as he remembers. Very probably this opinion arose from what might have been observed of these laborious insects, in cutting asunder with their saws such grains of corn, or other matters, which they might have occasion to carry to their nests, but were too bulky; for that they cut off grass, and other things, which they find in the road to and from their repositories, our author has observed: and it is observable, that the *Hebrew* name of the ant נמלה *nemala*, from the verb נמל *namal*, which signifies to cut off, is used for cutting off ears of corn. (*Job* xxiv. ver. 24. *)

But if we consider the two texts, in the book of *Proverbs*, cited by our author, there is not the least intimation in them of their laying up corn in store against winter. In chap. vi. ver. 8. it is said, *She provideth her meat in the summer, and gathereth her food in the harvest*: for, tho' the former verb הכין *bekin* signifies to prepare, or dispose in order, and the latter אגר *agar* to collect, or gather together; and in the only two places where I find it occur besides, is used for gathering in summer, as *Prov.* x. 5. and for gathering in the vintage, *Deut.* xxviii. 39. yet the expressions, in the text, necessarily mean no more, than that they collect their food in it's proper season; nor is there any thing else declared chap. xxx. ver. 25. So that all which may fairly be concluded from Scripture is, that they carry food for themselves into their repositories †. That they do this against winter can only be determined by examining into the fact: this our author has done with very great diligence, and has discovered, with respect to our *English* ants, that they eat not at all in the winter and have no stores laid in of any sort of food. The opinion therefore of their laying in magazines against winter, seems to me to have been grafted on these Scriptures, rather than found in them; and this from a conclusion naturally enough made, from observing (as I said) their wonderful labour and industry in gathering their food in the summer, supposing that this must be to provide against winter. And, after all, great part of their labour, which may have been bestowed in other services, might easily be mistaken, by less accurate observers, for carrying in food.

* I might also have referred to the *Theatrum universale omnium animalium* of *Jonston*, published by Dr *Ruyssch*, junior, of *Amsterdam*, in 2 volumes folio, Vol. II. p. 85.

† i. e. To serve them as long as it will keep good, or they shall need it.

I am sorry I must omit the ingenious author's just moral reflections ; but my time will only allow me to conclude, as he does, with the words of the Royal Psalmist, *Great is the Lord, and marvellous, worthy to be praised, and there is no end of his greatness.*

*An account
of the Locusts,
which did
vast damage
in Walachia,
Moldavia,
and Transil-
vania, in
1747 and
1748 ; and
of some
swarms of
them, which,
in the months
of July and
Aug. 1748.
came into
Hungary and
Poland ; by a
Gentleman
who lives in
Transilvania.
N^o. 491. p.
30. Jan. &c.
1749. Read
Feb. 2.
1748.9.*

IX. It is certain that the locusts came into *Transilvania* from *Walachia* and *Moldavia*; and particularly thro' those narrow openings in the mountains, which are commonly called *Passes*; the most considerable of which, in the neighbourhood of *Clausenburg*, is called the pass of the *Red Tower*, and through others not far from *Karlstat*, which are common roads from *Transilvania* into *Moldavia* and *Walachia*.

The first swarms entered into *Transilvania* in *Aug. 1747*: these were succeeded by others, which were so surprizingly numerous, that when they reached the *Red Tower*, they were full 4 hours in their passage over that place ; and they flew so close, that they made a sort of noise in the air, by the beating of their wings against one another. The width of the swarm was some hundreds of fathoms, and it's height or density may be easily imagined to be more considerable, inasmuch as they hid the sun, and darkened the sky, even to that degree, when they flew low, that people could not know one another at the distance of 20 paces. But whereas they were to fly over a river that runs in the vallies of the *Red Tower*, and could find neither resting-place nor food ; being at length tired with their flight, one part of them lighted on the unripe corn on this side of the *Red Tower*, such as millet, *Turkish wheat*, &c. ; another part pitched on a low wood : where having miserably wasted the produce of the land, they continued their journey, as if a signal had been actually given for a march. The guards of the *Red Tower* attempted to stop their irruption into *Transilvania* by firing at them ; and indeed where the balls and shot swept through the swarm, they gave way and divided ; but, having filled up their ranks in a moment, they proceeded on their journey.

They are of different forms, according to their different ages: for when, in *Sept.* some troops of them were thrown to the ground by great rains, and other inclemency of the weather, and thoroughly soaked with wet, they crept along in quest of holes in the earth, dung, and straw ; where, being sheltered from the rains, they laid a vast number of eggs, which stuck together by a viscid juice, and were longer and smaller than what is commonly called an ant's egg, very like grains of oats. The females, having laid their eggs, die like the silk-worm ; and we *Transilvanians* found by experience, that that swarm which entered into our fields by the *Red Tower*, did not seem to intend remaining there, but were thrown to the ground by the force of the wind, and there laid their eggs ; a vast number of which being turned up, and crushed by the plough, in the beginning of the ensuing spring yielded a yellowish juice.

In the spring of 1748. certain little blackish worms were seen lying in the fields and among the bushes, sticking together, and collected in clusters, not unlike the hillocks of moles or ants. As no body knew what they were, so there was little or no notice taken of them; and in *May* they were covered by the shooting of the corn sown in the winter. But the subsequent *June* discovered what those worms were; for then, as the corn sown in the spring was pretty high, these creatures began to spread over the fields, and become destructive to the vegetables by their numbers. Then at length the country people, who had slighted the timely warning given them, began to repent of their negligence; for, as these insects were now dispersed all over the fields, they could not be extirpated without injuring the corn.

At that time they differ little or nothing from our common grasshoppers; having their head, sides, and back of a dark colour, with a yellow belly, and the rest of a reddish hue. About the middle of *June*, according as they were hatched sooner or later, they were generally a finger's length, or somewhat longer, but their shape and colour still continued.

Towards the end of *June* they cast off their outward covering; and then it plainly appeared that they had wings, very like the wings of bees, but as yet unripe and unexpanded; and then their body was very tender, and of a yellowish green: then, in order to render themselves fit for flying, they gradually unfolded their wings with their hinder feet, as flies do. And as soon as any of them found themselves able to use their wings, they soared up, and, by flying round the others, provoked them to join them: and thus their numbers increasing daily, they took circular flights of 20 or 30 yards wide, until they were joined by the rest; and, after miserably laying waste their native fields, they proceeded elsewhere in large troops.

Wheresoever those swarms happened to pitch, they spared no sort of vegetable; they eat up the young corn, and the very grass; but nothing was more dismal to behold than the lands in which they were hatched; for they so greedily devoured every green thing thereon, before they could fly, that they left the ground quite bare.

There is nothing to be feared in those places to which this plague did not reach before the autumn; for the locusts have not strength to fly to any considerable distance, but in *July*, *Aug.* and the beginning of *Sept.* and even then, in changing their places of residence, they seem to tend to warmer climates.

Different methods are to be employed, according to the age and state of these insects; for some will be effectual as soon as they are hatched; others when they begin to crawl; and others, in fine, when they are able to fly. And experience has taught us here in *Transylvania*, that it would have been of great service, to have diligently sought out the places where the females lodged; for nothing was more easy, than carefully to visit those places in *March* and *April*, and to destroy their eggs

or little worms with sticks or briars ; or if they were not to be beat out of the bushes, dunghills, or heaps of straw, to set fire to them ; and this method would have been very speedy, convenient, and successful ; as it has been in other places. But in the summer, when they have marched out of their spring-quarters, and have invaded the corn-fields, &c. it is almost impossible to extirpate them, without thoroughly threshing the whole piece of land that harbours them, with sticks or flails, and thus crushing the locusts with the produce of the land.

Finally, when the corn is ripe, or nearly so, we have found, to our great loss, that there is no other method of getting rid of them, or even of diminishing their numbers, but to surround the piece of ground with a multitude of people, who might fright them away with bells, brass vessels, and all other sorts of noise. But even this method will not succeed, till the sun is pretty high, so as to dry the corn from the dew ; for otherwise they will either stick to the stalks, or lie hid under the grass. But when they happen to be driven to a waste piece of ground, they are to be beat with sticks or briars ; and if they gather together in heaps, straw or litter may be thrown over them, and set on fire. Now this method serves rather to lessen their numbers, than totally destroy them ; for many of them lurk under the grass or thick corn, and in the fissures of the ground from the sun's heat : wherefore it is requisite to repeat this operation several times, in order to diminish their numbers, and consequently the damage done by them. It will likewise be of use, where a large troop of them has pitched, to dig a long trench, of an ell in width and depth, and place several persons along it's edges, provided with brooms, and such-like things, while another numerous set of people form a semicircle, that takes in both ends of the trench, and encompasses the locusts, and, by making the noise above mentioned, drive them into the trench ; out of which if they attempt to escape, those on the edges are to sweep them back, and then crush them with their brooms and stakes, and bury them, by throwing in the earth again.

But when they have begun to fly, there should be horsemen upon the watch in the fields, who, upon any appearance of the swarm taking wing, should immediately alarm the neighbourhood by a certain signal, that they might come and fright them from their lands by all sorts of noise ; and if, tired with flying, they happen to pitch on a waste piece of land, it will be very easy to kill them with sticks and brooms, in the evening, or early in the morning, while they are wet with the dew ; or any time of the day in rainy weather ; for then they are not able to fly.

I have already taken notice, that, if the weather be cold or wet in autumn, they generally hide themselves in secret places, where they lay their eggs, and then die : wherefore great care should be taken at this time, when the ground is freed of it's crop, to destroy them, before they lay their eggs.

In *Sept.* 1748. we received certain intelligence, that several swarms of locusts came out of *Walackia* into *Transylvania* thro' the usual inlets, and

and took possession of a tract of land in the neighbourhood of *Clausberg*, near 3 miles in length; where it was not possible to save the millet and *Turkish* wheat from these devourers.

I am of opinion, that no instances of this kind will occur in our history, except what some old men remember, and what we have experienced; at least there is no account, that any locusts came hither, which did not die before they laid their eggs; however, this is a known fact, that, about 40 years ago, some swarms came hither out of *Walachia*, and did vast damage where-ever they settled; but either left this country before the end of summer, or died by the inclemency of the weather.

Perhaps better remedies may be had from other countries, where this evil is more common, against next spring; for the winter season is very safe from this plague.

The gentleman, to whom the foregoing account was sent from *Transylvania* to *Vienna*, and who transmitted it hither, has also informed us, that a considerable number of these locusts had also come within 20 leagues of that city; and that one column of them had been seen there, which was about $\frac{1}{2}$ an hour's journey in breadth; but of such a length, that, after 3 hours, tho' they seemed to fly fast, one could not yet see the end of the column. The eggs of these animals, which have been preserved in dry mould, have produced nothing; but those that have been preserved in mould that was moistened with water from time to time, gave early in the spring of 1749. some of these grasshoppers. The little ones were, soon after they came forth, of the size nearly of ordinary flies: they had already the form of grasshoppers, but they had as yet no wings. This observation shews, that the author of the foregoing account was mistaken, when he says, "These insects had at first the form of grubs, or small worms." They change their skin several times, but they do not acquire wings till they have changed for the last time.

The grasshoppers that were taken in *England* in 1748. have been compared with those that have been sent over from *Hungary* and from *Poland* that same year, and they have been found to be perfectly of the same kind. There are in Sir *Hans Sloane's* collection * some of the same sort of locusts or grasshoppers, preserved in spirits of wine, and which were taken up here above 30 years since, and are exactly like those from *Egypt* and *Barbary*.

X. 1. As I walked by the river's side at *Winchester*, I was told, that now was the time of year that the *May* flies, a species of *Libella*, came up out of the waters, and were seen for a few days, and then disappeared. This excited my curiosity, having never seen this insect.

Some observations on a sort of Libella † or Ephemeron; by Mr Peter Collinson, F. R. S.

* See Sir *Hans Sloane's* Hist. of *Jamaica*, Vol. I. p. 29.

† I take this to be the *musca tripilis* mentioned in *Mouffet*, Insect. Theat. p. 64. and may properly be called *musca, libellæ affinis, cauda tripili*.

N^o. 481. p.

329. Oct. &c.

1746. dated

Winchester,

June 23 1744.

Read Jan. 31.

1744-5.

Fig. 14, 15,

16.

May 26, 1744. I was first shewn it by the name of *May fly*, on account of it's annual appearance in that month. It lies all the year (but a few days) in the bottom or sides of the river, near the likeness of the nymph of the small common *libella's*; but when it is mature, it rises up to the surface of the water, and splits open it's case; then, with great agility, up springs the new animal, with a slender body, with four blackish-veined transparent shining wings, with four black spots in the upper wings; the under wings much smaller than the upper ones; with three long hairs in it's tail.

The husk or *exuviae* that it leaves behind floats innumerable on the water. It seems to me a species of *ephemeron*; and I imagined it was the same insect described by *Goedart* and *Swammerdam*; but a few days convinced me otherwise, for I soon found these had a longer duration than theirs. The next business (after this creature is disengaged from the water) is flying about to find a proper place to fix on (as trees, bushes, &c.) to wait for it's approaching change, which is effected in 2 or 3 days. The first hint I received of this wonderful operation was seeing their *exuviae* hanging on a hedge. I then collected a great many, and put in boxes; and, by strictly observing them, I could tell when they were ready to put off their old cloaths, though but so lately put on. I had the pleasure to shew my friends one that I held on my finger all the while it performed this great work: it was surprizing to see how easily the back part of the fly split open, and produced the new birth, which I could not perceive partakes of any thing from it's parent, but leaves head, body, wings, legs, and even it's three-haired tail behind, or the cases of them. After it has reposed itself a while, it flies with great briskness to seek it's mate.

In the new fly a remarkable difference is seen in their sexes, which I did not so easily perceive in their first state, being then male and female much of a size, but now the male was much the smallest, and the hairs in their tail much the longest. I was very careful to see if I could find them ingendering, conceiving it to be much after the manner of the same species: but that all I could discover was, that the males separated, and kept under the cover of the trees, remote from the river. Hither the females resorted, and mixed with them in their flight (great numbers together) with a very brisk motion of darting or striking at one another when they met, with great vigour, like as house-flies will do in a sunny room. This they continued to do for many hours, and this seemed to me their way of coition; which must be quick and soon performed, as they are of so short duration. I tried several ways to make farther discoveries, but all proved ineffectual. When the females were impregnated, they soon left the company of the males, and sought the rivers, and kept constantly playing up and down on the waters. It was very plainly seen, every time they darted down, they ejected a cluster of eggs, which seemed a pale bluish speck, like a small drop of milk, as they were sinking to the bottom of the river; and then, by the

the elasticity of their tails, they spring up again, and then dart down again. Thus they continue, until they have exhausted their stock of eggs, and spent their strength, being so weak that they can rise no more, but fall a prey to the fish. But by much the greatest numbers perish on the waters, which are covered with them. This is the end of the females; but the males never resort to the river that I could perceive; but, after they have done their office, drop down, languish, and die, under the trees and bushes.

I observed this species of *libella* abounded most with females; which was very necessary, considering the many enemies they have in their short appearance; for both birds and fish are very fond of them, and, no doubt, under the water are a food for small aquatic insects. What is further remarkable in this surprizing creature is, that, in a life of 3 or 4 days, it eats nothing, seems to have no *apparatus* for that purpose, but brings up with it out of the water sufficient support to enable it to shed it's skin, and perform the principal ends of life with great vivacity.

They appear at 6 in the evening. On *May* 26 I perceived a few; but the 27th, 28th, 29th, and 30th, it was a sight very surprizing and entertaining, to see the rivers teeming with innumerable pretty nimble flying animals, and almost every thing near covered with them; when I looked up, the air was full of them, as high as I could discern; and seemed so thick, and always in motion, the like it seems when one looks up and sees the snow coming down: and yet this wonderful appearance, in 3 or 4 days after the last of *May*, totally disappeared.

After I had drawn up these observations, it was more than a year before I had the pleasure of perusing Mr *Reaumur's* elaborate work, wherein he describes this insect by the name of *Ephemere* Tome VI. p. 516. Pl. 44.; but, as I find we vary in many particulars, and as it may tend further to illustrate the subject, I hope the above account will not prove unacceptable to the lovers of Natural Knowledge.

Fig. 14. represents the back of this insect. *Fig. 15.* and *16.* are two sides-views of the same.

2. About the beginning of *May* I observed many deformed water-insects, by Naturalists called *Hexapodes*, creep up out of the water, and fix themselves on the shrubs and rushes; in this situation they continue but a few hours before their back splits open; and from this deformed case creeps out a beautiful fly, with shining transparent wings: at it's first appearance there is only what one may call the rudiments of wings; but it is a most entertaining sight to observe how they shoot out, and expand themselves: thus, in less than an hour, they have attained their complete dimensions. During all this operation the creatures are immoveable, and so continue, until their wings are dry; and then they fly swiftly away, roving about the sides of ponds and rivers, seeking their food, being insects of prey, are very voracious, and, like the hawks among birds, are very swift of flight, and nimbly secure their prey, which

Observations on the dragon-fly or libella of Pensilvania, collected from Mr John Bartram's letters; communicated by P. Collinson, F. R. S. N^o. 494. p. 323. Jan. &c. 1750. Read Feb. 1. 1749 50.

which is mostly flies, and small green grasshoppers: they delight in sunshine; in cloudy weather they are rarely to be seen; but seek protection under the leaves and boughs of trees.

Towards the end of *May* the female is ready to deposit her eggs: she then seeks the warm quiet sides of ponds and water-courses, continuing in a hovering posture, dodging up and down in the water: in this action the male seizes her, and with the end of his tail catcheth fast hold by the back of her head, and flieth away with her. It is uncertain how long they continue in this position before the female bends the end of her body, so as to penetrate the part between the belly and breast of the male. In this singular and surprizing manner she is impregnated; then she repairs again to those still shallow waters, whose bottoms are covered with moss, sticks, and weeds, which may be a security to the little grubs. Here she in a hovering posture deposits her eggs in the water, which immediately sink, and find a proper *nidus* in the aquatic moss, &c. The eggs are soon hatched; the young reptiles creep amongst the stones and weeds, &c. and so continue water-animals the greatest part of the year, until the season comes round for their appearance in that beautiful fly before you, which is different from our *European*; but their process I think well agrees with ours, as it is curiously described and delineated by that excellent Naturalist Mr *Reaumur*. They have a great variety of this tribe of insects in *America*, as well as we have in *Europe*.

A further
account by the
same. Ibid.
p. 400. Read
April 5.
1750.

3. My observations on the wonderful appearance of the *libellæ* or *May* flies of *England* being perused by my ingenious friend Mr *Bartram*, excited him to make the following remarks on their appearance in *Pensylvania*. By the specimens before you, the *May*-flies of *America* have no very remarkable difference from ours; excepting a few days in the fly state, they live all the year a water-insect. Their bodies being replenished with an oily matter, they easily quit their husks, and rise up to the surface of the water, and disperse themselves a mile or more back in the woods, whilst others stay near the water.

May 4, 1749. I perceived many had attained wings, and were very thick spread on the bushes and grass, by the river-sides. The second day after their leaving their aquatic abode they cast another skin, after which their tails are longer, and their wings drier, and more transparent. The 5th and 6th was rainy, the 7th windy; so very few came out. The 8th was cool; so few were seen: but the 9th and 10th, being warm, many swarmed late in the evening; and the 11th, 12th, 13th, they swarmed abundantly. What I call swarming, was their gathering thick as bees, near rivers, to lay their eggs in the water.

In their flight they mount to the tops of trees, 20 or 30 feet high: their motion is surprizing, hovering up and down, rising and falling, 7 or 8 feet at a time: this I take to be the time and manner of their impregnation. After which they fly to the brooks, cast out their eggs, and perish immediately: their eggs sink directly to the bottom, and lodge amongst the mud and gravel, and may be food for some minute water

water animal. From their eggs proceeds a deformed grub, which subsists under water, and is food for eels, until next season, that it attains it's fly state, and then is food for fish and fowl.

The reason of their being so long in coming forth this year was, the cold chilly weather: other years, in a warm season, in five days they would have performed all their functions, and disappeared.

We have two other smaller kinds, that very much resemble the former, but they come later by 2 or 3 weeks: what is most remarkable, the males are black, and live several days after the females.

XI. 1. Mr *John Bartram*, a diligent observer of natural productions, sent me, from *Pennsylvania*, two sorts of curious wasps nests made with clay, which are commonly built against the timber under the roofs of houses and pales, to shelter them from the weather. They feed as the bees, on flowers; but whether they sting like them I do not yet know.

The plain clay-nest is fabricated by a small black wasp, of the same species of that in *Fig. 17.* but less, that has a speck or stripe of yellow in it's tail; and the cells are made four or five together, joining side by side to each other. But the clay-nests that are so elegantly wrought are built by a purplish black wasp, such as is figured in *Fig. 18.*: after one cell is formed, they stop it up, and join another to it's end; and then add another to that; which makes these wrought clay fabrics longer than the plain ones. Their method of working is much alike, and it is very diverting to see them at it: their art and contrivance is wonderful; and, as if it was given to cheer them at their labours, they make a very particular musical noise, the sound of which may be heard at ten yards distance. Their manner of working is, to moisten clay, and temper it up into a little lump, of the size of swan-shot. This they carry to build with; they begin first at the upper end of the cell, and work downwards; until it is long enough to contain the nymph or *chrysalis*: after they have spread out the little lump in a proper manner to form their little fabric, they set up their musical notes, and return to temper and work up more clay for the next course. Thus they continue alternately singing and working, until a cell is finished; which is made delicately smooth within; then, at the further end of each cell, they lay an egg; after this, by surprizing instinct, they go and catch spiders, and cram the cell full of them: but it is further wonderful to observe, that they only in some manner disable the spiders, but not kill them; which is to answer two purposes; first, that they should not crawl away before the cell is finished; and next, that they may be preserved alive and fresh until the egg hatches, which is soon. The spiders, by wonderful instinct, are provided for the *embryo* to feed on: having stored up sufficient for it's support, she very securely closes up the cell, and then proceeds to build the next in the same manner. The maggot, or *embryo*, having eat up all it's provision, before *Oct.* prepares for it's change, and spins itself up in a fine soft silken

An account of some very curious wasps nests made of clay in Pennsylvania; by the same. N^o. 476. p. 363. Apr. &c. 1745. Read April 25. 1745.

filken case, in which it lies all the winter in the *chrysalis*-state, until the spring, when it eats it's way out of it's clay-dwelling.

Since the above account was read before the R. S. I have had the pleasure to peruse Mr *Reaumur*'s excellent work, Vol. VI. on the clay-nests from *St Domingo*; but as these from *Pennsylvania* differ in many circumstances, I hope it will not be unacceptable to the curious to see their figures, with the best account I could procure of them. It may deserve our notice, that these species of *Ichneumon* wasps from *America*, like ours in *Europe* of the same tribe that feed their young with spiders, very much excel them in the elegant structure of their nests.

Fig. 19.

Fig. 19. A plain clay-nest, with single rows of cells.

Fig. 20.

Fig. 20. The backside of the same, by which it was attached to the timber-work of a building; the cells being partly open, containing some of the *embryo*'s in them.

Fig. 21.

Fig. 21. Part of a wrought, or wreathed tubulated clay-nest, as it appears in front on the outside.

Fig. 22.

Fig. 22. The backside of the same, where it adhered to the timber-building or pale; some of the cells being open, disclose the spiders lodged in them.

Fig. 23.

Fig. 23. Part of such another nest as Fig. 21.

Fig. 24.

Fig. 24. The backside of Fig. 23. in which some of the cells being open discover spiders lodged in them.

A description of the great black wasp, from Pennsylvania; by the same. N^o.

493. p. 278.

O&C. 1749.

Read Dec. 21.

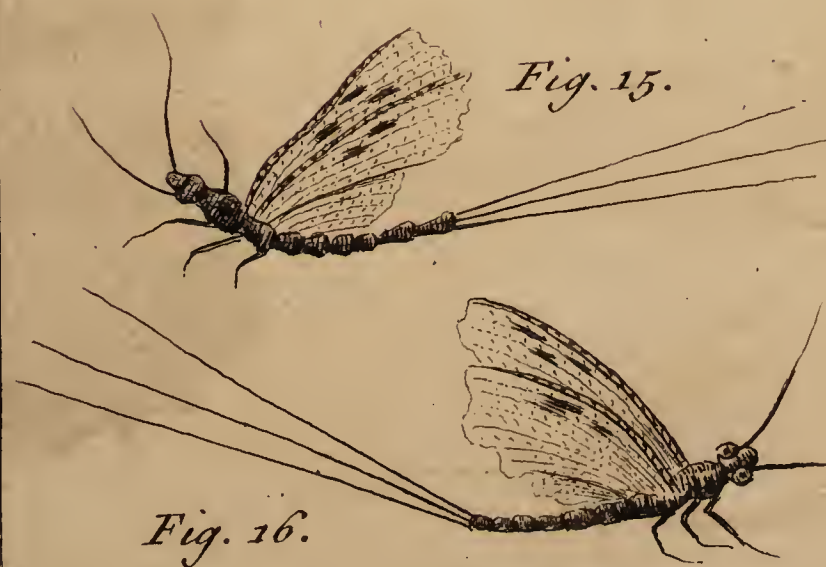
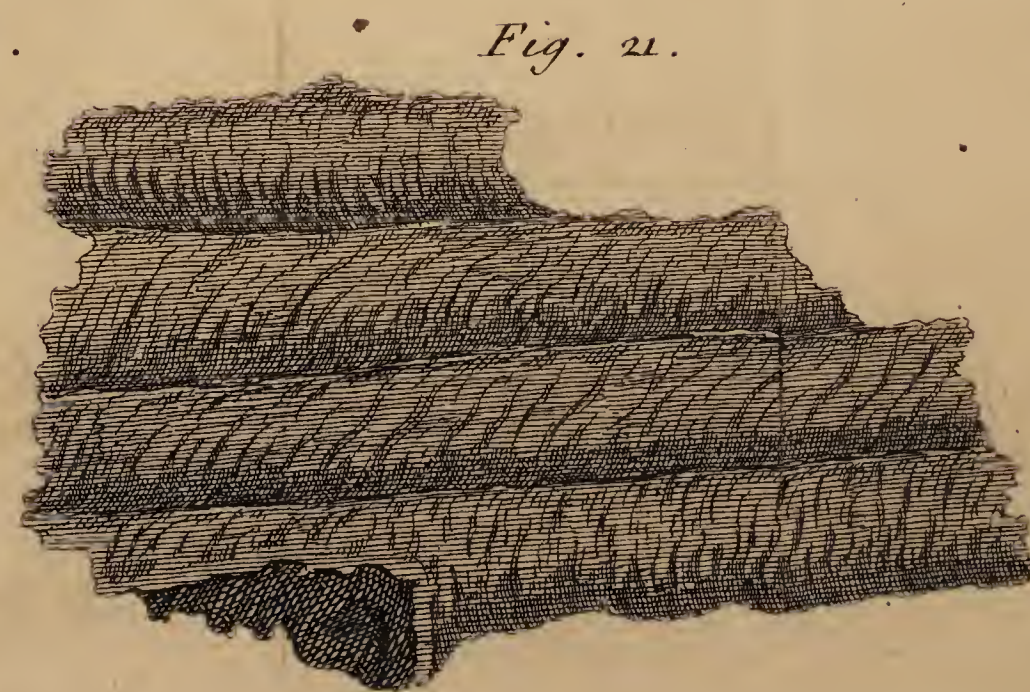
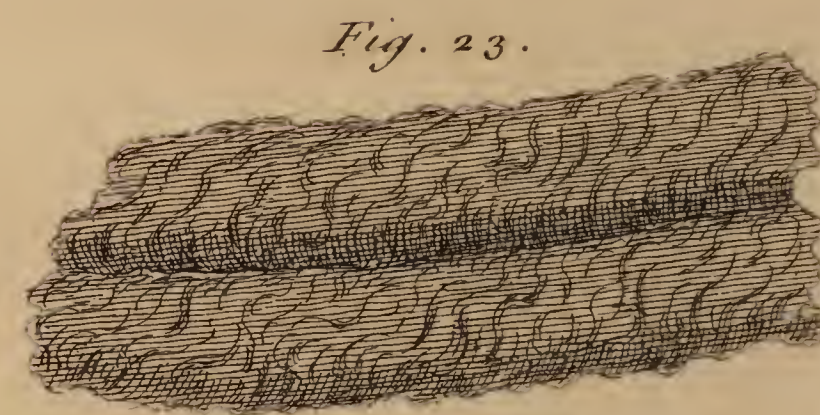
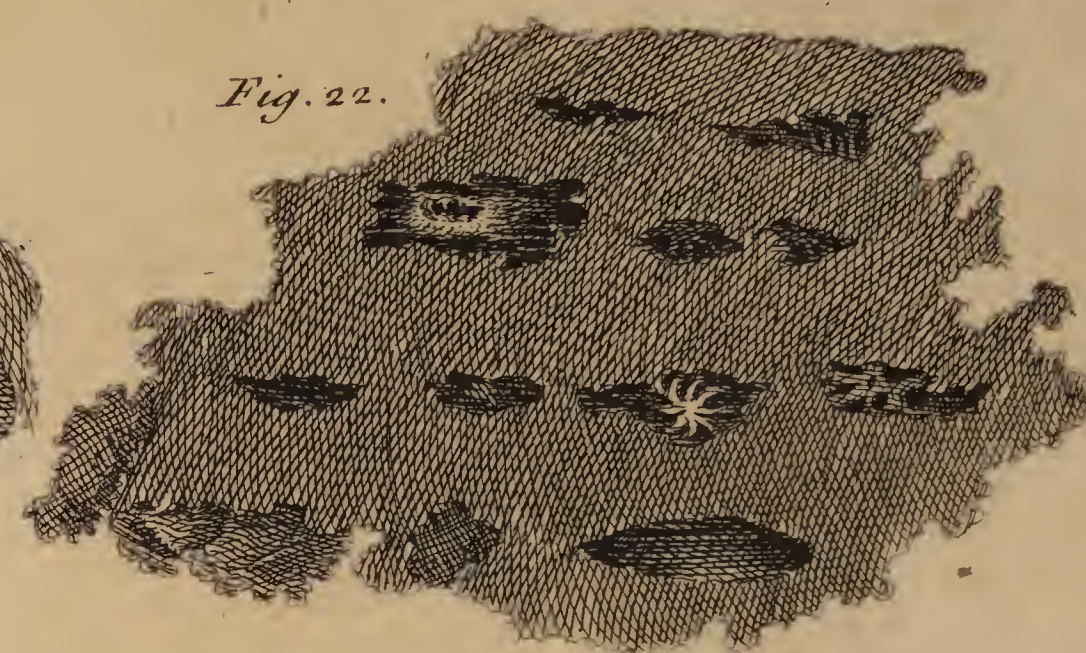
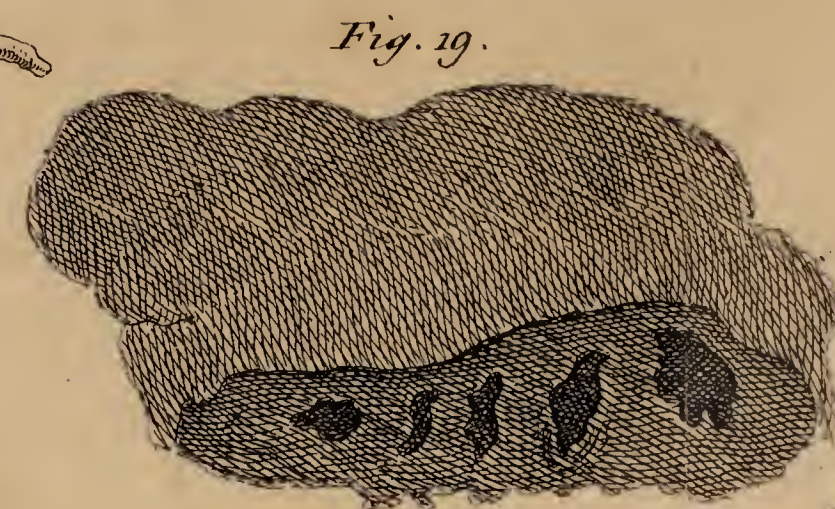
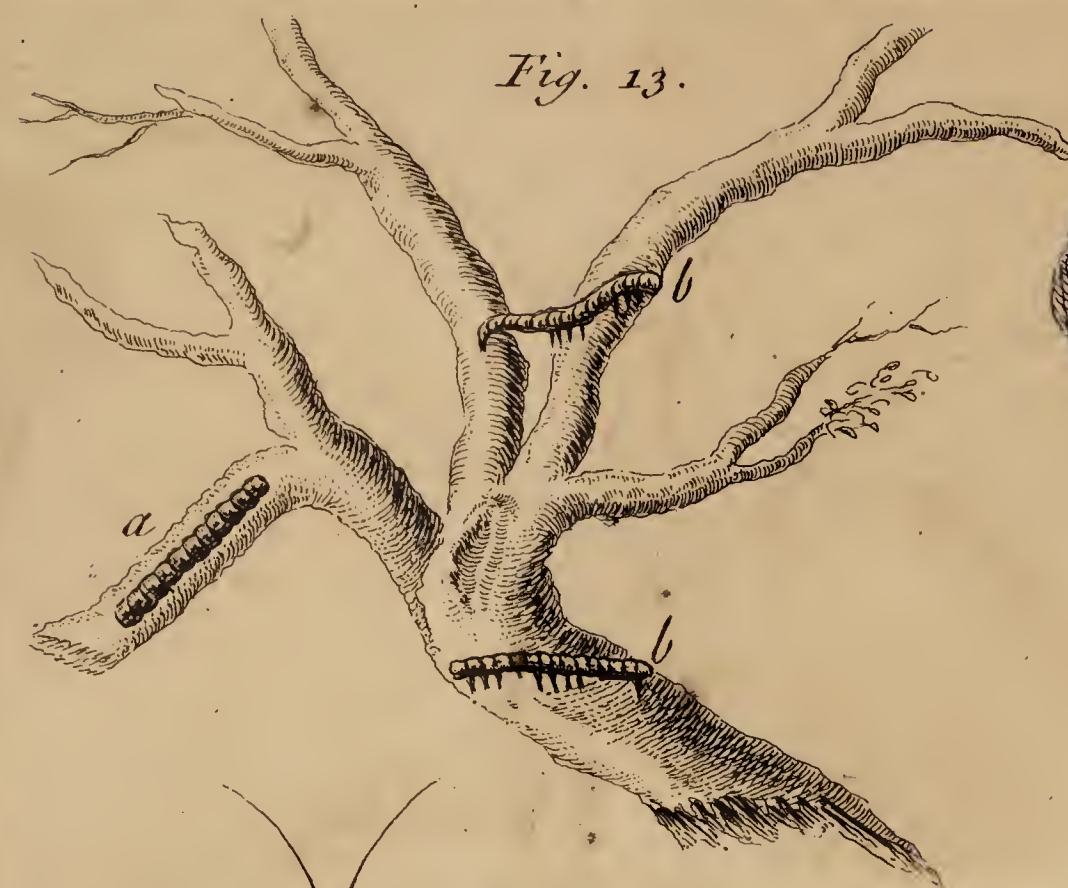
1749.

Fig. 25.

2. Some time ago, some Gentlemen present at a meeting of the R. S. *, were pleased to give a favourable reception to an account of a particular species of small black wasp, that fabricated it's nest with clay, in it deposited an egg, and then stuffed it full of spiders for the maggot to feed on. This that I now lay before you, has something analogous in it's nature, but is a much larger insect, and rarely met with in the province of *Pennsylvania*. The following observations I have collected from my ingenious friend *John Bartram*'s letters.

You will see by the specimen (Fig. 25.) the size of this *great black wasp*; it supplies itself with food, by roving about the meadows, catching grasshoppers, and other insects; on these it feeds, and not on fruits, as other wasps do. But what is more remarkable, is the method of making their nests, and providing for their young. With great pains and industry they scratch a horizontal hole, near an inch diameter, and a foot long, in the steep side of a bank of loamy earth; then away the wasp flies, and catches a large green grasshopper, and lodges it in the farther end of her nest; then she lays an egg, and then goes and catches two more, and deposits them with the other, then plasters up the hole. The egg soon produces a maggot. These grasshoppers, by marvellous instinct, are provided for it's food, until it changes into it's nymph

* See the preceding paper.



state, in which it lies for a certain period, and then eats it's way out, and flies away, seeking it's mate.

But what may deserve our farther attention, is the wonderful sagacity of this creature, not only in catching these large grasshoppers, (See Fig. 26.) which are very like ours, and are very strong and nimble, as most may have observed that take them up: but their peculiar skill is to be admired in disabling them, either by bite or sting, so as not to kill them; for then they would soon putrify, and be unfit for nourishment. Life sufficient is left to preserve them for the time the maggot is to feed on them. The three grasshoppers in the glass-case were all taken alive out of one nest; but they had lost their agility: being secured in a cavity in the earth, may be a means to keep them living: when exposed to the air, they died in a day or two. The sting of this wasp is painful, but does not swell like others.

XII. I had leisure and opportunities many years ago, to make some observations about bees; and all that I made confirm M. *Reaumur's* general theory, as well from his microscopical observations, as those made by means of glass hives; in which he had much greater and better opportunities to make observations than I have had: however, as there are two things, in which my observations are different from his, I think it but justice to the curious and learned world to mention them; that M. *Reaumur*, in case he be alive, and still able to follow his observations, or, if he is not, that some other curious person may make future remarks, to know whether those I have made are true: As I must own myself diffident of my own observations, when they differ from so accurate, minute, and careful an observer, as he has shewn himself to be throughout his whole theory of insects in general, and more particularly of the society of our garden-bee.

A letter from Arthur Dobbs, Esq; to Cha. Stanhope, Esq; F. R. S. concerning bees, and their method of gathering wax and honey. N^o. 496. p. 536. Nov. &c. 1750. dated Castle-Dobbs, Oct. 22, 1750. Read Nov. 8. 1750.

The only two things in which I differ from M. *Reaumur*, are, that I apprehend he says, the bees range from flowers of one species to those of another species, whilst they are gathering one load; so that the *farina*, or crude wax, loaded upon their legs, is from different species of flowers; which is contrary to what I have observed. The other thing that I differ with him in is, that he says the wax is formed in the bee, from the crude wax, or *farina* (so far I agree with him): but by his observations, he says, after digestion it is discharged upwards by the mouth; whereas, by my observations, it is the *feces*, husks, or shells of the *farina* or crude wax, after digestion, discharged by the *anus*.

As to the first, I have frequently followed a bee loading the *farina*, bee-bread, or crude wax, upon it's legs, through a part of a great field in flower; and upon whatsoever flower I saw it first alight and gather the *farina*, it continued gathering from that kind of flower; and has passed over many other species of flowers, tho' very numerous in the field, without alighting upon or loading from them; tho' the flower it chose was much scarcer in the field than the others: so that if it began

to load from a daisy, it continued loading from them, neglecting clover, honeysuckles, violets, &c. ; and if it began with any of the others, it continued loading from the same kind, passing over the daisy. So in a garden upon my wall-trees, I have seen it load from a peach, and pass over apricots, plums, cherries, &c. yet made no distinction between a peach and an almond.

Now M. *Reaumur*, in his memoir upon the bee's making honey, mentions *Aristotle's* observation of the bee's loading or gathering from one species of flower without changing; not quitting a violet to gather from a cowslip; which he says is not justly founded; for he has observed frequently a bee on a large border gathering from flowers of different species. If M. *Reaumur* only means, that, when the bee gathers honey, it takes it indifferently from any flower, I can say nothing against it; but, if he intends to mean the bee's loading the *farina* upon it's legs, then my observation directly contradicts it.

What further confirms my observation is this, that each load upon the legs of a bee is of one uniform colour throughout, as a light red, an orange, a yellow, a white, or a green, and is not upon different parts of the load of a different colour; so that as the *farina* of each species of flowers, when collected together, is of one uniform colour, the presumption is, that it is gathered from one species. For, if from different kinds, part of the load might be of one colour, and part of another.

Another observation to confirm the same fact is, that bees, in the height of the season, return to their hives with loads of very different magnitudes, some having loads as great as small shot, whilst others have very small loads; it cannot be conceived that this difference is from the inactivity or sloth of the bee in collecting it's load, but rather from the scarcity of the flowers, upon which it first began to load.

Now, if the facts are so, and my observations true, I think that Providence has appointed the bee to be very instrumental in promoting the increase of vegetables; but otherwise, might be very detrimental to their propagation; and at the same time they contribute to the health and life of their own species.

From the late improvement made by glasses, and experiments made, in observing the works of nature, it is almost demonstrable, that the *farina* upon the *apices* of flowers is the male seed; which entering the *pistillum* or *matrix* in the flower, impregnates the *ovum*, and makes it prolific. It is often necessary to have wind and dry weather to waft this *farina* to the *pistillum*, and from flower to flower, to make the seed prolific: and we find in wet seasons, that grain, nuts, and fruit, are less prolific, by the *farina's* not being properly conveyed to the *pistillum*; and also in very hot dry weather, from clammy honey-dews, or, more properly sweet exsudations from the plants themselves, which clogs the *farina*, and causes blights and mildews. Now, if the *farina* of specifically different flowers should take the place of it's own proper *farina* in the *pistillum*,

pistillum, like an unnatural coition in the animal world, either no generation would happen, or a monstrous one, or an individual not capable of further generation.

Now if the bee is appointed by Providence to go only, at each loading, to flowers of the same species, as the abundant *farina* often covers the whole bee, as well as what it loads upon it's legs, it carries the *farina* from flower to flower, and by it's walking upon the *pistillum* and agitation of it's wings, it contributes greatly to the *farina*'s entering into the *pistillum*, and at the same time prevents the heterogeneous mixture of the *farina* of different flowers with it; which, if it strayed from flower to flower at random, it would carry to flowers of a different species.

Besides these visible advantages, it may be of great benefit to their own species and society; for, as this *farina* is the natural and constant food of the bees, during one half of the year, and from this digested, as it is accurately observed by M. *Reaumur*, is the *bouillée* and gelly formed; which is lodged for the food of the young bees, until they become *nymphæ*: it is also necessary that stores of it should be lodged in the cells adjoining to the honey, for their winter provision; without which Mr *Reaumur* observes they would be in danger of dying of a looseness, their most dangerous malady.

It seems therefore highly reasonable to believe, that different kinds of *farina* may have different physical qualities: so that, by making collections of the same kind in each cell, they may have proper remedies for themselves against ailments we have no knowledge of, which otherwise they would not have, if they were filled at random from all kinds of flowers. These further advantages, directed to them by Providence, seem to add weight to my observations, and are a presumptive proof that they are true.

The only thing, besides the former, wherein my observations differ from M. *Reaumur*, is in the manner the wax is made and emitted by the bee. I absolutely concur with him, that the wax is formed by digestion in the bodies of the bees, and is emitted by them, and then becomes wax; and that it is almost impracticable to form wax any other way, unless the wax extracted from the myrtle-berries in *America* by boiling be an exception from it.

By M. *Reaumur*'s observations, he forms his opinion, that after the bee has fed upon the *farina*, or bee-bread, and it has passed through the first stomach (which is the reservoir where the honey is lodged, from whence it is discharged upwards by it's mouth into the cells) it is conveyed into the second stomach; and yet, when there, great part of it continues in it's spherical or oval form, still undigested, as viewed by him with his glasses; and consequently must be conveyed further, before it be thoroughly digested, and the particles broke; yet this he supposes is re-conveyed upwards through both the stomachs, and is emitted by it's mouth; and forms his judgment from his observation, that

the bee, when working, and finishing the cells, nips with it's teeth the wax, where it is too thick, or wrong laid; and has observed a motion of it's tongue as it were smoothing or laying on more materials, which he thinks must be then discharged from the stomach by it's mouth.

What makes me disagree with him in his opinion and observations, is from the remarks I have made, that the *fæces* of the bee discharged by the *anus*, after the *farina* is digested, is the true wax. We may with truth believe, that the *farina*, which is the male seed of all vegetables, consists of a spirit or moving principle, floating in a sweet oil, surrounded by an exterior coat or shell, in which is that *monade* that impregnates the grain or fruit, and makes it prolific; that, upon separation or digestion, this spirit and sweet oil becomes the nourishment of the bee; which spirit is of the same nature with the animalcules in *semine masculino* of animals, and becomes the animal spirits in the bee and other animals; and perhaps the true honey is the sweet oil included in the *farina*: and as all vegetables abound with these vegetable vivifying atoms, since, from many, every bud is capable of increasing each species, so the true honey breaking through it's shell by great heat, occasions those honey-dews observed in hot weather upon the leaves and flowers of most vegetables; which is no more than an exudation from the leaves and blossoms of these vessels that break with the heat; besides those that appear on the *apices* of flowers, which afterwards impregnates the fruit.

Of this inward substance of the *farina*, diluted with water, after digestion, is formed the *bouillée* and gelly, which the bees discharge upwards by the mouth, into the cells, to nourish the young bees until they become *nymphæ*; whilst the husk or outer coat is discharged by the *anus*, and becomes the genuine wax.

I have frequently, when bees have been swarming, had them alight upon my hands and cloaths; and many, at different times, have discharged their *fæces* thereupon: this I have taken off, and found it of the consistence of warm wax, with the same glutinous adhering quality, not crumbling like the *farina*. I have also distinguished it by the smell to be wax; but it had a heavier stronger smell, as it was fresh and warm from the bee.

What further confirmed me in this fact, was from my observation of the bees when working up their comb in a glass hive; where I have constantly seen (and must believe it impossible not to be observed by so accurate an observer as M. *Reaumur*) that several bees, soon after one another, have by hasty steps, walked along a comb then forming, for the length of 2 or 3 cells, bending their tails to the comb, and striking it with a wrigling motion from side to side, in a zigzag way; which I was convinced was discharging their *fæces*, or the wax, against the border of the cells, as they ran along, and repeated it as long as they had any to discharge, and then quit it; which is the reason why the outward border of the cells is so thick and strong: and immediately afterwards,

other

other bees came along the cells, and with their fore feet raised up the borders like paste, and thinning it, whilst other bees were ripping off with their teeth, and pruning away any irregular excrescences, so as to make the divisions of the cells vastly thinner than the borders or edges, which were always thick and strong, from the discharging the *faeces* or wax upon them.

M. *Reaumur* has very justly observed, that, besides the 3 transparent smooth eyes, which the bee has placed in a triangle betwixt the *antennæ* on the top of it's head, the bee has also on each side of it's head an eye, or rather a multitude of eyes, formed by a number of distinct *lens's* surrounded each with short hairs, which are confirmed to be eyes, both from *Swammerdam*, and his own experiments to determine it; and that, notwithstanding these *lens's* are lined with a darke opake substance, yet they assist so much their vision, that, when darkened by paint laid over them, the bees could not find their way to their hive, though at a small distance, but soared directly upwards; nor could they find their way when the 3 smooth eyes were darkened.

But there is one observation, which I don't find he has made, which may have determined the garden bees to make almost all their cells imperfect hexagons. The observation is this; that these opake eyes on each side of the head, consist of many *lens's*, each of which is a perfect hexagon; and the whole eye, when viewed in a microscope, appears exactly like a honeycomb: now, as the eyes composed of these hexagonal *lens's*, are in full view to the other bees, does it not seem that Providence has directed them so as to be a pattern set before them, for the bees to follow in forming their combs? Is it not also reasonable to believe, from the disproportion of the convexity betwixt the 3 smooth transparent eyes, and the *lens's* of the dark rough eyes, that they are appointed for different purposes? why may it not be thought that the *lens's* are great magnifiers, to view things nigh at hand, and by many reflexions to convey light into the dark hives, where light is still necessary; and that the 3 other eyes are to observe objects at a great distance, so as to conduct them abroad to fields at a distance, and back again to their hives?

I agree with M. *Reaumur* in the form and use of the fang or tromp of the working bee, and of the use of the mouth within the teeth of the bee; so that it does not suck, but laps or licks with it's rough fang or tromp, like a dog. But I have never observed the bee nipping or breaking open the *apices* of flowers, to let out the *farina*, when it is not fully blown or open; but have often with pleasure observed the bee gathering the *farina* upon it's fang, by licking it off the *apices*, and laying it upon the first pair of legs, which convey it to the second pair, and these lodge it upon the pallet of the third pair, with surprizing briskness; so that, by the time the second pair has lodged it upon the third pair, the bee has gathered more, and lodged it on the fore legs; so that all are in constant motion.

From

From the curious observations made by M. *Reaumur*, upon the structure and behaviour of the queen or mother-bee, the drone or male bee, and the working or mule bee, which is of neither sex ; from the queen bee's being so exceedingly prolific, as to lay from 30 to 40000 eggs of working bees in a season ; besides the eggs of 800 male bees, and of 8 or 10 queen or mother bees ; and from the coldness of the male bee, who so long resists the careffes of the queen or female bee ; and also from the indefatigable labour and oeconomy of the working bee, to nourish the young bees, make up the combs, and lay in stores of *farina* and honey for winter. I think very good reasons may be given why the queen should have a seraglio of some hundreds of male bees ; and why the working bee should destroy the males, when no longer necessary to impregnate the eggs of the mother bee.

It is evident, from the oeconomy of the garden bee, that Providence has appointed that they should share their store with mankind, by making them so industrious in every climate, as to provide, in tolerable seasons, a store of honey and wax, double of what is necessary for their subsistence during the winter, and of combs for the queen's laying her eggs in spring, before new work can be made. From the vast number of eggs which the queen lays in a season, it is absolutely necessary that she should have a great store of male sperm, to impregnate her eggs ; and as the eggs are not sensibly large in her body for 6 months after her coition with the males, who die, or are killed, in *Aug.* and she does not begin to lay from that time till *Feb.* or *March* ; it is therefore necessary that she should have a great store of male sperm within her, to impregnate all the eggs she lays from that time, until *June* or *July*, when young drones or males are hatched, who are not designed for her use, but for the young queens, who go off with the swarms, or for the young queen who succeeds the old one in the old hive ; since the drones are great feeders, and no workers ; and are of no use, but to give a sufficient store of sperm to the mother-bee ; as the working bees have so many enemies to deprive them of their store, they can't be maintained during the winter, even if their life should last so long ; and as it is probable each male has but one act of coition with the queen, as they are so cold, and take so much careffing before they act, and, by M. *Reaumur's* observation, die soon after the act is over, when, probably, their whole store of sperm is exhausted in that act, as soon as the queen has got as much sperm lodged in the proper reservoir, as is sufficient to impregnate all her future eggs, the males are no longer of use ; and if those who have acted, die, those who have not, being of no further use, are killed by the working bee, out of oeconomy to save their winter store, when, probably, by nature they could live but few days more ; as we find the silk-worm moth dies soon after the eggs are laid, as well males as females. It seems therefore necessary that the queen should breed so many males, as, by one act of coition from each, may impregnate all her

her eggs, and that the working bee should dispatch them, as soon as that is over, and a store is lodged.

There are two vessels described by *Swammerdam* in the mother-bee, whose plate *M. Reaumur* has given in his memoirs; one of which is placed betwixt the two lobes of the *ovarium*, which he supposes to be a bladder to contain air; the other is a spherical vessel, seated close by the common duct, in which the eggs fall from the lobes of the *ovarium*, which he supposes is to ooze out a juice to moisten the eggs in their passage. I take one of these, but most probably the last, to be the reservoir and repository of the male sperm, wherein it is lodged from the act of coition, until the eggs are enlarged, and pass thro' the adjoining duct from the two lobes of the *ovarium*.

Since the preservation and increase of bees are evidently beneficial to the public, I approve very much of *M. Reaumur's* instructions in driving bees from a full hive into an empty one, in case it can be done time enough to have new work, sufficient for the queen to lay her eggs in in spring; since they can be fed at very little expence, if care be taken to keep them in a middle state of stupefaction, neither too hot nor cold, during the winter: but I approve much more of his castrating or sharing the combs with the bees, by taking the combs best stored with honey, and leaving those wherein are the *nymphæ* and bee-bread; but think in taking the combs a safer and easier way may be taken, than he directs: his method is to stupefy the bees with smoke, to oblige them to croud together in the crown of the hive, and then turning up the hive, and cutting out the combs filled with honey. Now I think, that turning up the full hive, and setting an empty hive upon it, and driving the bees into it, is preferable to smoaking: for then a very few bees will remain in the full hive; and those few may be stupefied, and the bees in the empty hive being put on a table, the combs may be taken out and selected at leisure, without hazard; and afterwards the empty hive may be turned up, and their old hive set over them, so that they will go up without scruple into their former hive, and repair their work, by making new combs: and if the queen had not quitted the old hive, as is often the case, then they would return to their queen, and the society would not be lost, as is sometimes the case, in driving into an empty hive.

XIII. Some time ago there was found, on an island adjacent to this, a large snake, dead, with a porcupine in it's belly. Perhaps you may not know, that a snake always sucks in it's food by degrees. This had seized the porcupine by the head; and had so sucked it in. When it was quite in, the quills, which were flatted down whilst it was going in, rose; ran through the snake's belly; and killed it: so that there was a monstrous large snake dead, with the quills of a porcupine sticking out of it in many places.

Extract of a letter from Bombay, dated Jan. 23. 1743-4. communicated by Francis Woolaston, Esq; F. R. S. of a porcupine

swallowed by a snake. N°. 475. p. 271. Jan. &c. 1745. Read Jan. 31. 1744.

XIV. On

*A letter from
Mr J. Brein-
tal to Mr P.
Collinson,
F. R. S. con-
taining an
account of
what he felt
after being
bit by a rat-
tle-snake. N^o.
479 p 147.
Mar. & Apr.
1746. dated
Philadelphia,
Feb. 10 1746.
Read April
10. 1746.*

XIV. On *May 2.* last in the afternoon, I took a turn down to the river; and meeting there some company, we tarried about 2 hours. I hearing a bell upon the top of a steep hill, which I knew to be the cows of the people where I then quartered, and thinking to drive them home, it being almost in a strait line thither, went right up the hill; and as it was stony, sometimes I was ready to fall, so saved myself by my hands, and got safe very near the top; where either my foot slipped, or the stone under it gave way, and brought me down upon my knees. I laid my hand on a broad stone to stay myself; and, I suppose, the snake lay on the opposite side, and might be offended by some motion of the stone, so bit my hand in an instant, without any warning or sight; then slid under the stones, and sounded his rattles. I felt a sort of chiliness when I heard the sound; because I had a constant thought, that if ever I was bit, my life was at an end. Without stop I tore up the stones, resolving to slay my murderer: at last I found him, crushed his head to pieces with a stone, took him up in my left hand, and ran to my quarters, sucking the wound on my right hand as I went, and spitting out the poison. This kept it easy; but my tongue and my lips became stiff and numb, as if they had been frozen: so getting quickly home, “I am bit with a rattle-snake, and there lies my murderer!” casting him down on the threshold. All hands were aloft in a minute; some for one thing, some another, as they had seen or known in the like affair; and none seemed less concerned than myself, as I thought by their actions. The first thing applied was a fowl; his belly ript up, and put on my hand alive, like a gantlet, and there tied fast. This drew out some of the poison; for immediately he swelled, grew black, and stunk. I kept my elbow bent, and my fingers up, to keep the poison from my arm. Thus I walked about, and set some of the company to make a fire on the green; for, as it fell out, there were 7 or 8 people there more than our family. It was done quickly, and there we burnt the snake. Another hand this while had got some turmerick. This we bruised well, tops and roots; so made a plaster, and bound it round my arm, to keep the poison in the hand: but night came on, or else, I believe, it had never gone further than the hand; for this kept the arm secure, till midnight, or past. Nor all this while had I much pain: my hand grew cold and numb, but did not swell very much; but now puffed up on a sudden, and grew furious; so I slit my fingers with a razor, and this gave some ease. I also slit my hand on the back, and cupped it, and drew out a quart or more of ugly poisonous slimy stuff. But my arm swelled for all we could do: then I got it tied so fast, that all communication might be stopped with the body, that it seemed almost void of feeling; yet would it work, jump, writhe and twist like a snake in the skin, and change colours, and be spotted; and they would move to and fro upon the arm, which grew painful in the bone. Thus was it tied two days, and all things applied that could be
got

got or thought on. At last, the ashes of white ash-bark, and vinegar, made into a plaster, and laid to the bite, drew out the poison apace.

My tongue and lips swelled that night, but were not very painful, occasioned only, I suppose, by sucking the wound. The swelling of my arm being sunk, 'till it was at least half gone, we then untied it; but in 2 hours, all my right side was turned black, yet swelled but little; nor was there any pain went along with that change of colour. I bled at the mouth soon after, and so continued spitting blood and feverish 4 days. The pain raged still in the arm, and the fever more violent; and by turns I was delirious for an hour or two. This happened 3 or 4 times; and, 9 days being over, the fever abated, and I began to mend; but my hand and arm were spotted like a snake, and continued so all summer.

In the *fall* my arm swelled, gathered, and burst; so away went the poison, spots and all. But the most surprizing and tormenting were my dreams; for, in all sicknesses before, if I could but sleep and dream, I was happy so long; being ever in some pleasing scenes of heaven, earth, or air: on the contrary, now if I slept, so sure I dreamed of horrid places, on earth only; and very often rolling among old logs. Sometimes I was a white oak cut in pieces; and frequently my feet would be growing into two hickeries. This cast a sort of damp upon my waking thoughts, to find my sleeping hours disturbed with the operation of that horrid poison.

XV. I take the liberty to lay before you some observations on the common *lacerta aquatica*, water-lizard, newt or eft; a creature which most people, though without any good reason, have imagined to be venomous and mischievous, and, from a groundless aversion have avoided and neglected much more than it deserves. It is to be found in the spring, and during the whole summer season, in most ditches and shallow standing waters throughout *England*, and is, I believe, unknown to very few; but, lest it should possibly be mistaken for any other creature, I beg leave to lay a picture of what I mean before you.

A letter from Mr. David-Erskine Baker, to the Pres. concerning the property of water-ests in slipping off their skins as serpents do.

N^o. 483. p. 529. Mar. &c. 1747. Read May 14. 1747.

When fully grown, it is about 6 inches in length; the head is like that of a frog, with a couple of fine large eyes: it has four short legs, the two foremost having 4 toes, and the 2 hindmost 5, resembling the feet and toes of a frog; but not at all webbed, as the hindmost feet of a frog constantly are: the tail is very thin and flat, and lies not horizontally, but stands up in a perpendicular position, and serves as a rudder to direct it in swimming. It is amphibious, but lives mostly in the water, wherein, tho' it can swim, it most frequently crawls about at the bottom, rising to the surface only now-and-then, with a wriggling motion, to throw out a bubble of stale air, and take in a fresh quantity of new. There is some difference in their colour, but the back is usually of a light brown, and the belly yellowish spotted with black spots. In winter-time they are seldom or never seen, and therefore may be supposed

to retire into holes, and lie torpid there, as frogs, snakes, and many other creatures do.

Having several times kept these animals for many months together in glass jars, and watched them very attentively from day to day, I am able to speak with much certainty as to the wonderful manner of their putting off their skins without making the least hole or breach therein; a circumstance which has occasioned me to give you the trouble of this paper.

It has long been known, that most of the serpent-kind put off, or, as we commonly term it, cast their skins at certain periodical times; tho' we are very little acquainted with the manner of their performing this work, since it is commonly done in their retiring places, where we can seldom get a sight of them; nor should we indeed know that their skins are changed at all, did we not often find the skins they have cast off. But from this little lizard, which I have more than once carefully attended during the whole operation, a reasonable guess may be formed as to most other kinds; and as it is a creature easy to procure, may be kept in a jar of water for many months, and the intervals between the periods are so short (for they shed their skins every fortnight or three weeks), it is in every body's power to see with his own eyes what I am now going to describe.

A day or two before the skin is to be changed, the animal appears more sluggish than usual, takes no notice of the worms you give it, which at other times it devours greedily; the skin in some places appears loose from the body, and it's colour not so lively as it did before; and thus it continues till the great work of putting off the old skin is to be performed. It begins this work by loosening with it's fore feet the skin about it's jaws (which, when open, are wider than any part of it's own body) and pushes it backward gently and gradually both above and below the head, till it is able to slip out first one leg, and then the other; which when it has done, it proceeds to thrust the skin backwards as far as these legs can reach; it is then obliged to rub it's body against pebbles, gravel, or whatever else it can meet with, till more than half it's body is freed from the skin, which appears doubled back, and covering the hinder part of the body and the tail. When the business is thus far done, the animal, turning it's head round to meet it's tail, takes hold of the skin with it's mouth, and setting it's feet thereon, by degrees pulls it quite off, the hind legs being drawn out as the fore ones were before.

If the skin be then examined it will be found with it's inside outwards, but not having the least hole or breach; that part which covered the hind legs seeming like gloves that are turned without pulling out the tips of the fingers, tho' intirely perfect and unbroken. The coverings of the fore legs remain within the skin. They do not however put off the coverings of their eyes along with the skin, as some snakes are found to do; for the skin of this little creature has always two holes at the places

places where the eyes have been. It is very entertaining to observe it whilst engaged in this necessary work, which sometimes takes up near half an hour, after which it appears full of life and vigour, as well as very sleek and beautiful.

These observations have been made under the inspection of my father, and some other curious friends, who are witnesses of the facts here mentioned. The drawing (*Fig. 27, 28.*) added to this account, and intended to represent one of the animals getting rid of its skin, may, it is hoped, assist to make the foregoing description more fully understood. When the skin is come off, if it be not taken away soon, it is very common for the creature to swallow it whole, as it does all its other food; and if it takes in the head-part, as frequently is the case, the tail-part, being filled with air and water, becomes like a blown bladder, and proves so unmanageable that it is very diverting to see the pains it costs to discharge the air and water, and reduce it to a fit condition to be got down its throat.

Many creatures of very different kinds put off their skins and shells at certain periods. All serpents are supposed so to do; the skins of several kinds being oftentimes found whole. Crabs, lobsters, cray-fish, shrimps, and probably most or all of the crustaceous fishes, cast their shells from time to time; and if one may guess of the rest by the fresh water shrimp, which I have kept several times and observed, their shells are put off without any other breach than one, longitudinally, in the middle of the belly part, through which the body, tail, and claws are pulled out, and the shell left in a manner whole.

Of the insect tribe, every caterpillar has three or four skins before its change into the *aurelia* state, in which the place of creeping out is a little below the head. The spider throws off the skin or shell 3 or 4 times, getting out of it by a rupture underneath, and leaving every claw, and even the horny covering of his *forceps* intire. Even the little mite casts its skin also at several short periods, and nearly in the same manner.

Fig. 28. A skin of the water lizard cast off in the above manner: the extreme thinness of it makes it impossible to be got out of the water, stretched out and dried, and therefore it was necessary to be preserved in spirits.

Fig. 27. One of the living animals.

A Remark by the Publisher.

William Oliver the viper-catcher, mentioned in N^o. 443. of these *Transactions*, made a present to the *Royal Society* of a female viper big with young, which was kept alive in common green moss, in a box with a glass cover. She brought forth several young ones, who slipped off their skins, and the outward membrane of their eyes along with them, in 6 weeks after their birth; and they shed them again 2 months after: but being then put into spirits of wine to preserve them, they were killed; but may still be seen in the *museum* of the *Society*. They first loosen the skin about the mouth, and so slip it off backwards, by wriggling themselves thro' the entanglement of the moss: for some of the skins were torn, and parts stuck in the moss.

C. M.

5 R 2

XVI. I have

Extract of a
letter from Dr
J. Bartram,
to Mr P. Col-
linson, F.R.S.
containing
some observa-
tions concern-
ing the salt-
marsh muscle,
the oyster-
banks, and
the fresh-wa-
ter muscle, of
Pennsylvania.
N^o. 474. p.
157. June &c.
1744. Read
Nov. 8. 1744.

XVI. I have observed something of an extraordinary nature in our *Salt-marsh muscle*: by it's fibrous roots, which strike deep into the soil, it seems to be of a vegetable nature; for, it is highly probable, the animal draws some part of it's nourishment through them: they are fixed by these $\frac{2}{3}$ of their length in the sand, with their broad ends uppermost, which open at every return of the tide, to be replenished by the salt-water: when it is retreated, they are found lodged in the grass, sedge, creeks, and banks, singly and together in plenty. I herewith send you a specimen, which will give you a better idea of this wonderful creature.

There you may plainly observe the ligaments draw their origin from the principal parts of the animal, and unite near the extremity of the shell, which they pass through on that side of the muscle that opens to let in the water; then they divide again into many capillary roots or fibres, which penetrate and extend themselves into the mud or soil of the marsh; which, by long observation, seem to me for 2 uses; first, as I have above observed, to convey part of their nourishment; which seems probable, by their being dispersed through the body of the muscle (This is better seen when alive; but now they are dry, one of the specimens plainly shew it). See Fig. 29.

Fig. 29.

The other use of these fibrous roots (for so I must call them), by their striking deep into the mud or sand, is to secure the creature from being carried away by the rapidity of the tide: so that, in this circumstance, they are somewhat analogous to plants, whose roots both nourish them, and secure them from the injuries of wind and flood.

Our *oysters* are of an oblong figure; they grow at the sides and bottoms of creeks, rivers, and bays, near the sea; but mostly in such a situation where they are near or quite dry at low water: they have the power of opening and shutting, like the muscle, to take in and retain the salt-water, which is their principal nourishment: tho' they stick in the mud, they are not so secured as the salt-marsh muscle before-mentioned; and though these oysters grow in great clusters or heaps, commonly called oyster-banks, yet every one that is alive hath free communication with the air and water, and liberty to open and shut. If the oyster's way of growing may be compared to that of a plant, I think there is great similitude between it and the *opuntia*, or *Indian fig*; a leaf produces and supports a leaf, and so on: thus the young oyster grows on the sides of the old one, which, by degrees, is so deep immersed in the mud, that it dieth; but yet it serves to support the young one upright, until it comes to maturity to produce others; and then that, by degrees, subsides; so that, by this method, banks of dead and living oysters are extended of an inconceivable length and breadth through all our coasts.

Our common *fresh-water muscles* differ from our salt-marsh muscle, in that they are not fixed to any place or thing, but have a method of trailing along on the sandy bottoms of creeks and rivers: they have the power not only of opening and shutting their shells at pleasure, but

have,

have, moreover, the power of creeping * along as it were like a snail, by turning upon the upper edge that opens, and so work themselves along the soft yielding sand in little furrows about $\frac{1}{2}$ an inch deep. I have traced them for several yards, by these little chanel, when the tide is down, and left the sands bare.

XVII. I have observed some circumstances in the structure of shells, which I know not whether they have been sufficiently attended to; I mean their durable hardness and polish; which, in the slenderest of these bodies, is very remarkable; and it is unknown how long they will resist the injuries of time and weather.

I saw an instance of these permanent qualities at *St Edmond's-bury*, in the ruins of that abbey, which is built of a kind of stone composed of grit or sand, interspersed with an infinite number of very minute shells, which appeared to be a species of smooth shining cockle. In several parts of the building, much exposed to the air, the sand was mouldered away; but these exceeding small shells remained intire, and their polish not in the least decayed.

It may be alledged, that a petrifying juice, the same that had united the particles of the sand together, had likewise hardened these, and rendered them more durable than nature had formed them. But, as we have many instances of shells retaining their natural politure and firmness, where no such allegation can justly be made (for instance, the shells found in chalk-pits, in loams, and several other places, where no such juices are hitherto proved to exist); yet of so tender shells, long exposed to the weather, and still remaining uncorrupted, the instance mentioned above is the most singular I know, be the cause of their duration what it may.

The other observation, which I would offer to your consideration, relates, in part, to the same subject, but of a different nature. I was led into it by the following circumstance:

In *September* last, having bought some foal-fish, before they were skinned, I observed their bellies were prominent and hard, as if they were full of large rows; but, instead of that, their guts were filled with shell-fish, a species of *peetunculae*. Before these shells were taken out of the transparent guts of the fish, the whole had very much the appearance of strings of beads, or necklaces; the interstices betwixt the shells occasioning this resemblance. Upon taking the shells out, I observed, that some of them were almost intirely dissolved, others partly so, but many were whole and intire. It is well known in Natural History, that shell-fish are the food of several species of fish. The sea-porcupine, and a kind of ray, are known to subsist chiefly upon them; but then they are wonderfully provided with a suitable *apparatus* for reducing them into a state more fit for digestion: their upper and under jaws are hard

Some observations on the hardness of shells, and on the food of the foal-fish; by Mr Peter Collinson, F. R. S. No. 472. p. 37. Jan. &c. 1744. Read March 15. 1744.

* I have seen this of our horse-muscles in ponds here in *England*. C. M.

enough

enough to break or grind almost the strongest shells to the condition of pulp. But the foal-fish has nothing of this kind : she feeds, we see, on shell-fish, but digests them not by attrition ; for neither her mouth, nor any of her *viscera*, are framed for this purpose ; but, as it would appear from the preceding account, by a proper *menstruum*, which is prepared in the body of this animal.

Thus we see, *shells*, which can resist the teeth of time, the inclemency of seasons, and lie, without apparent decay, for unknown ages, in the bowels of the earth, reduced, in all probability, in a little while, almost into a state of fluidity, by the juices of a small tender animal. Is it the juices of the whole animal, or is it the consequence of such a kind of nourishment, that renders this fish so delicious ? It would seem no unnecessary disquisition of those who are properly qualified, to search, with some care, into the nature of this solvent. It is probable, that either the juice itself, or the fish under proper management, may be rendered more beneficial than has hitherto been expected.

Observations
upon certain
shell-fish
(lodged in a
large stone
brought from
Mahon bar-
bour by Mr
Samuel More,
purser of the
Sterling-Ca-
stle man of
war) ; by
James Parsons,
M. D. F. R. S.
in a letter to
Martin Folkes,
Esq; LL. D.
P. R. S. No.
485. p. 44.
Jan. 1747-8.
Read Jan. 21.
1747.

XVIII. The favour Mr *More* and Mr *Graham* have done the R. S. by their present of the stone containing the shelled fish, and called the *dottle-stone*, has given occasion to the following hints, to shew of what sort these fish are, and that they are rare and curious to us.

Upon our first hearing of them by Mr *More's* letter to *Rich. Graham, Esq*; because it was said the fish were lodged in a stone, it was thought they were the same we know by the name of *pholades*, and of which there are plenty upon our own coasts ; but I believe none of us had seen these before, nor had a notion of any others being lodged in stone but our *pholades* above mentioned ; whereas these seem to be peculiar to the Mediterranean Sea, since *Rondeletius*, and after him *Aldrovandus*, have given their account and figures of them, among others that are inhabitants of the same regions. These two authors call them simply *pholades*, which term is derived of the Greek verb *φωλέω*, and signifies, to have a hiding place ; every animal therefore that absconds in earth or stones might be called so too : hence I think, that *φώλας* is too vague and insignificant a name for any particular animal, and that some other, which has a nearer relation to it's generical character ought to be given to it. If *Aldrovandus* had seen those, which we as erroneously call *pholades*, inclosed in their cells, he would, no doubt, have called them by the same name, and for the same reason ; but I am inclined to think he never saw the intire fish, but only the shell ; because he gives a very imperfect description of it, among several others which he has figures of also, and which he calls *conchæ longæ authoris aliæ*, which follow an account and figure of the *concha longa* of *Pliny*.

Dr *Lister* and *Rumphius* also have figures of this Mediterranean fish, and, after that author, call it *pholas* : but since this term barely denotes the place of it's residence, let us endeavour to give it a proper name, which

which may be done by considering it's similarity with some *genus* already known.

The above-mentioned Gentleman says, the stones are from $\frac{1}{2}$ a hundred to 4 or 5 hundred weight each, lying at all depths to 20 feet under water; full of cells, each containing a single fish, called by the inhabitants the *dottle* fish; which name he judiciously supposes to be a corruption of the word *daetylus* from their form. He also says, the fish is of the same nature with the common muscle, but much more delicious, and that eating them is never attended, with those poisonous symptoms, that have been often thought to be caused by eating muscles.

The shells are indeed, in all respects but one, like the *mytulus vulgaris*, or *common muscle*; these being small at the hinge-end, and having a broad thin edge at the opposite; whereas the former are nearly equal at both ends, as well as strait and somewhat depressed; and as to the structure of the fish of both, they are alike, though with this small difference, that the *lingula* of the common muscle is detached towards the point, and that of the other is confined all along. I therefore submit it, whether either of the following names would not properly express it,

Mytulus cylindroides, the cylindroid muscle, or *mytulus daetyliformis*, the *date muscle*. It's external form conducing much to encourage the latter, which Mr More has hinted at, in calling the stone the *dottle*, *dotting*, or *dating-stone*; for as to the place of it's residence, that belongs rather to it's general natural history than it's distinguishing name.

Dr Lister, and after him M. D'Argenville have drawn our *pholas* with 5 shells; but we have some reason to suspect they are only bivalves; for, upon examining those inclosed in the *specimen* before you, in company with Mr Hill, none of them appeared to have any more than 2 shells. And in a specimen (given to Mr P. Collinson by Sir Charles Wager) of one of these fish, which lodged itself in the bottom of a ship, there were but two shells found; which shell together with the piece of wood, wherein it was lodged, I myself saw along with several other friends in the above-mentioned Gentleman's collection. Now these ought, in like manner, after it's generical character, to be ranged among the *chamæ*; and as they have a *proboscis* which none of the *mytuli* have, I would also offer the following name for this fish;

Chama longa rugis asperis, alba. The long rough white *chama*.

Mr Baker has shewn me another species of *pholas*, which he lately took out of a stone from the coast of Cornwall, and which has more of the *pectuncle* than any other kind, in it's form, *cardo*, and shutting close; which the shells just mentioned cannot do: now these 3 distinct kinds of shell-fish can never be said to be rightly called by the single name of *pholas*.

The common objection to these fish boring their way into the stones in which they are found, *viz.* that the stones are first in a soft state, and so harden about them, may be obviated by the following considerations:
first,

first, that in Mr *More's* great stone, when it was broken, there appeared thro' it's substance several petrified fossil shells; which clearly shew that it's formation was of an ancients date than the age of these muscles can admit of. Secondly, that the holes on the surface are narrower, in general, than the cavity in which the fish lies; and which demonstrates, that they enter young, and are capable of enlarging their room as they grow bigger, by abrading the sides of their cells: and this is further apparent, by the sandy matter found in the bottoms of those cells, which the fish cannot well get rid of, when it happens, that the orifice is higher than the bottom; abundance of which Mr *Hill* and myself observed in some of the holes; and which is easily thrown quite out, when the orifice is depending; for in these we observed none: and this is further confirmed by what Dr *Woodward* relates in the first volume of his catalogue*, that certain pillars of white *Carrara* marble taken out of the sea, on the coast of *Leghorn*, after lying there a number of years, were destroyed by the boring of these *pholades*.

As to the manner of their penetrating the stones, I cannot give the least account of it.

Observations
on the cancer
major; by
Mr Peter Col-
linson, F.R.S.
N^o. 478. p.
70. Jan &
Feb. 1746.
dated New-
port in the
Isle of Wight,
July 1. 1745.
Read Feb. 20.
1745-6.

XIX. In riding about this pleasant island, to see what was curious, or may deserve a traveller's notice, I visited a little parish called *Crab-Nighton*, which probably has it's additional name from the plenty of crabs found on this coast. What intelligence I have gained from the fishermen, and my own experience on the nature and faculties of this animal, I shall carefully relate.

The *cancer major*, or largest species of crab, have their chief abode from 20 to 40 fathom water; they herd together in distinct tribes, and have their separate haunts for feeding and breeding, and will not associate with their neighbours. This has been carefully tried, by taking a crab, and marking it's shell, and carrying it 2 or 3 miles distance, and leaving it amongst the same species. This crab has found it's way back to it's old home, and has been caught by the same fisherman that carried it.

The smallest crab that comes to hand is about the size of a chestnut; the full-grown 7 pounds weight, but there has been one caught that weighed 12 pounds. The bait is flesh, or pieces of skait, or small shark, of which he eats but little. The fishermen all agree, the crab will live confined in the pot or basket some months, without any food but what is collected from the sea-water, and not decrease in weight. The difference of sex is very conspicuous, and they are very prolific; but I could procure no certain account of their way of coupling, nor in what time they attained to full growth.

Once a year, like the lobster, they lose or cast their shells. Against this extraordinary change, they choose a close and well-secured retreat

in the cavities of rocks, and under great stones: there they creep in, and wait, until, by degrees, the parts are disengaged; which is effected by withdrawing their legs from their old shells, leaving them, and the upper part of their body-shell behind. In this naked state they make a very odd appearance, being an ill-shapen lump of gelly-like substance, which gradually hardens into a shell a size larger than the old one: for this is the way of growth appointed for this animal, and others of the crustaceous species. These hints I collected from people of good character, whose employ was always amongst them.

But now I shall relate something more wonderful, of my own knowledge, being one of the most surprizing *phænomena* in nature; which is, that this large species of crab has a power in itself voluntarily to crack and break it's own legs or claws, and drop them off.

The reproduction of the legs of craw-fish has been mentioned in the *Hist. R. Acad. Sc.* with some just remarks about the growth of these creatures shells; but I do not know any writer has taken notice of this strange event of the crab.

Mr Cook, of this place, told me of this marvellous property in the great crab; but I could not comprehend it, until I saw the experiment tried on two crabs; then I was soon convinced of the truth of the fact; for, in a few minutes, the legs all dropped off one after another. This the crab will do in any position; but the easiest method is to lay it on it's back, and then take a pair of strong iron pincers, and break the shell, and bruise the flesh of the third or fourth joint of it's small leg: after it has received the hurt, it bleeds, and gives sign of pain, by moving it's leg from side to side; but afterwards holds it quite still, in a direct and natural position, without touching any part of it's body, or it's other legs, with it. Then, on a sudden, with a gentle crack, the wounded part of the leg drops off at the second joint, or *internodium*, from it's body; just as one sees the neck of a retort separate, where it has been heated by a red-hot iron ring, on the application of cold water. The great legs are cast off in the same manner, but are not so easily laid hold on as the small ones. Those that have not seen this wonderful operation may reasonably conclude, that the leg is cast out of it's joint or socket; but it is quite otherwise; for it cracks and breaks off in the smoothest part of the joint, and the rim of the body-shell is no-ways assistant to it. To try what effect increase of pain would have in this work, a small hole was pierced in the great legs, and then a pointed iron was put in to lacerate the inclosed muscle: the consequence was answerable to expectation; symptoms of greater pain ensued, and the leg was cast off with greater violence. It is really amazing and inconceivable, by what power or contrivance in itself, so wonderful an operation can be performed by the crab, as voluntarily to crack and break so hard a shell, and it's muscles, and then cast off it's legs. The small diameter of this joint, the disposition of the fibres, and a very small circular *fossa*, may contribute greatly to accelerate the work; but yet the

main spring of action (for the present) seems beyond the reach of human comprehension. The whole performance is so curious, and so singular a fact in the history of nature, that it may well deserve a nicer consideration, by those that have greater abilities, and more leisure, for such inquiries.

When the leg is dropped off, a *mucus* or gelly is discharged on the remaining part of the joint next the body, which, as a natural styptic, instantly stops the bleeding, and gradually hardens and grows callous, and forms into a leg in miniature, which, by degrees, shoots forth, and attains to it's natural size, to supply the place of that which was lost.

An experiment was next tried, to see of what great service the *mucus* or gelly was to the crab. When it's legs were all cast off, the ends of 2 or 3 of the stumps were pierced with a pointed iron, so as to break off the gelly that stopped them; upon which signs of more intense pain were exhibited, a very large flux of blood ensued, and the creature soon died in great agonies, as was manifest by a *tremor* of those parts about the mouth, and a frothing like that which attends epileptic fits.

It is reasonable to conclude, that this wonderful faculty, is given to the crab for wise ends and purposes which will evidently appear when the nature of this animal is better known.

The crabs are naturally very quarrelsome, and with their great legs or claws fight and kill one another: with them they catch hold of their adversary's legs, and whatever they seize, they strongly retain for a long while: there is no escaping their cruel foe, but by voluntarily leaving a part of the leg behind, in token of victory; but the principal end for which this is done, is the saving the life of the conquered; for when they are bitten and bruised, and cannot break off that limb, they soon bleed to death.

The fishermen shewed an experiment, to give us some idea of the tenacious disposition of this creature, by obliging a crab with it's great claw to lay hold of a small one: the silly creature did not distinguish that itself was the aggressor; but exerted it's strength, and soon cracked the shell of it's own small leg, and it bled freely; but, feeling itself wounded, to save it's life required a power peculiar to itself to break off that limb in the usual place; which it presently effected, and held fast for a long time the broken part in it's great claw: which evidently shews, that this creature retains whatever it lays holds on, and, when overcome by it's enemy, ransoms it's life at the expence of a limb.

Some account
of the rana
piscatrix; by
James Par-
sons, M. D.
F. R. S. N^o.
492. p. 126.

XX. In some parts of *Italy* this fish is called *rospo*; in others *bora*; and by the *Lombards* *zatto*.

Lophius ore cirroso. Petrus Artedus.

Ὁ ἀλίας βάτον, βάτραχος ἀλίας. *Aristot.*

Rana Piscatrix, by the following authors; *Bellonius, Rondelletius, Salvianus, Gesnerus, Charleton, Willughby, Ray.*

Piscatrix vel marina. Schonveld.——— *vulgaris.* Aldrovand.

Apr. &c.

1749. Read

April 17.

1749.

Altho' this fish is already described by most of the Natural Historians, yet several of it's properties appear to have been overlooked; and as I am persuaded many of this learned Society may not have seen it, I laid hold of this opportunity to lay it before them, with some little account, and drawings, I believe nearer the truth than any exhibited already; referring the curious to the general history of this animal, as collected by *Gesner*; and to Sir George *Ent*'s account and dissection of him, as delivered by Dr *Charleton*, in his *Exerc. de Diff. & Nom. Animalium*; whose figure of him is copied by *Willughby* with most of the dissertation, and which, if I mistake not, was taken from *Salvianus* by Dr *Charleton*, for the better illustration of Sir George's dissertation.

This animal is 4 feet 3 inches long, and about 19 inches from side to side in the widest part. His mouth is very wide, and his teeth are set in clusters in both the upper and under jaws, and not in regular rows, as was the vulgar opinion: they are long and small like spikes, moveable, and directed inward, in order to secure his prey from escaping, after he has once laid hold on him. His lower jaw is longer by far than his upper; having a large capacity in the skin of the former, to yield according to the bulk of the creature he seizes; for with this jaw, and the external clusters of the teeth of the upper jaw, he holds it fast, whilst with another inner cartilaginous jaw (whose teeth correspond with an inner cluster of teeth in the upper) he chews and tears his prey, swallowing it by degrees as he minces it; neither the under jaw, nor external row of the upper, having any share in the mastication at all.

Altho' he is said to be of the cartilaginous kind, his head is as bony as that of any fish; having rough spiny ridges, serving as eye-brows. Between these arise 3 black limber twigs; the anterior is longest, the second shorter, and the next shortest; each having at it's extremity a white flat piece, with which, it is said, he allures other fish to approach near enough to seize on them. There are two others less considerable on his back, between those fins or webs, which, in him, must be called *humeral* webs. These webs are cartilaginous and fleshy, and are supported by strong bones, analogous to the humeral bones of some other animals. Under each of these is a *sacculus* or *marsupium*, which runs up the side of his head, 28 inches deep, and 6 inches wide: these have not been duly taken notice of, except I think by *Bellonius*, who mentions two holes, without adding any thing else about them. But they are of so singular a nature, that I think there is some encouragement to make the following conjecture.

The branchial holes are three on each side, which are situated deep in the mouth, and open into these *marsupia*, the sides of which are the *branceostegæ*, having several long slender cartilaginous bones running longitudinally for their support, analogous to the branceostegal bones of other fishes; so that probably these sacks may answer two ends; first,

to form the *membranæ branchiostegæ* ; and, secondly, to make a convenient receptacle for the young, till they are able to shift for themselves. Perhaps the following conjectures may serve to strengthen this opinion ; for if this end was not to be answered, the *branchiæ* might have been terminated near their origin in the mouth, as it is in other fishes.

Authors have ranked this fish among the cartilaginous tribe, who are said to be viviparous ; but of this there are disputes among them as yet undetermined. Now if this fish does not bring forth it's young perfect, there can be no use assigned to these sacks ; for eggs are deposited by the oviparous tribe in sand, weeds, or any other proper *nidus* ; nor could the creatures by any means place eggs in them, because they open in a wrong direction for such a purpose. But if they are viviparous, then the young may probably be harboured in them, being capable of crawling into them, as we may see by the pectoral webs on the under side.

And to strengthen this conjecture, we may draw another consideration from the manner of their feeding ; for these are creatures of no swift motion, and crawl on bottoms of shoal places, watching and alluring their prey ; now their young cannot be supposed to have power or sagacity enough for this work, till they are grown large and strong, and have these twigs in perfection ; therefore they must of necessity be protected by the parent, till they are able to provide for themselves ; which probably may be when they grow too large to enter into these *marsupia*.

There are 7 small finny webs like little indented leaves, on each side the under jaw, and others of the same kind all round the sides to the tail.

He has a dorsal fin near the tail upon the spine, and a ventral fleshy fin nearer the tail than the former.

The five-fingered webs under the *thorax* are rough and fleshy, shewing their business is to assist in slowly crawling from place to place ; and there appears the vestige of the spine from the place of the vent to the tail on the most posterior part of the belly.

As to the sex of the fish, I could be no judge of it, nor of any internal part, as the *viscera* had been taken out before I saw it, and all appearances destroyed that might inform us ; and therefore we must refer the reader to that curious dissection of it made by the learned Sir George Ent, as it is quoted by *Charleton*, in his *Mantissa Anat.*

References to
the Figures.
Fig. 30.

Fig 30. is a back view of the *rana piscatrix*. *aa*, the bony ridges and asperities between the eyes ; from the central *fulcus* of which arise *b b b*, the three *virgæ piscatoriae*, or fishing rods. *c c*, the *cirruli*, or little webs, all round the borders of the fish. *d d*, the large humeral fins, under which are the openings into the *marsupia* and *branchiæ*. *e e*, the two posterior rods. *f*, the posterior and superior spinal fin. *g*, the tail, which in this fish is vertical.

Fig.

Fig. 31. A view of the under surface or belly of this fish. *a a*, the angles of the lower jaw, seen and felt through the integuments. *b*, the skin or floor of the mouth capable of stretching into a sack, according to the bulk of the prey he holds. *cc*, the fleshy five-fingered webs, by which they crawl upon the bottoms of shoals. *d d*, the openings into the *marsupia* and *branchiæ*. *e*, the vent or anus. *f*, the posterior and inferior spinal fin. *g*, the cartilaginous *branchio-stegal* bones.

Fig. 31.

Fig. 32. is a view of the mouth opened to shew, *a*, the skin of the floor of the mouth, as at *b* in Fig. 31. *b*, the tongue. *cc*, the external teeth in the upper and under jaws, for holding the prey. *d d*, the corresponding clusters of teeth in the inner cartilaginous jaw, for mastication, and tearing the prey. *ee*, the *riētus oris*. *f*, the upper jaw. *g*, the entrance into the *gula* and branchial holes.

Fig. 32.

Fig. 33. is a full view of the opening into the *marsupium*, lying under the fin *d*.

Fig. 33.

XXI. 1. In the beginning of Sept. 1744. I procured a small dace, about an inch in length, which I put into a glass jar, that held near a quart. I kept it till the latter end of May following; in which time it grew full half an inch in length, but very little in breadth.

A letter from Mr Wm. Arderon, F. R. S. to Mr Baker, F. R. S. on keeping of small fish in glass jars: and of an easy method of catching fish. N^o. 478. p. 23. Jan. & Feb. 1746. Read Jan. 16. 1745-6.

I began my observations with this kind of fish, because I took it to be the most difficult (having formerly made some few attempts this way); supposing, if I but tolerably succeeded in this, my other attempts might be more fortunate. All this while it seldom or never eat any thing, excepting the small *animalcula* which happened to be in the water I gave it; which I found necessary to do once every day in winter, and twice or thrice in the spring, as the weather grew warmer. When the water was fresh, it would come up to the top about once in an hour, to blow out some small bubbles of air; then, putting it's nose near the surface, it would take in a fresh supply; and when it had taken in a sufficient quantity, it would retire to the bottom again. But, as the water became more and more adulterated by it's use, it's returns to the surface were more frequent, till, at last, it would remain continually, till I gave it a fresh quantity.

Thus, I believe, I might have kept this fish for years; but a multitude of business one day prevented me from giving it clean water in due time; which unfortunately put a period to the life of my little companion. However, such care I had taken before of this beautiful little animal, that, to the moment it died, it had not so much as lost one single scale. At first when I caught this silver-coloured fish, it would not suffer me to come nigh the glass which included it, without the utmost confusion and surprize; but at last, by gentle usage, and a little art, it grew so tame, that if I came but in sight it would be sure to be of the same side of the glass I was on, and there lie gazing at me until I was weary of observing of it. I very often took the opportunity

of.

of looking upon this fish by candle-light, which I always thought it took great pleasure in.

In the before-mentioned month of *Sept.* 1744. I likewise put a *ruff*, about 3 inches in length, into another glass, which held about 3 quarts. This fish at first appeared mighty reserved, and would not eat any thing, or suffer me to come nigh it, for several days; but, in a very short time, all-powerful hunger assisted me to make it so tame, as is scarce believable. Tho' my dace found, amongst the minute *animalcula*, the little inhabitants of our river-water, enough to subsist upon, this ruff found nothing by which it could satisfy the calls of nature; so, of consequence, was compelled to take what I provided for it, and in what particular manner I was pleased to give it. After this method did I bring it to be so tame, that it would not only eat small worms I threw into the glass for it, but would also take them out of my hands, or of a quill, just as I thought proper to give it them: nay it would even rise out above the water for it's prey: which is quite contrary to the way this kind of fish takes it's food. And, at last, it would come to my hand whenever I put it into the glass, and suffer me to handle it. But, to ease me of my care, when I had made all the observations I thought necessary, and in pity to it's confinement, when I had kept it about 7 or 8 months, I gave it it's liberty.

Out of the various sorts of fish I have made trial of, I never could bring any to be so tame as this above-mentioned: from which I infer, that fish of prey, as pikes, perch, ruffs, &c. are the properest objects for this kind of amusement; and did but our *English Virtuosi* know how easily these sort of fish are tamed, and kept in glasses, it might be a means to lessen their esteem for those brought from *China*; unless it chiefly rise from their coming 4 or 5,000 miles off.

Of all the kinds of fish I have viewed the circulation of the blood in, there's none shew it in a finer manner than ruffs, whose fins are exceedingly transparent: besides, it is a fish that is vastly tenacious of life, and will live twenty or thirty minutes out of water, and not receive much damage.

As what can any ways conduce to the service of mankind, however trifling it may appear to some, will never prove disagreeable to an ingenious and humane mind, I shall add hereto a short account how small fish are caught in some parts of *Norfolk*.

At a country town about 5 or 6 miles from *Norwich*, the poor sort of people have a very cheap and expeditious method of catching small fish, such as dace, roach, &c. out of a little rivulet which runs close by.

They procure a bough of white-thorn, which abounds with numbers of thorns; one of which they cut off, and fashion into the form represented by *A. Fig. 34.* To this they tie a piece of thread, as pictured at *B.* Then they take a worm, and slip it on the thorn and thread together, as is shewn at *C.* The other end of the thread they fasten

Fig. 25.



Fig. 30.



Fig. 26.

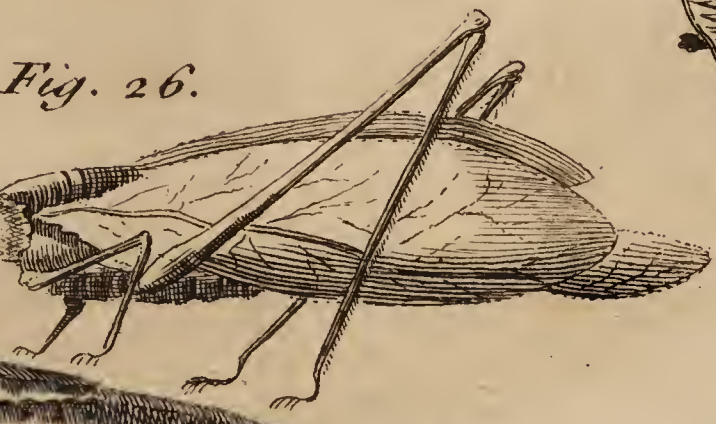


Fig. 27.



Fig. 28.



Fig. 32.

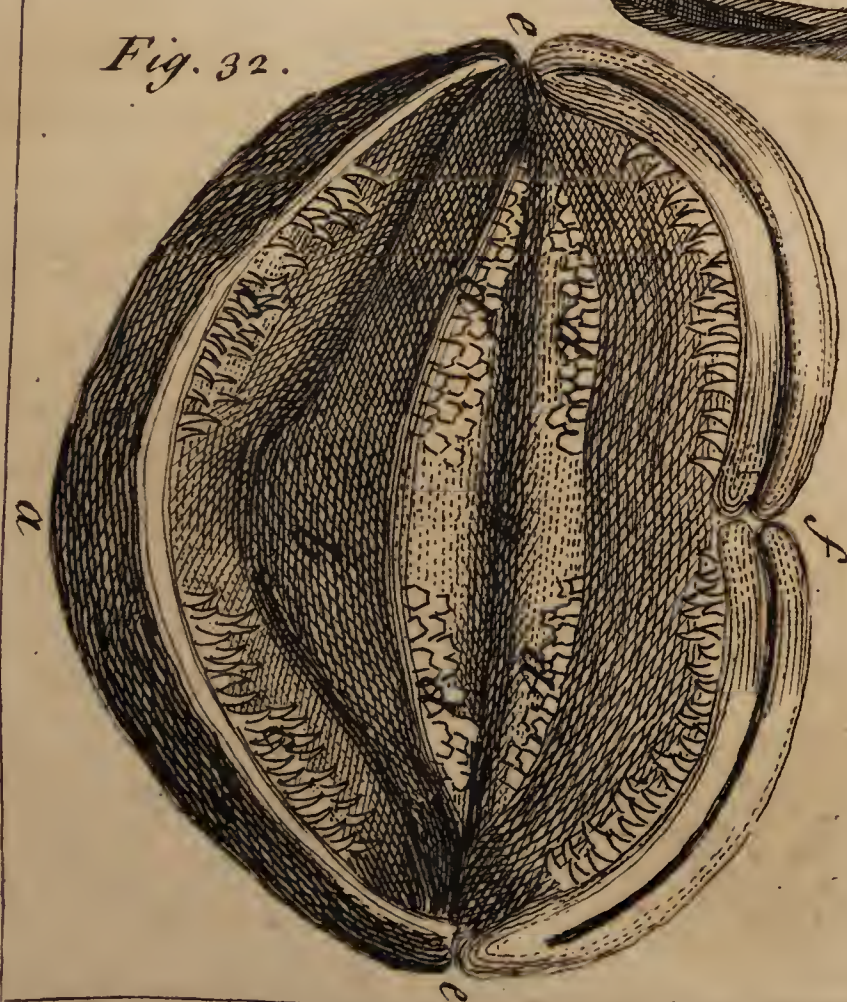


Fig. 31.



Fig. 33.

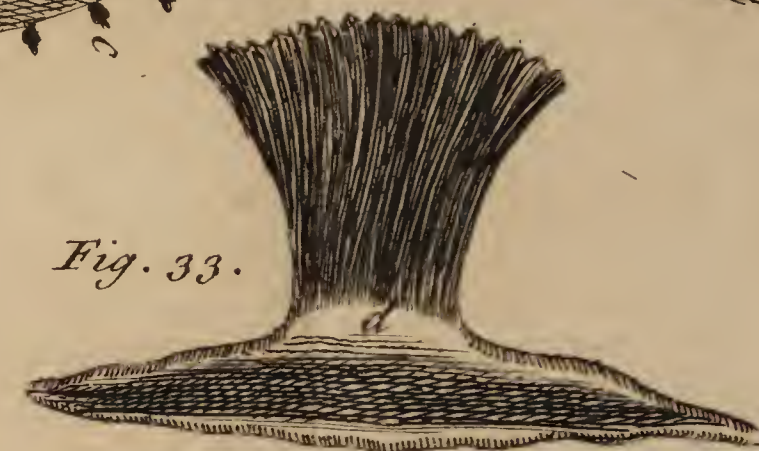
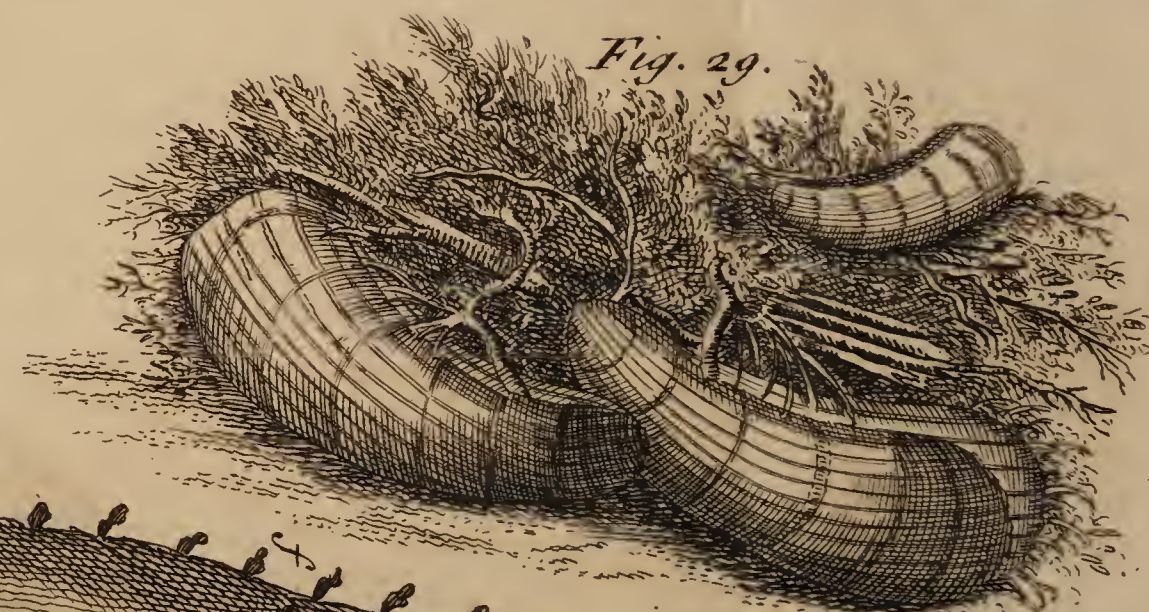
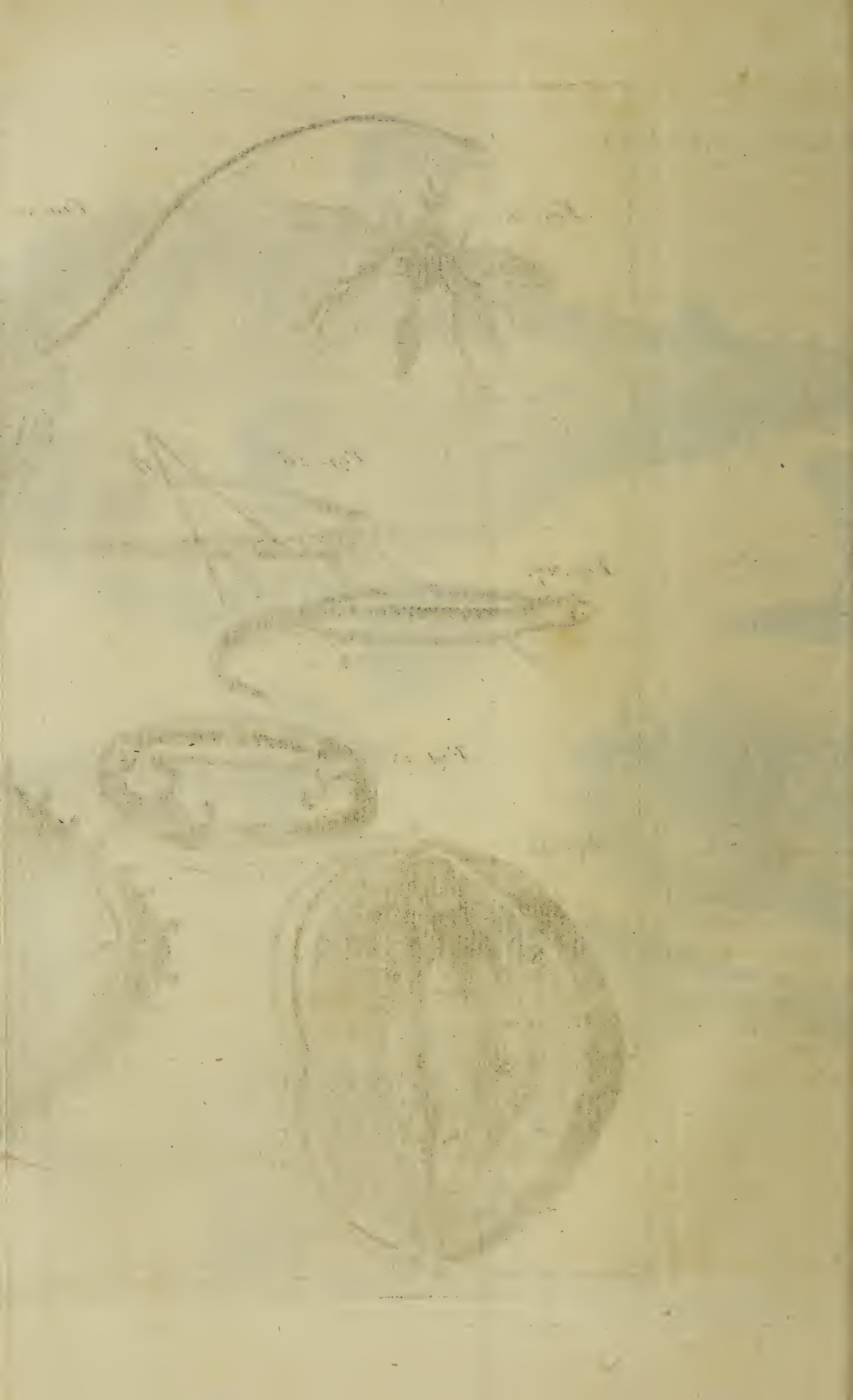


Fig. 29.





fasten to some small twig that hangs over the rivulet, as *D.* Thus they do by some hundreds at a time; by which means they seldom fail of catching a great many fishes: for no sooner does a fish take the worm into it's mouth, and endeavour to be gone, than it's mouth is gagged quite open, and it is presently drowned.

2. Of all the several kinds of fish which for some years past I have been keeping in glass jars (in hopes of becoming acquainted with the nature and properties of these animals, by having them daily under my inspection) none seems more impatient of imprisonment, than the roach; nor, if they are well looked after, and supplied often enough with fresh water, have I observed any, except the roach, to become distempered. But most commonly, after this fish has been a little while confined, the finny part of it's tail begins to drop off piece by piece; and when the finny part is all gone, a sort of mortification seizes upon the tail itself, and gradually creeps along until it reaches the intestines, at which time the fish immediately dies.

The last roach I had under this disorder was about the beginning of *Jan.* when in the space of a month, it had lost the greatest part of the fin, which induced me to clip off the rest, hoping thereby to stop the progress of the mortification. But this was of no manner of service that I could perceive: the distemper still gained ground; and as it increased, a fine fibrillous substance grew out from it, and appeared like what the picture shews at *Fig. 35.* These fibrils, when examined by the microscope, shew themselves to be a number of minute tubes, filled with a brownish liquor; and this liquor, upon pressing them, becomes immediately discharged. A small piece of this fish, with the fibrils growing out of it, as seen by the fourth magnifier of Mr *Cuff's* double microscope, is shewn at *Fig. 36.*

When first I perceived this fibrous substance enveloping the fish's tail, I supposed it to be nothing but a mouldiness, of that kind which frequently is seen upon decayed flesh and fish; but, upon trial, I found it to be of a much stronger texture and consistence than such mouldiness is ever known to have, for, notwithstanding I have several times let a full stream of water run upon it from a cock, I could never wash it off.

This fish lived with me till the latter end of *March*, and then died; having for many days before it's death lain at the bottom of the jar, without being able to rise. As the mortification advanced, and came nearer to it's intestines, the quickness of it's taking water in at the mouth increased, till at last it took it in 3 times faster than a lively strong fish did. On my cutting off part of the fish's tail, in hopes of stopping the mortification, the equilibrium of the body was so far lost, that it hung in the water most commonly with the head downwards, and could never afterwards continue in any other posture, without great strugglings, or sinking down to the bottom of the vessel. Which may serve to shew how nicely and wonderfully the bodies of fishes are balanced, for the keeping

—by the
same. N^o.
487. p. 321.
Apr &c. 1748.
Read May 12.
1748.

Fig. 35.

Fig. 36.

keeping them in a horizontal position; since in this case the losing a few grains of the tail could so sensibly destroy the equilibrium, as to render the rest of it's fins almost useless.

I dare not however assert it will happen thus to all sorts of fish on cutting off the tail; nor does it to the roach immediately: for as it is a posture very unnatural and troublesome to fishes, they exert all their strength to prevent their heads from sinking downwards; until, being wearied out, they at last are forced to submit.

— by the
same. N^o.
482. p 424.
Jan. & Feb.
1747. Read
Feb. 5.
1746-7.

3. About the beginning of last *April* I took a bansticle out of our river, full of spawn, and put it into one of my glass jars, at the bottom of which I had placed a small quantity of sand, as I always do in every vessel wherein my fish are kept; and about the 20th of *May* it buried it's spawn in the said sand. I was in hopes this spawn would have produced a young brood, but was unluckily disappointed; which I impute to it's being frequently disturbed by the pouring in of fresh water.

For some days after I had caught this bansticle, it refused to eat any thing I could offer it, as is common with all fish I have yet kept; but frequently giving it fresh water, and coming often to it, it became so familiar as to eat small worms I now-and-then threw into the jar, and from that time grew so tame as to take them out of my hand; nay it became so bold at last, that when it's belly was full, or it did not like what I offered, it would set up it's prickles, and with it's utmost strength made a stroke at my fingers, if I put them into the water to it.

This fish was of so unfociable a disposition, that it would suffer no other fish to live in the jar with it, and so audacious, as to attack whatever I put in, though ten times it's own size. One day I put a small ruff into the jar to it, which the bansticle immediately assaulted and put to flight, having in the conflict torn off a good part of it's tail; and would, I dare say, have killed it, had I not separated them very soon.

Infinite numbers of these prickle-backs are to be found in almost all fresh waters, where-ever it is possible for fish to live; and whatever other kinds the water is replenished with, this certainly is one, as far as I have yet had opportunity to make any inquiry. The endeavours they use, and the ability they have, to get from place to place, are also extraordinary; for though the largest of them scarce measures above 2 inches in length, I have seen some of them leap out of the water a foot high perpendicularly, and even much further in an oblique direction, when they wanted to get over boards or stones, or some other obstacle to their passage.

It is scarce to be conceived what damage these little fish do, and how greatly detrimental they are to the increase of all the fish in general amongst whom they inhabit. For it is with the utmost industry, sagacity, and greediness, that they seek out and destroy the spawn of all sorts of fish; and moreover all the young fry, that come in their way, are pursued by them with the utmost eagerness, and swallowed down without

without distinction, provided they are not too large. And in proof of what I here assert, I must assure you, that the bansticle before-mentioned in my glass jar, did, on the 4th of *May* last, devour, in 5 hours time, 74 young dace, which were about $\frac{1}{4}$ of an inch long, and the thickness of an horse-hair. Two days after it swallowed 62, and would, I am persuaded, have eat as many every day, could I have procured them for it.

Could Gentlemen, who take pleasure in fish-ponds, intirely prevent these destroyers from getting into them, I am convinced their produce would be much greater than it commonly is: and though it may not be possible to keep them out intirely, it is most certainly advisable to be very diligent in the destroying of them: and whenever, by netting, or other means, any of them are got out of the water, never throw them in again, on a supposition of their being harmless.

Nature has furnished this little fish with a kind of breast-plate or armour, to be it's defence against any outward injury: she has likewise bestowed upon it several offensive weapons or spines, placed upon it's sides and back, which it immediately erects upon the least appearance of danger, or when it attacks some other fish. The sharpness of these prickles guards it well enough from larger animals, that might otherwise prey upon it; but neither these, nor all the endeavours it can use, are able to free it from an enemy that torments it even to death; what I mean is a kind of louse, of an oval figure, having 8 legs, and a very transparent body, which is able either to swim or crawl, and sticks on it so fast, sucking and plaguing it all the while, that it makes it almost mad. One remarkable particular in this louse is, that it's little fibrilous fins are always in motion, whether the creature be swimming about, or fixed upon the fish.

All fish regulate their times of eating and abstinence by the temperature of the air, and the quarter from whence the wind blows; and would those persons who are lovers of angling, take the pains to keep a few small fish in glasses, they might at any time easily foretel, from their taking or refusing food, what sport is to be expected, and often save themselves many a weary step taken to no purpose. I have always observed, amongst the fish I keep in jars, that such as have lived a while together contract so great an affection for each other, that if they are separated they become melancholy and fullen, and are a long time before they forget the loss. About *Christmas* last I put two ruffs into a jar of water, where they lived together until *April*; when, at the desire of a friend, I gave one of them away. After this separation the fish that remained with me was so affected, that for 3 weeks it would eat nothing I could give it; and therefore, fearing it would pine to death, I sent it to the Gentleman on whom I had bestowed it's companion; and what is very extraordinary, upon being put together again, it eat immediately, recovered it's former briskness, and both of them are still alive. I have made abundance of other observations on fish, but shall

only add at present, that when they remain supine and unactive, they every now-and-then gape and yawn, as most land-animals do, when weary of the situation they are in.

Concerning
the perpendi-
cular ascent
of Eels ; by the
same. Ibid.
p. 395. Read
Jan. 29.
1646-7.

XXII. When I read, some years ago, what Dr *Plott* in his history of *Staffordshire* relates concerning the passage of eels across meadows, in the night-time, from pond to pond, I could hardly forbear thinking, that the gentleman there mentioned must by some means or other have been deceived ; but what I have lately seen with my own eyes gives me great reason to believe his account to be strictly true.

On the 12th of last *June*, whilst I was viewing the flood-gates belonging to the water-works in *Norwich*, I beheld a great number of eels sliding up them and the posts adjacent, notwithstanding they all stood perpendicular to the horizon, and 5 or 6 feet above the surface of the pool below the water-works. They ascended these posts and gates, until they came into the dam above : and what makes the matter appear still more strange, they slid up with the utmost facility and readiness ; though many of the boards and posts were quite dry, and as smooth as a common plane had left them.

I observed, that at first they thrust their heads, and about half their bodies, out of the water, and held them up against the wood-work for some time : I imagine, until they found the glutinous matter, which is constantly about their bodies, become sufficiently thick or viscid, by being exposed to the air, to sustain their weight : then would they begin to ascend directly upwards, with as much ease, seemingly, as if they had been sliding along the level ground ; and thus they continued to do, until they had got into the dam above.

The figure of
the mustela
fossilis ; com-
municated
from Dr Gro-
novius at
Leyden to Mr

XXIII. *Mustela fossilis, sive cobite cærulescens, lineis quinque nigris longitudinalibus.* Arted. *Ichthyol. gen.* xi. 3. *. *Vide Fig. 37.*

This fish was kept alive in a jar of water a year wanting 9 days, without changing the water, and without any other food than what the water afforded. They dig them out of the sands near *Wesel* in *Holland*.

P. Collinson, *F. R. S.* N^o. 483. p. 451. Mar. &c. 1747. Read Mar. 12. 1746-7.

Some observa-
tions on the
Belluga-stone ;
by Mr P. Col-
linson, *F. R. S.*
Ibid. Read
March 12.
1746-7.

XXIV. These stones of the *Belluga* were collected by Dr *Cook* at *Astracan*, and sent to Dr *Sanchez* at *Petersburgh*, by whose favour they came to me. I have applied to those Gentlemen to satisfy my inquiries about them, and the accounts they have communicated, with my own observations, are as follow :

The *calculus* of the *Belluga* is found of various shapes and sizes ; it is mostly of a flatted oval figure, sometimes roundish, globular with unequal depressions, and of a yellowish white colour externally, and a smooth polished surface. It differs in magnitude, as it does in figure,

* Willughby, *Hist. Pisc.* p. 124. TAB. G 3, 4. Raii *Syn. Pisc.* p. 69.

from the bulk of a pigeon's egg up to 4 or 5 times that size. They are mostly compact, ponderous and solid, not very friable, but requiring a pretty smart blow of a hammer to break them. They yield easily to the saw; but this defaces their internal texture, which is very remarkably elegant and regular. The stones consist of concentric coats firmly adhering to each other, formed about a *nucleus*, which appeared to be quite an heterogeneous substance, both from it's colour, hardness, and texture. But another obvious circumstance in it's structure renders the *Belluga* stone different from most others, which is it's radiated appearance. It seems composed of an infinite number of shining rays, regularly diverging from the central *nucleus* to the circumference, representing both in colour and form the flakes of a pure white *terra foliata Tartari*, or (excepting the colour, which is yellowish) the striated *spicula* of antimony. This stone is found in the fish called the *Belluga*, a species of sturgeon, the *acipenser tuberculis carens Artedii*, Part III. p. 92. It is commonly called *lapis bellugæ*, by the *Russians* *kamen belluga*, which signifies the same thing.

Of this fish several authors have given us the following account; in shape it is not much unlike a sturgeon (*a*); only it's snout is proportionably shorter and thicker; the skin on the back is light-grey, but under the belly it is white, and without scales (*b*): it's flesh is whiter than veal; whence the name *Belluga*, or the white fish; and affords a much more delicious dish (*c*) than sturgeon. Of it's roe or spawn is made the *caviar*; and some are found so large as to yield from 156 to 200 weight of it. They are found in greatest plenty, and especially those of the largest size (*d*) in the river *Volga*, about the city of *Astracan* (*e*). *Strahlenberg* says, he saw one caught in this river 56 feet long and 18 feet thick; and takes them to be the largest river fish in the world. They are likewise found in other rivers, as the *Don*, and those that flow into the *Baltick* and *Caspian* seas.

I am not certainly informed, neither do authors agree, in what part of the fish this stone is found; *Strahlenberg* says, in the head and stomach; some (*f*) say, in the air bladder; others in a particular bag near the *anus* or inferior gut; others again in still different parts. It is found in both sexes, but ofteneft in the male, and of all ages; but is very rare and scarce, for in a thousand fish it often happens not to meet with a stone.

From hence it would appear, that these stones are preternatural to the fish; perhaps morbid productions, just as the stone in the human bladder, notwithstanding it's curious and regular form; probably the food of the fish; the situation of the parts in which it is generated, and many other circumstances, may contribute to this uniformity of appearance.

(a) Vide Crull's *History of Russia*.
History of Russia.
Cook's letter.

(b) *Strahlenberg's History of Siberia*.
(d) *Stralenbergh, ibid.*

(c) Crull's
(e) Crull's *Hist. of Russia*.
(f) Dr

A little of this stone scraped, and laid upon an hot iron, gave a faint urinous smell, and calcined into a light, greyish, insipid earth. Had it been a real animal substance, or a constituent part of the animal, it's smell would, in all probability, have at once discovered it.

The natives about the *Volga* very much esteem this stone for it's virtues, being in great reputation to promote delivery. The common people take from 10 grains to 30, 40, or even 60 scraped fine in a little water, 2, 3, or 4 times in 24 hours, when the case is dangerous. It is also highly commended as a diuretic and lithontriptic; and this not only amongst the common people, but amongst such as are more capable of informing themselves of it's effects.

- Fig. 38. *Fig. 38.* an oval stone, flat and rugged on it's under side; part of which *a, b*, has been scraped away, and is broken into two pieces by the crack *c, d*. At *e* and *f* appear 2 *nuclei* or centers of smaller incrustations near the surface of the larger stone.
- Fig. 39. *Fig. 39.* Is the larger fragment of the same stone, or the side *b, c, d*. At *g* is a cavity answering to the protuberance *b*, in the next figure. *i, i, i*, are the shining rays diverging from the central *nucleus*.
- Fig. 40. *Fig. 40.* The smaller fragment, or the side *a, c, d*, in which the smaller *nuclei e* and *f*, of *fig. 38.* appear. *b* is the central *nucleus*, which fills up the cavity *g* in *fig. 39.* and *i, i, i*, are the like sprays, or *spicula*, as in *fig. 39.*
- Fig. 41. *Fig. 41.* A smooth oval stone, in form of a long egg.
- Fig. 42, 43. *Fig. 42, 43.* This stone split in two pieces, *a, b* and *c, d*. *e* in *fig. 42* is the *nucleus* or center of the stone, which seems to have been a tooth of a fish, and filled up the cavity *f* in *fig. 43.* and *i, i, i, i*, are the sprays, or *spicula*, in both figures.

A letter from
Mr H. Baker,
F R. S. to the
Pres. concern
ing an extra-
ordinary fish,
called in Rus-
sia Quab; and
concerning
the stones call-
ed crab's eyes.
N^o. 486. p.
174. Feb. &
Mar. 1748.
Read Feb. 25.
1747-8.

XXV. Having been invited some time ago to a correspondence in *Muscovy*, with Dr *James Mounsey*, one of the Physicians to the *Czarina's* armies, a Gentleman much esteemed in that country, for his knowledge in Natural Philosophy, and his unwearied endeavours to discover truth, I readily embraced so favourable an opportunity of making inquiry concerning some things, as to which the accounts hitherto received appeared to me extremely doubtful. I therefore desired of the Doctor to send me what information he could depend on, first, concerning the swallows, and other birds of passage, that are observed in *Russia*, as we have had some accounts of them that seem incredible. Secondly, concerning an extraordinary fish in that country, called the *quab*, which is reported to be first a tadpole, then a frog, and at last a fish. And, thirdly, I requested of him to inform me concerning the *oculi cancrorum*, commonly called *crabs-eyes*, particularly as to their production, and the manner of their being gathered.

As to the *quab*, which some report to be first a tadpole, then a frog, and at last a fish, 'tis very well known, he says, to him; but with regard to such changes, he believes them to be intirely fabulous. He has indeed seen, in the chamber of rarities at *Petersburgh*, this fish, preserved in spirits, under all these appearances; but was not permitted to take out any one of them, in order to remove the scruples he made: however, desiring as far as possible to come at the truth, he turned the bottle

Fig. 42.



Fig. 41.



Fig. 34.



Fig. 43.

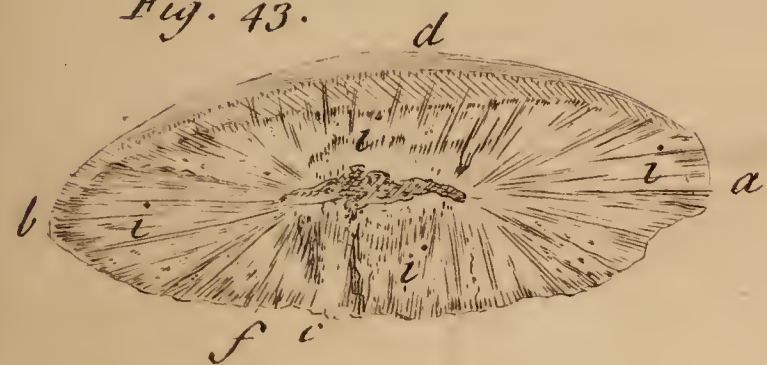


Fig. 40.



Fig. 38.

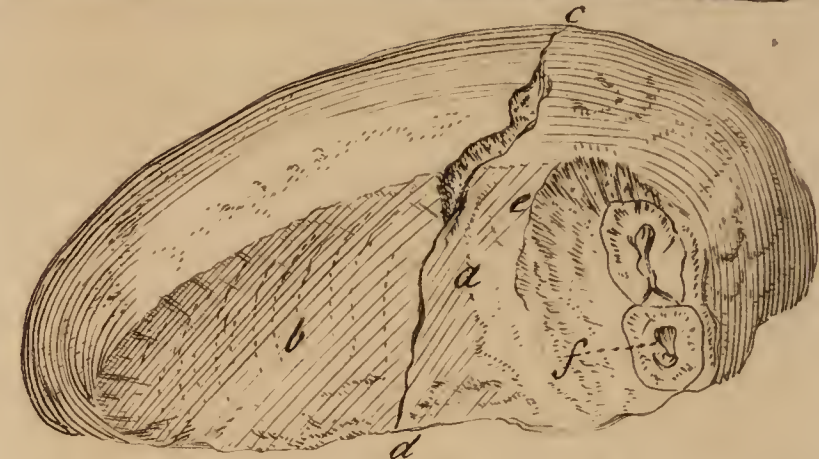


Fig. 39.



Fig. 36.

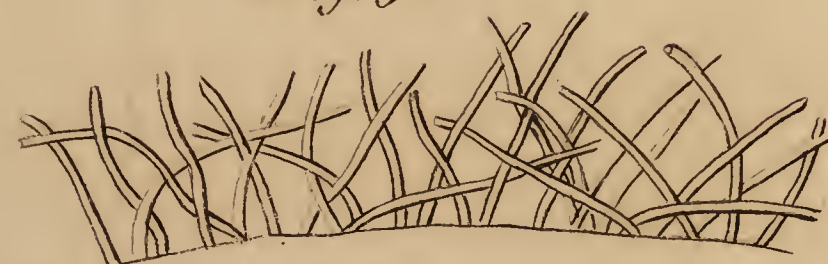


Fig. 37.



Fig. 35.



bottle hastily on one side, to make the fish fall to the glass, which he thought they did, with more seeming hardness than could be supposed in fishes; which induced him to conjecture, that they are pieces of art, the idea whereof has been taken from the resemblance of the head of this fish to that of a frog. Whence he supposes they may be made of wax, and kept in this manner to amuse the world. If there be, he says, such a thing in nature (which he does not think probable) it must be peculiar to some one place, whereof he has no knowledge. He has made inquiries about these imagined changes, of people of many nations, but could never learn any thing to the purpose. He has seen the fish itself in several countries, and found they spawned like other fishes, and grew in size, without the least similitude to what has been asserted. He adds further, that these fishes delight in very clear water, in rivers with stony or sandy bottoms, and are never found in standing lakes, or rivers passing thro' marshy or mossy grounds, where frogs chuse most to be.

As to my inquiries concerning the *crabs-eyes*, he expresses a surprize to find Naturalists differ so much from one another, and yet not one of them he has ever seen giving any true account of the situation, formation, and casting of these concreted bodies. He therefore is so obliging to send me the following description from his own observation and knowledge.

Those concretions called crabs-eyes, are found, says he, in the bodies of cray-fish. Each fish annually produces two, one on either side of the anterior and inferior part of the stomach, and each is generated about a point lying between the coats thereof. The flat or concave side lies next the internal coat, which is very thin and clear, though strong and horny; the convex side is consequently outwards, and is immediately covered by the fleshy and softer coats of the stomach, whose fibres make impressions on it's surface. Between these two membranes it grows by degrees *lamellatim*, and is supplied with petrifying juices discharged through the mouths of vessels or *sudamina* opening on the internal surface of the outer coat. The inner membrane, being horny, gives resistance only; wherefore the stones are concave on that side, and the first remarkable scale (whereon all the others are formed) may be perceived in the centre, the brims or circumferences of many of the rest being very apparent. At the time these stones are not to be found in the animal, there are little circular spots, somewhat opaque, and whiter than the rest of the stomach, to be perceived in their place; nearly opposite to which are tenacious mucilaginous substances, formed like little *placentalæ*, and called by some the glands of the brain: these are larger, and more perceptible when the stones are wanting; but are not turned into stones by different degrees of induration, as some have imagined them to be.

It is believed, he says, that they cast these stones with their shells, which they shed every spring; but he finds this is not the way of getting rid of them; for, a little before, or after the time of their casting, their

their shell, the stones break thro' the internal or horny coat of the stomach, and being ground or broken by the three serrated teeth therein, become dissolved in the space of a few days, which makes it difficult to find them just at this time, and thereby gives ground to imagine they are cast with the shells. He says, however, he has found several of them in the stomach partly consumed, one specimen whereof he has sent herewith, and a farther proof that they are so consumed, is, he thinks, their being never discovered in rivers, tho' the fish themselves be in great plenty there; and in the shops it is observable, that many of these stones are of a brown hue; which is the case of such as have been already lodged in the cavity of the stomach, when the fish was taken. They likewise eat the old shells immediately after shedding them*. What the use of these stones to the creature is, he cannot positively determine, but supposes they may be designed to furnish new petrescent juices to it's fluids; which may be also assisted by the old shells which they devour, the particles whereof, as well as of the stones are probably disposed of, according to their degree of purity, and properly deposited at the extremities of vessels, for the reproduction of their annually new crusty dress; which, he observes, does not greatly recommend the opinion that these stones have a dissolving quality, of service against the stone in the human kidneys or bladder.

The Doctor has sent along with this particular account, specimens of the cray-fish both boiled and raw, which differ little or nothing from those caught in our rivers here; in which I am assured the like concretions may be also found at a certain time of the year: he has likewise sent me some of their stomachs dried, where the stones appear, situated in the manner above described between the two coats; and in one of them they are got thro' the internal coat into the stomach itself. I received also from him several specimens of the beginning scales, or concretions, of different bigness, which he collected himself, in dissecting these creatures; several of the formed stones of his own taking out, some of a larger size, which were given him by a Gentleman, who took them out of the cray-fish in the river *Donne*, and others still larger, which he chose from the Apothecariry of the army. These last were from *Astracan*; and he observes that the fish and stones are much the largest in the great rivers there, where there are fishers for cray-fish on account of the stones only; which they separate from the fish at different fisheries after different manners; at some they are beaten to pieces with wooden pestles; then washing away the flesh and shells, the stones are found remaining at the bottom of the vessel; at others they are laid in heaps till they rot; and then, being washed, the stones are easily separated and gathered. The price comes to a groat or sixpence a pound.

* I have observed the same thing in the small fresh water shrimp; which I have kept in a glass with water throughout several of the periods of it's casting it's shell, which it does once in about a month or 5 weeks. The water-newt also eats it's skin as soon as pulled off, if it be not taken away.

All the Apothecaries shops throughout the whole *Russian* empire are furnished with them, and great quantities besides are exported.

This seems to be a very particular and exact account of these productions, which are frequently prescribed in medicine. Their price, we find, is extremely low in the countries where they are gathered; notwithstanding which, fictitious bodies, made of chalk, tobacco-pipe clay, or other such-like materials, cast in moulds, so as to represent real crabs-eyes, are often substituted instead thereof.

XXVI. This fish is smooth-skinned, has no scales, nor teeth. It has one erect fin on it's back, which arises below it's neck, and runs within a little of it's tail. On each side about the middle, between it's back and belly, behind the gills is a fin: from the bottom and middle of it's belly, a little forward of the vent, arise a pair of fins: from behind the vent runs one fin, within a little of the tail: the tail-fin is large and forked. It's eyes are large; the *irides* are scarlet, encompassed with a circle of a gold colour verged with scarlet. It's nostrils are placed above it's eyes. The back, and upper part of the body quite to the tail, was of a dark blue, or violet colour; these, and the sides of the body, which were of a bright green, were all speckled with oblong white spots; the chaps were of a pale red; the nose, gills, and belly, were of a silver colour; and all the fins of a bright scarlet.

The description of a fish, shewn to the R. S. by Mr Ralph Bigland, on Mar. 22, 1749-50. Drawn up by C. Mortimer, M. D. Sec. R. S. N^o. 495. p. 518. May &c 1750. Read July 5, 1750.

It was 3 feet 7 inches long, and 3 feet 10 inches round in the thickest part. It weighed 82 pounds. It's mouth is small; it's tongue was thick, almost like a human tongue in shape, but rough, and thick-set with beards or prickles, which pointed backwards; so that any thing might easily pass down, but could not easily slip back again; therefore these might serve instead of teeth for retaining it's prey or food. It's gills resemble those of a salmon. It's body grows very taper towards the tail; and, from being compressed to 10 inches thickness, becomes near the tail almost round, and about 3 inches thick. The whole shape of this fish much resembles the sea-bream; but it differs in size, being much larger, and in not having teeth nor scales. The fin standing erect on the back, has some *aculei* next the neck, and rises up 8 inches; but in the middle diminishes to 1 inch; and near the tail rises again to about 3 inches. The belly-fin opposite to this spreads 3 inches near the tail, and diminishes towards the vent. The tail-fin is forked, and spreads 12 inches. The gill-fins are 9 inches long, and 3 wide at their basis. The 2 belly-fins were 11 inches long, and 3 wide at their basis. It seems to me to be a new species of fish, not yet described by any author.

Plate V.

The black prince, and his cousin, from *Anamaboe* on the coast of *Guinea*, and Mr *Creighton*, formerly Governor of *Capo Corso Castle*, upon seeing this fish immediately knew it, and said it was common on that coast, and is very good to eat. The natives call it *opah*, and the *English* there call it the *king-fish*. I shall therefore retain the *Guinea* name, with these characteristics; *OPAH Guiniensium est piscis offeus, non squamosus.*

mosus, edentulus; habens unicam in dorso pinnam anteriùs aculeatam, pone branchias par pinnarum, in medio ventre par pinnarum, ad posticam ventris partem unicam pinnam, caudam forcipatam.

Mr *Bigland* says, that, on opening of it, all it's bowels would have gone into a quart mug; that the flesh of the fore part was firm, and looked like beef, and the hinder part like fine veal; that the bones were like those of quadrupeds; particularly the shoul-der-blades, which resembled those of sheep. In a letter to me, he adds, that probably this was a [Pelagian or] ocean fish, wandering by chance into the Frith of *Forth*; and, by the tide ebbing, being left upon a considerable shoal, or flat sand, near *Leith*, was discovered from land in a state of distress; where-upon some fishermen plunged into the sea, and with a net surrounded it, and brought it to shore.

Extract of a
letter from
Mr W. Arde-
ron, F. R. S.
to Mr H. Ba-
ker, F. R. S.
concerning the
Hearing of
fish. N^o. 486.
p. 149. Feb.
& Mar. 1748.
Read Feb. 11.
1747-8.

XXVII. 1. Though fishes are not provided with organs for hearing, similar to those serving to that purpose in other animals, it would be too presumptuous to declare, without experiment, that they are unable to hear, by organs differently placed, whose situation and structure, for want of due examination, we are unacquainted with.

In order therefore to be able to judge from real facts, without being in the least prejudiced by what has been written for or against their capacity of hearing, I have, for almost 3 years past, been continually trying experiments on several kinds of fishes; viz. perches, ruffs, banticles, millers thumbs, minnows, &c. which I have kept in glass jars for that purpose; and at the hours of feeding them, as well as at other times, have, by different noises, such as whistling, halloing, the sounds of several musical instruments, and every other means I could contrive, endeavoured to discover their sense of hearing, if they were indeed endowed with that sense; but could never perceive they were affected by any of these noises.

But whether fishes do or do not hear, it is certain their senses of *feeling* and *seeing* are exquisitely quick; and I believe, by the extreme sensibility of these two, one may explain most of the accounts that have been brought by writers as proofs of their hearing; such as their coming, when called by their names, as *Plutarch* relates of *Marcus Crassus's* lamprey; their flocking in throngs when called to be fed, as Mr *Bradley* tells us he saw the carps do in the pond of one Mr *Eden* at *Rotterdam*; and their flying away from the halloing and noises made by sailors, as *Wolfgangus* reports the dolphins do, when the sailors have a mind to fright them. But may we not as reasonably imagine these dolphins fly from the sailors, their ships and boats, on account of the violent action wherewith such halloings usually are performed, as merely on account of the noise they make? And in the other cases, is it not as probable, that the fish in ponds, either by their sight or feeling, discovered the approach of their benefactors, whose coming they were accustomed to expect, as that they were sensible of their voices calling them?

I have

The Opah or King Fish.



R. Bigland delin.

A Scale of Inches.



J. Mynde sc.

I have often struck with my thumb-nail against the edge of a glass jar, in which I kept two ruffs, a stroke not harder than the beat of a pulse, which would cause them in a moment to dart from the bottom of the jar to the top; though I am sure they did not see me. But if I made the same motion without hitting the glass, or if I made an hundred times louder noises than the striking of my nail against the glass, at a very small distance from it, I could not perceive they were in the least affected thereby; which, if duly considered, may I think amount to a proof of the deafness or want of hearing in this kind of fish at least; and that their delicate sense of feeling supplies them with the knowledge of the motions of bodies, when their other senses fail. Indeed I have often been convinced by experiment, that their feeling is exceedingly acute, perhaps more so than in other animals; whence I have been led to imagine, that their fins may possibly be the organs more immediately sensible of the slightest motions in the medium wherein they dwell. The curious, who have observed the fins of fishes with the microscope, find them to be composed of infinitely fine vessels, arteries, veins, muscles, and membranous *fibrillæ*, whose structure seems more delicate than is necessary for parts that serve only as oars to waft the fish along. This however I desire you to regard as a mere conjecture, for which the necessary proofs are wanting.

At other times, if, by striking on the top of the jar with a small key, the stroke or tremor has been a little more violent, the fish would shut down their back fins in a moment, and remain motionless at the bottom of the glass. The sudden appearance of my hand at the top of the jar would likewise produce the same effect; but noises made near them seemed to give them no disturbance.

These experiments I have often repeated before several of my acquaintance, as well as by myself in private, and seldom found them to vary much. But fish newly taken out of ponds, or rivers, must not be expected to perform all I here relate: for they, like birds just taken in the fields, and put in cages, are thrown into confusion at the approach of any thing, and endeavour continually to regain their freedom.

If the eyes of fishes be carefully examined, when swimming in a glass vessel, the *cornea* or black *uvea* of their eyes may be seen, sometimes advancing forwards, and at other times retiring back, just as their sight is directed to near or distant objects, through a grosser or finer medium; the form of their eyes altering, as the occasion requires, to make them distinguish objects; and their eyes have so great a liberty in the orbits, that they are able to turn them any way, upwards, downwards, to one side or the other, nearly a quarter of a circle, which makes them full amends for the want of motion in their necks, and enables them to change or direct their *optical axis* to any designed place in a moment.

Those who have been accustomed to fly-fishing can bear witness, that the sight of fishes is quick and distinct almost beyond belief: for it is not uncommon to behold a fish dart itself 20 or 30 yards in an instant at

a fly thrown out at the end of a long line, and catch it even before it can well touch the water. Few other creatures are perhaps capable to distinguish objects so small at so great a distance, at least not so perfectly as these do; for, let the artificial fly differ in colour, shape, or bigness but very little from the natural one it should represent, and not a fish will meddle with it.

These instances of the exquisite *feeling* and *seeing* of fishes, together with their want of organs that can be certainly known to serve them for *hearing*, as well as of sufficient facts to prove that they do hear, may, I think, amount to the highest probability, that they are really destitute of that sense *, and stand in no need thereof, notwithstanding the contrary opinions of some authors: and their living in an element, where land animals are capable of remaining but a very short time, may render an absolute certainty in this case unattainable.

But in order to discover what land animals can do, or what fish, had they organs of hearing similar to those of land animals, would be capable of doing, I endeavoured last summer to find out by experiment,

First, Whether or no sound made in the open air can be heard by a land animal immersed under water.

Secondly, Whether, and in what manner, sound made under water can be heard by a land animal in the open air. And,

Thirdly, Whether, and in what manner, sound made under water can be heard by a land animal that is likewise under water.

To satisfy my first inquiry, whether sound made in the open air can be heard by a land animal under water; I caused three people, stript quite naked, to dive down at the same time, and to remain about 2 feet below the surface of the water; in which situation I spoke to them as loud as I was able. At their coming above water, they repeated my very words, but said I spoke very low.

I caused the same persons afterwards to dive down about 12 feet under water, and a gun was discharged over them, which they all said they heard, but that the noise was scarce perceivable.

As to my second inquiry, whether, and in what manner, sound made under water can be heard in the open air: I caused a young man to dive some feet down, and then to endeavour to halloo, which he did; and I could hear him, though very faintly. But imagining the sound might come up with the water he discharged at his mouth whilst he halloo'd, I contrived a kind of hand-granado, which I threw into a place in the river about 9 feet deep. The fuzee burnt under water near 10'', and then the granado went off, giving a prodigious hollow sound, and shaking the adjacent ground to such a degree, that the whole of a large

* 'Tis not hereby denied, that fishes of the cetaceous kind may probably hear, as well as some other kinds produced in the sea, that have parts in common with land animals. These observations are confined to the common fish of our rivers.

building, some yards distant from the explosion, was put into a tremor, far beyond what could be expected from so small a quantity of powder.

I satisfied my third inquiry, Whether, and in what manner, sound made under water can be heard by a land animal that is likewise under water, by procuring a young man to dive down with a bell in his hand; and he assured me, that he heard it's tinkling very distinctly, at all depths under water, with little or no difference from what he did when rung in the open air: he likewise affirmed, that he plainly heard the noise and rushing of the water, which came violently through a flood-gate, about 20 feet distant from the place he then was in.

2. Our author in the first place classes them into two orders, the first hath lungs, the other is furnished with organs analogous to lungs, which we call fish-ears, or gills: all the whale-kind, the dolphin, porpoise, and such like, have lungs. There are two families of the second class, to one of them belongs all that tribe, which have 1, 2, 5, or 9 air-holes, at the back, or sides of the head, or in their *thorax*, in which concealed gills are found: the other family comprehends all kinds of fishes, whose gills are usually placed on each side the back of the head. Our author's antagonist alleges, that all fishes of both orders are equally deaf; but that all Naturalists except M. *Reaumur* are of a contrary opinion, that fishes hear distinctly.

Our author begins with an air of ridicule, and shews how far the letter-writer is ignorant of the various opinions, modern as well as antient. Our learned countryman Mr *Ray* thinks to reconcile these, by allowing that some hear, while others are deaf; but the greatest part allow that fishes actually hear; and most, except *Scheuchzer*, seem agreed about the auditory passages. But the letter-writer denies they have any organs of voice, merely upon the proverbial authority, *mute as a fish*; hence he concludes they are likewise deaf. But in answer, 'tis replied, the spouting whale hath all it's internal organs, precisely similar to the organs of voice in other creatures, and therefore they may answer the same purposes, nay actually serve this end: for when the whales in the *Greenland* fishery are struck, they roar frequently so loud, as to be heard at 2 *French* miles distance.

But some of the first family of our second class, as the skate, lamprey, conger, and others, our author hath heard utter some kind of noise; and gives his opinion, that most sorts of cartilaginous fishes can do the same. From analogy he argues, that as no beast, from the lion to the meanest animal, nor from the eagle to the humming bird, but can utter a voice, so he thinks the same general law is observed in the oeconomy of fishes: but at the same time our author here seems to lay too much weight upon what he supposes final causes, and metaphysical arguments, which have in all ages ruined Natural Philosophy.

But the letter-writer queries, whether fishes may not be mute in our air, and yet capable of some voice in their own element. Our author takes the noise which carp and such fish make in hot weather, on the surface

Upon the Sounds and Hearing of Fishes; by Jac. Theod. Klein, R. P. Gedan F.R.S. or some account of a treatise, intitled, "An Inquiry into the Reasons why the Author of an Epistle concerning the Hearing of Fishes endeavours to prove they are all mute and deaf;" by Richard Brocklesby, M. D. F.R.S. Ibid. p. 233. Read March 10. 1747-8.

surface of the water, to be a voice: and this is most remarkable when the male impregns the row which the female has before deposited; yet this is often heard, when the fish is 6 or 7 inches under water. Our author further enumerates many foreign fishes, and particularly our smelt, which put alive into vinegar hisses very audibly.

The letter-writer had objected against fishes, that they have no occasion for hearing, because they never copulate, as other animals do: but our author describes the manner of whales, which is performed as that of other animals; and observes, that they bring forth their young alive: these follow the female, and suck milk from the teats, which are placed in them near the organs of generation; and in violent storms the dam takes her off-spring into her mouth, and protects them from danger. This last is common to several of the skate-kind.

The letter-writer alleges, that fish never sleep; but our author assures us, all such as have lungs do in the night-time, thrusting up their nostrils into the open air. For others he cannot be positive, as their history is little known.

The letter-writer premises two questions; first, whether fishes have any ears? or, if the gills serve the same purpose? and answers positively in the negative to both: and therefore concludes they cannot hear. But our author asserts, that snakes, frogs, chameleons, and others of the lizard-kind, actually hear, without any of the usual external *apparatus* of hearing. For though they want the auricles and ears, yet have they auditory passages, by which sound is conveyed, and even internal organs, to which the *meatus auditorius* reaches. But our author farther asserts, that all the whale-kind, and in general such fishes as have lungs, have likewise a *meatus auditorius*, and the internal organs of hearing; and appeals to a public dissection of a porpoise, and another fish of the whale-kind, made by himself; in which the *os petrosum*, with the other parts of these organs, had been separately shewed; and calls in the concurrent testimony of Dr *Tyson*, in his anatomy of a porpoise.

Thus having satisfied us about such fish as have lungs, he goes on to consider the cartilaginous species, such as the skate, ray, and kind of lamprey, which have organs of generation, and copulate like brutes; yet exclude the *fœtus* while yet in the egg-state: and this from analogy, that these, and in general all other fish, as they have organs which serve them for lungs, so they may have what answers in others to the *apparatus* of hearing.

In proof of this he asserts, that all kinds of fish but these which have lungs, are always found to have stones in their heads naturally formed, and invariably placed in the same situation, being joined to the contiguous parts with ligaments and nerves, which take their rise from the substance of the brain; and having examined the head of a pike minutely with a microscope, he discovered the auditory pores in the stones, and persuades himself, that three pair of stones are to be referred to this use; there-

therefore concludes, as there is some analogy in the organs, that all fishes in some measure hear.

The letter-writer farther objects, that water is not the medium of sounds ; and though air is actually contained in all water, yet it cannot be put into undulations, any more than the circumambient water ; but that would require a much greater vibration than the external air can give. Thus, says he, if a person immerge his head a foot under water, he will hear nothing but a boiling din ; and however great a noise is made in the open air, the event will be still the same ; and if the water itself be put into the most violent agitation, the person will discover no odds in that sensation of his ears from what he perceived in the stillest water. Hence he concludes water incapable of transmitting sounds. Our author replies, that as fishes are unanimously agreed to be capable of smelling, so, by analogy, it is probable they have hearing ; for odours are conveyed by the air, as well as sound. But he thinks the unnatural position of a man's head immersed a foot under water may be some cause for that confused noise, and opposes the experimental testimony of Abbé *Nollet* himself, who went different depths under wates, to satisfy himself how far sounds could be conveyed in that medium.

At 4 inches under water he heard the sound of a gun discharged, of a clock striking, and of a hunter's horn ; these, repeated at different depths, were heard first at 4, then at 8, afterwards at 18 inches, and lastly at 2 feet. A man's voice was also heard in the same manner. At different altitudes of water, none of them exceeding 2 feet, he could perfectly distinguish mixt sounds, when 2 bells were struck, or 2 pipes sounded together. He could distinguish under water, very distinctly, words uttered aloud : and proved this assertion, by declaring, when he came above water, what was said while he was under it. All sounds were heard more faintly, and attenuated ; yet the difference of the sound, at 4 and 18 inches depth, was not answerable to the difference of the altitude of water. He observed at first, that momentary sounds were not so well conveyed as continued ; yet he afterwards determined, at the same depth, one tap of a drum head, as plainly as a continued round. This he thinks was the same in a man's voice, and the sound of a pipe ; but ingenuously owns, he was not fully satisfied in this experiment ; and therefore does not lay as great stress on it's certainty as on the former. Lastly, he held his head under the surface of the water, so as barely to cover him ; but could not hear the clock strike, which was audible in the open air at 45 feet distance, especially on a plain. The Abbé therefore concludes, if fishes do not actually hear, 'tis for want of proper organs, and not because the medium cannot convey sounds.

Our author mentions the common notion of carp, and other fish, coming out of their holes at the sound of a bell to be fed ; and adds a story, which Mr *Boyle* somewhere relates, that near *Geneva* a man had a fish-pond, whose banks were so high from the plain on which it was, that one could not look over them into the pond ; and therefore it was impossible

impossible the fish could see the person ; yet they were at any time convened at certain sounds by the Gardener, in order to be fed, as a creditable person asserts.

The letter-writer, having made a high partition in a pond, watched while an accomplice behind it made a very great noise, and discharged a gun, in order to frighten the fish (if possible) that were playing on the surface of the water ; but they did not give any attention ; yet as soon as ever they came in sight, the fish immediately made off.

Our author thinks this objection of little weight, because the question is not, whether fishes, when they see nothing, can be frightened by sounds only.

Upon the whole, our author shews himself an experienced and diligent Naturalist, and will (if I mistake not) be allowed to have fully proved the falsity of any assertion, that all fish are intirely mute and deaf.

*Of birds of
passage ; by
Mr Mark
Catesby,
F. R. S. No.
483. p. 435.
Mar. &c
1747. Read
Mar 5.
1746-7.*

XXVIII. The places whereto birds of passage retreat when they take their leave of us, are first of all to be inquired after ; and then it will be proper to examine by what road, and in what manner they convey themselves to such places wheresoever situated on our globe.

The reports of their lying torpid in caverns and hollow trees, and of their resting in the same state at the bottom of deep waters, are so ill attested, and absurd in themselves, that the bare mention of them is more than they deserve. Of much the like stamp is a late broached hypothesis, which sends them above our atmosphere for a passage to their retreat ; which to me seems as remote from reason, as the ethereal region is from the aëreal ; through which last region I cannot conceive any obstruction to their passage, when, by the approach of our winter, they find a want of food, and at the same time are directed, by instinct, to resort to some other parts of the globe, where they may find a fresh supply. For the want of food seems to be the chief if not the only reason of their migration. And though titmice and other small birds abide here the whole winter, and subsist on insects, which they find torpid, or in a state of mutation, in the crevices of the barks of trees, and other their winter-recesses, yet most birds of passage, having tender bills, are incapacitated for this work ; but then the length of their wings enables them to prey on numberless flying insects, with which the air is stored during the warm months : and it is observable, that not only swallows, but most other summer birds of passage, feed, on the wing, on such-like insects as are seen no more when cold weather begins to come.

The various conjectures concerning the places whereto birds of passage retire, are occasioned by the want of ocular testimony to bring the matter to some certainty. But if the immenseness of the globe be considered, and the vast tracts of land which still remain unknown, unless to their own barbarous inhabitants, it is no wonder we are yet unacquainted with the retreat of these itinerant birds. If I may be allowed to offer
my

my own sentiments, I cannot but agree in the general opinion of their passing to other countries by the common natural way of flying, with this additional conjecture; *viz.* that the places, to which they retire, lie probably in the same latitude in the southern hemisphere as the places from whence they depart; where the seasons reverting, they may enjoy the like agreeable temperature of air.

It may be objected, that places of the same latitude in the southern hemisphere may be divided by too wide a tract of sea for them to pass over. But why then may not some other parts of the southern hemisphere serve their turn? This seems more reasonable to me, than that they should remain on our side of the northern tropic; within a few degrees of which, at the winter solstice, it is so cold, as frequently to produce snow; which, by dispersing such insects as birds, that feed upon the wing, and particularly the swallow kinds subsist on, must make them perish inevitably, were they not to change their quarters for those more favourable climes, where a continuance of warm weather affords their natural and proper food. This their sagacity dictates to them, and is the apparent cause of their periodical leaving us at the approach of winter, before flies are so dissipated by cold and winds as to be found no longer in the air; tho' they may with other insects be met with in holes and hidden recesses, and serve to subsist other birds of passage.

What I infer from hence is, that as swallows cannot continue and subsist so long in cold seasons as other birds of passage, they are necessitated to visit us somewhat later, and to depart sooner: for tho' nightingales, and other birds of passage, are not often seen or observed after they cease singing, yet I have frequently taken notice of them in their solitary coverts a month after the departure of swallows. From these reasons I therefore conclude, that birds of passage, particularly swallows, are necessitated to pass the tropic of *Cancer*; but how far more S. or to what part of the southern hemisphere they go, remains unknown.

The manner of their journeying to their southern abode may vary, as the different structure of their bodies enables them to support themselves in the air: those birds with short wings, such as the red-start, blackcap, &c. tho' they are incapable of such long flights, and with so much celerity, yet I can't see why they may not pass in the like manner, but by gradual and slower movements. Swallows and cuckows may probably perform their flight in half the time; yet there seems no necessity for a precipitate passage, because every day's passage affords them increase of warmth, and a continuance of food a longer time than is necessary for their passage, were it to be the same latitude S. as that from whence they came.

As Providence in many instances has guided defenceless animals to make use of the most necessary means for their security, why may not these, and other itinerant birds, perform their long journies in the night-time, to conceal themselves from rapacious birds, and other dangers that day-light exposes them to; which nocturnal travelling of
birds.

birds of passage I have reason to believe more than barely probable, from the following observation, which may serve in some degree to confirm it :

Lying on the deck of a sloop on the N. side of *Cuba*, I, and the company with me, heard 3 nights successively flights of rice-birds (their notes being plainly distinguishable from others) passing over our heads northerly, which is their direct way from *Cuba*, and the southern continent of *America*, from whence they go to *Carolina* annually at the time rice begins to ripen ; and, after growing fat with it, return S. back again.

The flight of birds of passage over the seas has by some been considered as a circumstance equally wonderful with other stories concerning them ; and especially in regard to those with short wings, among which quails seem, by their structure, little adapted for long flights ; nor are they ever seen to continue on the wing for any length of time ; and yet their ability for such flights cannot be doubted, from the testimony of many. *Bellonius* in particular reports, that he saw them in great flights passing over and re-passing the *Mediterranean* sea, at the seasons and times they visit and retire from us.

The same sagacity that instructs them to change climates may also reasonably be thought to direct them, and other birds of passage, to the narrowest part of our chanel, thereby to evade the danger of passing a wide sea ; tho' by the many instances I have seen of birds driven hundreds of miles from any land, there seems not that necessity for their finding the Straights of *Calais*, as the shortest passage to our island, they being not unable to perform much longer flights.

There are also winter birds of passage, which arrive here in autumn at the time the summer birds depart, and go away in the spring season, when summer birds return. These however are but few ; there being only 4 sorts that I know of ; the fieldfare, redwing, woodcock, and snipe ; which two last I have frequently known to continue the summer here, and breed ; so that the fieldfare and redwing seem to be the only birds of passage that constantly and unanimously leave us at the approach of summer, retiring to more northern parts of the Continent, where they breed, and remain the summer, and at the return of winter are driven southerly from those frigid climates in search of food, which there the ice and snow deprives them of. There are many others, particularly of the duck and wading kind, that breed, and make their summer abode in desolate fenny parts of our island. When the severity of our winters deprives them of their liquid sustenance, necessity obliges them to retire towards the sea in numerous flights, where in open brackish waters they find relief, and at the approach of the spring they retire to their summer recesses. But these cannot be included among those usually termed birds of passage.

Besides the different kinds of swallows, I know of but one kind of *European* bird, that subsists in like manner by catching it's food on the wing,

wing, and that is the *caprimulgus* or goat-sucker, the capacious structure of whose mouth and gullet is formed to receive insects of the larger kinds, as *Scarabæi*, *Grillotalpæ*, &c. These are also birds of passage.

We have, 'tis hoped, made it pretty evident, that summer birds of passage come to and depart from us at certain seasons of the year, merely for the sake of a more agreeable degree of warmth, and a greater plenty of food ; both which advantages they procure by an alternate change of climate ; but the migration of winter birds of passage, and particularly of fieldfares and redwings, is much more difficult to be accounted for, there being no such apparent necessity, either on the score of food or climate, for their departure from us.

The reason of their coming here in winter is, 'tis highly probable, for the sake of food, and a more suitable climate than that they leave behind them ; but, in some severe cold seasons, and when there is a scarcity of berries they subsist here with difficulty, and are even famished sometimes for want of sufficient food ; yet what appears most unaccountable is, that such as have continued with us a whole winter in penury, and should, one would imagine, rejoice at our approaching spring, and build their nests, and couple, on the contrary all depart ; as if that mild and pleasant temperature, which delights and cherishes most other creatures, were disagreeable to them. We know the places of their summer retirement to be *Sweden*, and some other countries in that latitude ; but, as they would find those countries too cold for their reception, and probably destitute of provision, were they to hasten directly thither when they depart from hence, they journey gradually, and prolong their passage through the more moderate countries of *Germany* and *Poland*, by which means they don't arrive at those northern regions, adapted by Providence for their summer abode, and the breeding of their young, till the severity of the cold is so much abated as to render it pleasing to them, and food may be there found. When they visit us again in winter, their return back is after the same manner.

The winter-food of these birds being berries, and particularly haws, as a greater abundance of them grow in this island than can be supposed in the more northern regions, that may possibly be one great allurement to bring them over hither : but the principal reason inducing them to travel southward is probably the rigour and severity of the cold in those frigid climes, which nature therefore directs them to desert for such as are more temperate.

A *Swedish* Gentleman informed me some years ago, that, observing the use we make of quick-set hedges in *England*, he sent some plants of the white thorn over to his own country for the same purposes ; before which time he assured me there were none of them in *Sweden*, which I mention, in support of what was said above.

The coming of these birds to us may then pretty well be accounted for from the reasons foregoing ; but the cause of their departure from hence at the time they leave us, is one of those secrets in nature which

are not yet discovered ; for, should it be suggested, that they do not leave us till the haws and berries are all gone, and they are under a necessity to seek for food elsewhere, this would amount to little, unless it could be shewn, that the northern regions to which they journey can afford them a fresh supply ; which it is almost certain they cannot. And therefore, when first they go from us, they must either alter their diet, or be in much distress ; but, as 'tis evident their food in the summer-time must be of a different kind than what they eat in winter, 'tis most likely they change their diet ; and then one would imagine they should find subsistence here in greater plenty, and much sooner than in the colder countries to which they fly.

In short all, we can be said to know of the matter, ends in this observation, that Providence has created a great variety of birds, and other animals, with constitutions and inclinations adapted to their different degrees of heat and cold in the several climates of the world (whereby no country is destitute of inhabitants) and has given them appetites for the productions of such countries, whose temperature is suited to their nature, as well as knowledge and abilities to seek and find them out : from which we may infer, that the birds we have been mentioning could no more subsist in the sultry climes of the *Molucca* isles, than birds of Paradise could in the frigid regions of *Sweden* or *Lapland*.

Besides the migratory birds already mentioned, which breed and remain the whole summer, there are other birds that arrive periodically at certain places for the sake of some sort of grain, or other food, which may be supposed their own country is destitute of : these birds, after no long continuance, depart, and are no more seen till that time 12 months after ; at which time they return, and so continue repeating these annual visits, as has been already observed of the rice-bird, and blue-wing of *Carolina* (*Nat. Hist. Florid. &c.* Vol. I. p. 14, & 99.) Though the secret ways by which instinct guides birds, and other irrational creatures, are little known to us, yet the causes of some of their actions are apparent.

Analogous to the lucrative searches of man through distant regions, birds take distant flights in quest of food, or what else is agreeable to their nature ; and when they discover some new grain, or pleasing food, they return and acquaint their community therewith, and joining in numerous flights, make annual excursions to solace on this their exotic food.

Since the discovery of *America* there have been introduced from *Europe* several sorts of grain, which were never before known in that part of the world, and which not before some length of time were found out, and coveted by some of these migratory birds. No wonder this grain should not be immediately known to birds of distant regions ; for above half a century passed from the time of cultivating wheat, rice, and barley, in *Virginia* and *Carolina*, before those grains were found out and frequented by these foreign birds, of which one has but lately made it's first

first appearance in *Virginia*, as Dr *Mitchel* informs me, that he being in his garden a bird flew over his head which appeared with uncommon lustre, and surprized him the more, not having seen the like bird before. Mentioning this to some of his neighbours, he was told by them, what afterwards was confirmed to him by his own observation; viz. that these exotic birds had but within these few years appeared in *Virginia*, and had never been observed there before.

They arrive annually at the time that wheat (the fields of which they most frequent) is at a certain degree of maturity; and have constantly every year from their first appearance arrived about the same time in numerous flights. They have attained the name of *wheat-birds*.

XXIX. Persons who have at heart the progress of Natural History, and intend to facilitate the study of it, must needs be desirous to see the collections of divers sorts of productions, which form the objects of it, multiplied and enlarged, and therefore will be disposed to contribute towards it with all their ability. Those collections present together in one place more different sorts of bodies of the mineral, vegetable, and animal kingdoms, there to be at leisure compared and examined one against the other, than one could hope to find successively in the longest and most laborious voyages and travels. In order to render those collections as complete as possible, there should be in all the countries of the world men zealous for their improvement, who should take pleasure in transmitting the particular productions of those parts which they inhabit, to such repositories as they know to be already considerable, and intended to be rendered useful to the public.

That part of Nat. Hist. which can offer to us the largest series of agreeable objects, and actually offers a vast number which are not sought after merely for the pleasure of looking upon them; viz. that part which treats of birds, has remained as yet very imperfect, nor has it yet made them sufficiently known to us, because no considerable collections have hitherto been made of them; and those who had begun to make any soon became weary of going on, having had the mortification to see them every day destroyed by ravenous insects, in spite of all the care that had been taken to preserve them against their teeth. M. *Reaumur* having found easy methods of preparing birds which are intended for those collections, so as to put them out of danger of being spoiled, and to make them look as if alive, has thus found what was still most desired, viz. the means of putting them out of harm's way from greedy insects. He intends soon to inform the public how to render with success this sort of collections durable. He has had the luck to make one, which is already very numerous, and has room to hope that it will be still larger. The birds, for which he is obliged to several learned men, lovers of Natural History, are an earnest to him that he shall owe thanks to them for more, according as they shall find opportunities to procure them

Divers means for preserving from corruption dead Birds, intended to be sent to remote countries, so that they may arrive there in a good condition. Some of the same means may be employed for preserving quadrupeds, reptiles, fishes, and insects; by M. de Reaumur, F.R.S. and Memb. R. Acad. Sc. Paris. Translated from the French by Phil. Hen. Zollman, Esq; F. R. S. N^o. 487. p. 304. Apr. &c. 1748. Read from March 10. 1747-8. to April 27. 1748.

them for him : besides he is sensible how much he may depend on their good disposition to instruct him, for which he is very thankful.

However desirous one may be of sending birds of the country where one lives, to another, where the like are not to be seen, one may be at a loss how to send them on a long journey without their being disfigured or falling to pieces by corruption on the way. I am going to explain here the different means one may have recourse to, for keeping them from corruption, and to make them arrive in a good condition.

The first way. The method hitherto practised to acquaint Natural Philosophers of very remote countries with birds of another country, is to send them stuffed, that is to say, to take off their skin with all the feathers upon it, from the body and the thighs, leaving the legs, the wings, and for the better conveniency the whole neck with the bill sticking to it. Filling afterwards the skin thus taken off with some soft stuff, either straw, hay, wool, or flax, &c. or even stretching it over a solid mould of the shape of the bird, you give to this skin, as near as possible, the form of the body of the bird, which it had when it covered it's flesh and bones ; in which one sometimes succeeds tolerably well, by attention, and some small processes, the particulars of which are not intended here to be entered into.

The second way.

The foregoing way of preserving the shape of birds requires a hand used to it, and even falls short of sufficiently imitating nature, unless with care and time. So it is certainly most convenient only to send the bird as it has been received. There is no great skill required for putting one or several into a vessel full of spirit of wine, or very strong brandy. It has been usual for a long time to make use of those liquors with success for preserving the flesh of dead animals ; and wherefore has this method so seldom been used hitherto to prevent whole birds from corruption ? Perhaps it is because their feathers do not shew those various and bright colours, which are natural to them, whilst they are immersed in some liquor, and which appear no longer on the bird's feathers when taken out of it. Besides, the vanes of the feathers are then disordered, and glued too much together. Upon these first appearances, it was judged too hastily, that spirituous liquors changed the colours of the feathers, and hindered the reducing of them to the order and pliability they had upon the animal, when dry and living. However repeated experiments have made M. *Reaumur* sensible, that the colour of the feathers is proof against the strongest brandy, and even spirit of wine, and that after having dried the bird that had been soaked, one may easily put it's feathers into their natural order, and make it appear as it was when alive :

1. To preserve birds which are to be sent far off, you are only to keep them in brandy ; the stronger it is the better it will be for producing the intended effect : spirit of wine is even preferable. As for the rest, it is indifferent whether the brandy be distilled from wine, corn, or sugar.

2. Though

2. Though the birds may be put into the liquor so as one receives them, yet some small attention is to be had, and some precautions to be used, before they are dipped in, which contribute towards preserving them in a more perfect state. If any of the bird's feathers are bloody, you must wash them from time to time with a wet linnen, till they do not any longer leave a mark upon that linen, or in the water in which they are soaked. Above all it is of consequence to hinder the feathers from taking a wrong bent; or rumpling. It is easy to put them into the shape they are to be, by smoothing them with a finger from the head towards the tail in squeezing them together. This helps the feathers to take the position which is most natural to them, and in this position they are kept by wrapping the bird up in a rag, tying about the neck and the body several times a strong packthread: the feathers on the neck are chiefly those which must be kept from turning aside or backwards.

3. The precaution of taking out of the body the intestines and other parts it contains, is not absolutely necessary; it is better however to do it: if afterwards one supplies their place, by filling the cavity of the belly with all the quantity it can contain of wool, hemp, cotton, or other soft matter; if you fill the neck, though without distending it, with the same soft matter, you will more surely preserve the shape and dimensions of the bird. It becomes less big in the spirituous liquor; not just because the flesh shrinks or dries up, but because the parts which form the cavities endeavour to contract them, and in effect will do so, if the cavities do not contain a matter which resists.

4. After these plain and easy preparations, you are only to put the birds into the vessel containing the liquor which is to preserve them. This vessel may be a jar of glass, if it is only intended for receiving small birds; one may contain a great number of them, which you may put in at different times, accordingly as you get them, till it is quite full. Wooden barrels however are preferable to jars, as they are not liable to break in long journies; there are to be had very small ones for smaller birds, and some large enough for others of the tallest size. The barrel is to have a hole large enough for passing the birds through: this hole can be no other than the bung widened, it will even be better placed in one of the heads. It is unnecessary to advertise that it ought to be kept shut up with a stopple of a proportionable diameter, except during the short time when it is opened for putting the bird in.

5. The birds may be sent in the very jars or barrels where you have put them: but if they are to be on the road for several months, or for years, you will renew the liquor before you send them: that which has been poured on at first, may have been weakened by evaporation, and by the aqueous juices extracted from the flesh.

6. If those birds are not to arrive by sea to their journey's end, if they are to be carried by land for part of their way, one must contrive it so, that they may not be liable to be tossed by much jumbling; and they will

will be less so, if the vessel is so much fuller of them ; they will close the more together. In case they should float too much in the liquor, you need not scruple to press them with hay or some other stuff, which you thrust into the vessel.

7. It is still more easy to hinder the birds from being tossed, and they will even be the better preserved, if before you send them you take them out of the liquor, in which they have lain a sufficient time ; it has made them fit to dry without any danger of corruption. Small birds, such as of the bigness of sparrows, and even of black-birds, after having been covered 8 or 10 days with strong brandy, may be taken out without any fear of their being corrupted. Large birds, and especially such as are very fleshy, are to be kept longer in the liquor ; but there are none or few, for which it may not be enough to have lain in it a month or 5 to 6 weeks. According as you take them out, you must range them one next to the other, and upon one another in a box, filling up the intervals with a matter easiest to be had, as chaff of oats or barley ; that is to say, those small shells in which the grain was wrapped up whilst it stuck to the ear. This chaff is the best stuff for this use : you may also use small hay, moss, hemp, cotton, &c. Far from it's being necessary to leave the birds to dry before ranging them in the box, the best is to put them in quite dripping with the liquor. Having filled the box well, there remains only to shut it up.

8. Any box, of what form soever, may be fit for birds which are to be on the journey only for some weeks or a few months : such as are to travel years, require more precaution ; though they are not subject to corruption, yet they may be torn to pieces before their arrival, if insects greedy of them can come at them, and multiply in their new habitation. One may by care so well close up those boxes, as to render it impossible for those dreadful insects to get to the inside ; paper glued over all the joints will prevent it. But barrels are preferable to boxes, for such birds as are to remain shut up for a year or longer ; the smallest insects will not find a passage for creeping into a barrel, which will not permit the smallest drop of liquor to get out. Birds being put wet into the barrel, keep from drying up too much, and keep one another the closer. As good luck will have it, carnivorous insects are none of those that will pierce wood. So by using spirit of wine or strong brandy, as we just now said, one will succeed in having those birds arrive in a good condition at the remotest places. There is still another way for it, which may appear more convenient, especially for birds of a large size.

The third way Is to preserve birds by a sort of embalming, and even by actual embalming, in countries where the spices are cheap. First, you begin with emptying the body of the bird, and then fill it with those powders I am going to specify to you ; you also fill it's neck with the same powder, thrusting it in through the bill. If the bird is extremely fleshy, you may make an incision in the flesh of the thick part of each leg, and one in the flesh of each wing ; that is to say, two on the breast, and one nearer

nearer the first and large bone of each wing, into which you put the powder ; having afterwards brought the flesh together again, and put the feathers in order, those incisions will be hid so as not at all to disfigure the bird. But there are very few on which it was necessary to make such incisions ; one may make some even inwardly, which will serve as well ; having thrust your fingers into the belly, you may tear the integuments over-against the thick part of the leg, and in other places, and make cavities to be afterwards filled up with the powder.

2. There are many powders proper to produce the principal effect intended here, which is to promote the bird's drying before it be so far corrupted as to occasion the falling off of the feathers. All sorts of spices may be used for it with success ; if there are any in the country which are very cheap, you may use them. You may even make use of a powder composed of as many sorts of spices as you will, the result of which will be at least, that the bird, after being dried, will smell the sweeter, and becomes as it were a piece of perfume. But instead of using resinous gums, as aloe, myrrh, frankincense, and other productions of plants, as cinnamon, cloves, pepper, ginger, &c. which are dear materials, you may content yourself with a salt which is cheap in most countries ; it is sufficient to fill the cavity of the body and of the neck with alum reduced to powder. A material still easier to be had in all places, and very cheap, and which works with great effect, is lime. If it can be had quite unslacked, you will take it preferably ; however, without scrupling to take such as is old, and which has been somewhat slackened by the humidity of the air.

After the body and the neck of the bird have been filled up, either with pulverized lime, alum, or any other powder, you put it into the box or the barrel, in which it is to be transported. You will take care, in placing it, to give a natural position to the neck, neither to give to the legs any other inflexion than they had when the bird stood upon them alive. At the bottom of the box or the barrel there is to be a layer of the thickness of an inch, or thereabouts (if there be more there will be no harm) of the same powder with which the cavity of the body is filled, or of any of those which are proper for it. You bury the bird in this powder, and put enough of it about it and upon it, so as to cover it with a layer of the thickness of an inch or more. The outward powder will make it dry the sooner, and keep off voracious insects, which will not care to attempt to pierce through it in order to come to the flesh they are fond of. During the first days, and even during the first weeks, the birds may cast a bad smell, which you need not be uneasy at, for it will lessen in proportion to the bird's drying ; and it will dry so that none of the feathers will come off ; and when it is once dried, they stick fast to it for ever. This way of preserving birds, which is very simple, has procured to M. *Reaumur* some from very remote countries, which arrived as wished for.

This

*The fourth
way.*

This is one, by which birds are more speedily dried, than by that which is explained before; it is to dry them by the heat of an oven. You make use of that heat which remains in it after the bread is taken out of it; sometimes it is then too great, but there is a plain way to be sure that the degree of heat is not too great, which is, to put feathers into the oven, and to take them out 5 or 6' after; if you find that they are not singed, nor turned red, you ought not to be under any apprehension for the feathers of the bird, which is to be put into the oven. Small ones need remain in it only 1 or 2 hours to be sufficiently dried; those of a middling size require a longer time; and those which are big, and very fleshy, ought to be put in at several times. When they are grown cold, you may know whether they are dried enough, by pressing with the finger the flesh of the legs and of the breast; if it does not yield, or yields but little under the finger, the bird does not any more want to be put into the oven. The inconveniency attending it's being kept there longer than is necessary, is, that some parts of it, as for instance, the neck and the rump, are thereby rendered too brittle. You will prevent the bird's bulk sensibly diminishing in the oven, if, before you put it in, you fill the cavities of it's body and the neck with some soft stuff, like any of those which we mentioned to be used for filling the cavities of such birds as are intended to be preserved by the means of spirit of wine, *viz.* hemp, flax, cotton, &c. What is the most difficult in the way of drying birds in the oven, is not hitting the proper degree of heat, and to know the time how long they are to be kept in it: here will be the difficulty, how, as this way of drying requires the bird may be kept in a natural attitude, before it is put into the oven: if dried, it will be fixed for ever in that which it once received. There are several ways, plain in themselves, for putting and keeping the bird in it's natural attitude, which however would be too long to be explained as to the particulars; the little we shall say of them, will be sufficient to industrious persons for their use. The bird may be kept in order by the means of a frame, made like a farrier's travise; it is composed of a small board, which forms the basis of it, the length of which need not be greater than that of the bird: on each side of this board rises an upright post of wood; these four posts are secured by traverses fixed to them by small nails: the use of those posts and traverses is to keep fixed the small ribbons and threads, which keep the body, the wings, and the neck of the bird in the position it has been brought to. A thread run through the head of the bird, with the help of a needle, enables you to place it as high or low as you please. There are various ways of fixing the feet on the board, with the claws extended; it may be done with small points of nails. With a wire only, and a small board, all may be done as well as with a frame: this wire is run through all the length of the body and of the neck of the bird, by insinuating it through the *anus*; but before doing so, you make a sort of a strong knot to it, by twisting it; this knot is to touch the *anus*; it afterwards

afterwards hinders the bird from sliding: close by the knot you bend down perpendicularly that part of the wire which is without the body, and which is to be at least of a length equal to the height which the legs are to have; you make afterwards it's end pointed by filing, if you have not already done it, and you run it into the board. That part of the wire which then is out of the body, serves for a supporter, which keeps the bird raised, because it is continued to the rest of the wire which runs through the body and the neck: the wire which runs thro' the latter keeps it in it's bending way, and the direction that has been given to it.

Dried birds ought to be sent in boxes or barrels sufficiently closed up, that insects may not slip in during the journey; and you will take care to fill up all the empty spaces left in the barrel with some of those soft stuffs, which we have already pointed out for such uses. Many weeks, nay even months, may pass between the time, when you have dried the first birds you intend to make a collection of for a journey, and that time when they are to set out: this interval is dangerous. There are certain worms, and certain beetles, which are more greedy after those dried in the oven, than after those dried any other way; if they meet with free access, they sometimes seize the first moments to settle under the feathers, or in the bodies, where they multiply.

You will put your birds out of the reach of the formidable teeth of those insects, if after they have been taken out of the oven, you bury them in sand contained in a large box or a barrel. You must take care in covering them with sand, that they may not contract bad attitudes, and that their feathers be not ruffled. Slacked lime reduced to powder, chalk, and all earthy powders, fine and dried, may be successfully employed for the same use. You will press with your hand the surface of the powder, to render the uppermost lay compact, which is very necessary. Lastly, if from the falling of the feathers it appears that the insects have defeated the precautions taken against them, there is still a remedy left; you may stop the progress of the evil by putting the bird again into the oven, not hot enough to singe the feathers, but hot enough to kill the insects in less than $\frac{1}{2}$ an hour.

1. It will not be amiss to send 2 or 3 birds of each sort; and, as near

as you can, let there be one male and one female.

2. One cannot help being curious to know the name which each bird bears in the country where it was taken: you write it with common ink upon a slip of parchment, which you tie with a thread to one of it's feet; the writing will be preserved, even when the bird is in brandy.

3. If you know of a bird any thing besides it's name, you may make a small note, shewing in what places it lives; what it feeds upon; whether or no it stays all the year in the same country; how and where it makes it's nest; how many eggs it lays; the wiles and cunning's particular to it; whether it is good to eat; in short, whatever is known of it's history.

Remarks that are common to the 4 ways of preparing birds.

4. A collection of nests is a proper repository to be joined to that of birds ; it shews such works as hardly could be imitated by men, admirable for their form, their workmanship, and the materials employed in them : M. *Reaumur* has already made such a repository. If one can have nests not too bulky for easy transportation, you may be sure to see them with pleasure joined to the birds that have built them.

5. The colours and figures of the eggs make also part of the history of birds ; collections made of them will give satisfaction to curious minds : those which are to be sent would be in danger of being broken on the way, by the very substance they contain, if it comes to ferment. Before you send them therefore, you must empty them : to this end you make a small hole on each end, and shake them ; and if this shaking will not be enough, you blow into one of the holes to force out through the other what liquid matter remains in the egg.

Quadrupeds.

Quadrupeds that are not of too large a size, and particular to certain countries, may be put into a state fit to be sent to the most remote parts, by one of the 4 ways used to preserve birds : you may make durable collections of them like those of the latter. M. *Reaumur* has begun one, which makes persons who see it wish that there might be more complete ones of the same kind.

Fishes and reptiles.

Fishes and reptiles, which, as well as quadrupeds, are engaging objects for Naturalists, are easier to be sent ; it is sufficient to put them into barrels full of strong brandy. They may also be dried, either by materials with which you may fill the cavities of their bodies, or by a gentle and well-manag'd heat.

Insects.

Insects, which offer to us so many admirable varieties, deserve the care of gathering them into collections, which cannot but be precious to those who have made those little animals their study. All those which are soft, as for instance worms and caterpillars, may be preserved in brandy. Their tender colours will run less hazard of being altered, if you put into the brandy such a quantity of sugar as it is able to dissolve. Beetles may also be put into the same liquor ; but butterflies and flies would be spoiled in it : after having killed them, you must range them in lays in boxes, and separate those lays with beds of cotton. Though one ought to collect in each country, preferably, those which strike most, either by the variety and lustre of their fine colours, or by their size, or by their singular and odd form, or by the use one knows to make of them ; yet you ought not to neglect to collect and send such as do not offer so remarkable singularities, such as even are most common. There are among the latter some, which have wherewithal to satisfy an observer, who looks upon them with other eyes than those wherewith they had been regarded before, and with other views.

An account of a quadruped brought from Bengal, and

XXX. I embrace the present opportunity of viewing and describing this creature, which I cannot find mentioned by any Natural Historian, nor any figure exhibited, in the least, like it. Nor is it indeed to

to be wondered at, since the beast was brought to *Bengal*, from a very remote part of the *Mogul's* dominions; infomuch that no person at *Bengal* had the least knowledge of him. The only hint that seems to point at it is that mentioned by *John Albert de Mandelsloe*, in his voyages through the *Indies*, which are published in *HARRIS's Complete Collection of Voyages and Travels*, N^o. 52. p. 775. where he says, that among the horses in the stables of the *viceroy* of *Goa*, he saw "a beast called a *biggel*, a creature much about the colour and bigness of a rain-deer: it's head like that of a horse; it's main like that of an afs, with black cloven feet, and 2 black horns on his head." This is the whole of his account, which is so imperfect, that it can hardly be thought absolutely to mean this very beast before us. The following description and drawing I hope will be found pretty exact; and may serve to shew with which *genus* of quadrupeds he may be classed, and consequently, what proper name may be given him; which I submit to this learned *Society* to determine.

now to be seen
in London:
presented by
James Parsons,
M. D. F. R. S.
N^o. 476. p.
465. Apr. &c.
1745. Read
June 27. 1745.

Fig. 44.

The creature is a male, having the *penis* and testicles like those of a deer; but, as the *penis* does not come very forward, it cannot be seen in a side view of the animal. The head is formed like that of a deer, with a rhomboidal spot of black hair on his forehead; his ears are dark without, and yellowish within, with dark spots toward their edges; and the horns rise about 7 inches high, bending forwards; which is very particular, because those of all other horned animals are directed sideways or backward, except the brow-antlers of some kinds of deer. He keeps his ears in continual motion, which is an action common with deer, and butts with his horns as they do. His eyes are black and lively, and the *riētus oris* is long. His neck bends forward like the deer kind, but is thick and strong, somewhat resembling that of a male deer in rutting time. His mane is thin of hair like that of an afs, and on the convex part of his neck forward he has a tuft of black hair. His shoulders are thick, and his breast pretty broad and strong, from which a piece of loose skin hangs like the *dewlap* of a cow. His legs are slender, with cloven hoofs like those of a deer. His back rises, directly over his shoulder, pretty high, upon which the mane, continued from his neck, ends in a tuft of hair. From the back of this *bunch* or rising, his posterior parts resemble those of an afs, having a tail like that of the afs, only it is flat on the side next the animal, and convex on the back. It is about 22 inches long, and ends with some long hairs. He is of light ash-colour, having a smooth coat of short hairs, which grow darker, inclining towards a black, upon some parts of his limbs. He has some white under his belly towards his breast, and under his tail, with white testicles. He feeds on hay, grafs, or any kind of greens; and, being tried whether he would eat raw flesh, refused it. His keeper says, when he lies down he chews the cud; and his excrements are like those of a deer. He is about twelve hands high to the top of the bunch in his back. His

keeper says, he never lies down on either side, but directly upon his limbs like the *camel*, and that he rises as suddenly as that beast.

There is something very particular in his voice, which imitates the creaking noise of a child's rattle, or the croaking of some birds, rather than the voice of any quadruped except the deer, who, I think, exhibits something like it in rutting time.

A natural
Hist. of the
mus alpinus,
or marmot;
by Jac. The.
Klein, Sec. to
the Repub. of
Dantzick, and
F. R. S. com-
municat'd by
Mr Collinson,
F. R. S. N^o.
486. p. 180.
Feb & Mar.
1748. Read
Feb. 25.
1747-8.

XXXI. The *mus alpinus* is so called from the highest mountains of Switzerland, abounding with herbs and grass, on which it dwells. In Savoy they are called *marmota*; in Germany, *murmeltiere*; in Ukrania, *Podolia* and Russia, *bobasci*; and on the Carpathian mountains, *swiszez*; whence the valley of *Swiszez* is called by the French *Rats des Alpes*.

They are something bigger than a tame cat: the younger ones are reddish; but those which are full grown are of a brownish colour; their hair is stiff; their feet short; their head narrow; their nose in a manner divided; and they have whiskers like a cat; they have teeth like a squirrel, or rather like those of a beaver: their neck and back are broad and thick; their ears short and round, as if they had been cut; their eyes are full and prominent; their tail is like that of a dormouse, flat and hairy, a span and a half long; their toes and nails are like a squirrel's, but stronger.

When they are at liberty, they feed upon herbs, grass, roots, and sheathed insects and grasshoppers: when they are tame their food is various; they love any thing made of milk, but they will eat flesh, bread, fruits and such like, taking up with their fore feet as squirrels do.

They breed 3 or 4 young ones at a time: from autumn till spring several families of them sleep together in a little straw or hay, in caverns formed like a Y, well arched and closed up: when the weather grows warm, they awaken, open their doors, carry out their dung, go out to feed and copulate, in which they are very wanton, playing together like the field mice, skipping about, and running up the trunks of trees; sometimes they run upon their hind legs, and make a noise like young puppies, or the sound of a shrill pipe.

The circulation of their blood is very slow in winter; as are all their secretions also; they have no return of *serum* or lymph, so that their blood is at length entirely deprived of *serum*. The *omentum* and neighbouring intestines are very thick. They cannot be said to ruminate; for they have a single membranous stomach, though when they are at liberty they feed upon herbs. Towards the *cæcum* they have annular connivent little valves, stretching as it were into branches, as does the entrance of the *ileum* between the 2 annular coats; so that the excrements cannot return to the small guts; but are collected in the *cæcum* during their time of sleeping, and there continue till spring.

Concerning
the natural

XXXII. Since the complete and full demonstration of the circulation of the blood in animals by Dr *Harvey*, the generality of medical writers



Fig. 44.

writers have attributed the natural heat of animals to the motion of the blood in the blood-vessels, or rather to an attrition of all the fluids in the animal arising from it; which fluids, from the later discoveries by injections and microscopes, are found to move in conical canals communicating one with another near the *apices*, or where the arteries are the narrowest, soon afterwards growing wider and wider, when the same continued canals obtain the name of veins, and convey back the fluids they contain to the heart. They ascribe heat in an animal to strong and frequent contractions of the heart and arteries; which heat * will be the greater, the more dense the humours are, the more strongly they are propelled, and the greater the resistances are, near the ends of the arteries. From this supposition they conclude, that the heat arises from attrition; that, by a violent agitation of the particles of the blood and humours against one another, and especially by the attrition of them against the sides of the containing blood-vessels, there must be great friction excited, and from that friction heat generated; as is easily done by rubbing two pieces of wood together, or a piece of wood and a piece of metal, or two pieces of metal, or hard stones: but it is known, by daily experience, that either any watery fluid, or oily or greasy substance, applied to these bodies while rubbed, will prevent the excitation of heat; as for instance, the use of water in polishing of glass or marble, and the greasing or oiling all manner of wheel-machines, many of which, for want of that application, have heated, taken fire, and been even consumed in flames of their own exciting. I know of no experiment, whereby it appears, that any the least degree of heat has been generated by the simple or mere mechanical agitation or friction of the particles of any fluid, either by itself, or mixed with various fluids; water, wine, vinous spirits, oils, quicksilver, either agitated singly or mixed, will by no force, or velocity of motion I ever heard of, produce heat; nor can the blood of animals, when once let out of the body, be kept either fluid or warm by any the most violent agitation. Indeed heat is generated in fluids in some particular circumstances, as in those two so commonly known cases of *fermentation* and *effervescence*; which, as they are frequently confounded by persons not thoroughly versed in chemical matters, I shall beg leave to explain the difference. *Fermentation* is that spontaneous intestine motion, which, in the degree of heat of the universal temperature of subterraneous caverns, will, in a few hours, bring on such a change in vegetable juices, or in water charged with a strong tincture of vegetable particles (for fermentation is confined to the vegetable kingdom solely) as from a vapid must or wort quenching fire, to make it become more or less inflammable and nourishing of fire, as it is impregnated with more or fewer of the vegetable particles, and in the alembic to afford that volatile subtil inflammable liquor commonly called vinous spirits. The heat produced by fermentation never

heat of animals; in a letter to M. Folkes, Esq; P. R. S. from Cromwell Mortimer, M.D.R.S. Sec. N^o. 476. p. 473. Apr. &c. 1745. Read July 4. 1745.

* Boerhaave Instit. §. 968.

exceeds that of the human body. *Effervescence* arises from an intestine motion, to be excited in various sorts of fluids; either by the mixture of fluids with fluids of different natures together, or by dropping in salts or powders of different natures into different fluids: the two most common opposites, *acids* and *alkalies*, on being mixt, cause a great ebullition or frothing, but no great heat; but the solutions of some metals in *aqua fortis* cause intense heat, and emit flame: the mixing aromatic oils with acid mineral spirits actually kindle, and burn with violent explosions; and some vegetable substances, putrefying with moisture, will sometimes heat so, as to kindle what lies dry above that part of the heap where the putrefaction happens. Thus dung-heaps will heat, and haycocks often kindle into actual fire.

In these cases of effervescence, as there is no adventitious heat or fire applied, there must be the elements of fire lying hid or dormant in one or other of the bodies; and it is sufficiently known, by experiment, that there is abundance of air lies dormant in all bodies both solid and fluid; and it is likewise known, that fire cannot exert itself without the elastic assistance of common air; for wood will not burn, nor even gunpowder fire, in the artificial *vacuum*. It being therefore granted, that there are the elements of fire and of air lying dormant in all bodies; there is only required such an action as may set at liberty the particles of air, and the particles of fire; by which action the particles of air will recover their elasticity, and, putting the particles of fire in motion, cause heat or warmth, but not incension or inflammation; unless the fire thus agitated meets with a proper *pabulum*, which *pabulum* is *sulphur* only, though differently modified, whether under the appearance of brimstone, *bitumen*, oil, vinous spirits, vegetable substances when deprived of their water, metalline sulphurs, or the most inflammable of all, animal sulphur, commonly called by our modern Chemists *phosphorus*.

Thus in fermentation, the fire and air being let loose, produce a warmth, but do not kindle, because of the water predominating; whereas in the effervescence produced by the solution of metals, the fire meets with the metalline sulphur, which it kindles, and sometimes causes explosions; the aromatic oils containing but little water, being almost entirely composed of the sulphureous parts of the vegetables, immediately kindle, and break out into flame; and *phosphorus*, which is nothing but the animal sulphur, as appears by the curious account of it given us by the late Mr *Godfrey*, is so greatly disposed to take fire, that if it be only exposed a few minutes to the open air, it kindles and flames.

Now all animals, on which experiments have been made, are found to contain more or less of the phosphoreal principles; some insects constantly shine, or emit light, in the open air; many sorts of fish are luminous, if exposed to the air a short time; nay even the bubbles of the sea-water appear like fire in the dark: some quadrupeds have been observed to emit light on very slight friction of their hair, as the necks of horses, the backs of cats, and the like; and there are many instances in

in our own species, of many parts of the body appearing luminous, and even of the exhalations from it adhering to the cloaths, causing them to shine likewise; of which several curious observations have lately been laid before this *Society*: these, I think, are convincing proofs of *phosphorus* existing, at least in a dormant state, in animal fluids; and as it is likewise certain, that they all contain air, it is only necessary to bring the phosphoreal and aëreal particles to contact, and heat must of consequence be generated; and was it not for the superabundance of aqueous humours in animals, I do not doubt, but fatal incensions would frequently happen. This, I think, explains evidently the cause of animal heat: indeed the heart and arteries are the instruments which excite this heat; but that is not done by the friction caused by the circulation of the humours, but only by the intestine motion, which the circulation gives to the several particles which constitute the mass of animal fluids; and as the velocity of these fluids is increased, so must the different particles, of which they consist, come oftener into contact; and, consequently, the oftener the phosphoreal and the aëreal meet, the more frequent and greater must the *nîsus's* be to create heat.

Hippocrates (*Aph.* I. 14.) mentions the θερμὸν ἐμφύλον, *calidum innatum*. *Galen* takes it for the *soul*, and more modern writers have supposed it to be the very *spirit*, the *archeus*, and others the *vital heat*; but have all treated of it as a certain degree of fire existing in animals; not having any notion, that the element of fire might be absorbed, or lie latent, in fluid bodies, ready to become active as soon as it meets with air, or even to kindle, if it meets with sulphureous particles under proper circumstances. This I fancy the Antients, in the very earliest ages of the world, had some notions of; when they thought proper to communicate to the vulgar some shadows only of more profound and real knowledge under types and fables, as handed down to us in the fictions of the poets: of this kind, and quite to our purpose, I take the fiction of *Prometheus* stealing fire from Heaven to animate his men with, to be one. And, I think, upon this principle of *phosphorus* existing in animals, one may easily explain the cause of those melancholy accidents which have happened to some of the human species, as that of the lady at *Cesena* in *Italy*, the Carpenter in *Hampshire*, and the woman lately at *Ipswich*; who, it is most probable, were all set on fire by lightening: it may be said, many are struck by lightening, but not set on fire; but it is to be remarked, that the lady at *Cesena* had charged all her pores and absorbent vessels with a great quantity of camphire; the woman at *Ipswich* had drank plenty of gin; and as for the Carpenter, that circumstance is not recorded of him, whether he was a hard drinker or not; which circumstances must greatly promote the kindling the phosphoreal fire in them; and, as this *pabulum* was conveyed into the most minute capillary vessels, might produce an almost instantaneous deflagration and dissolution of all the solid containing parts.

A large stone found in the stomach of a horse.

Animals appearing more susceptible of electric fire than other bodies, greatly confirms my conjectures of the phosphoreal principles; and I should think, that being rendered electric to any high degree might prove a dangerous experiment to a person habituated to a plentiful use of spiritous liquors, or to embrocations with camphorated spirit of wine; on the contrary, in some languid, cold, or worn-out constitutions, possibly, future experiments may evince, that electricity may be used medically, in order to renew and regenerate a proper quantity of vital fire, such as is necessary for the conveniently carrying on, and performing the animal functions.

Concerning a large stone found in the stomach of a horse; in a letter from W. Watson, F. R. S. to Francis Wollaston, Esq; F. R. S. N^o. 475. p. 268. Jan. &c. 1745. Read Jan. 31. 1744.

XXXIII. 1. I send you a few observations upon the *calculus* you favoured me with the examination of. You informed me, that it was found in the stomach of a coach-horse in *July* last; and that it then weighed 3 pounds $2\frac{1}{2}$ ounces *averdupois*. You likewise mentioned, that the poor creature was observed frequently to be in violent pain; and would sometimes eagerly turn his head to one of his sides, and sometimes to the other, as though he endeavoured to bite out that which annoyed him; and that he died, after having taken various remedies, which the Farriers administered. When I weighed this stone about 10 days ago, it's weight was 2 pounds and $\frac{3}{4}$ of an ounce; so that, in about $\frac{1}{2}$ a year, it had lost 1 pound 1 ounce and $\frac{7}{8}$. The figure of it is spheroidal, as these sort of stones generally are; it's periphery $17\frac{1}{4}$ inches by $16\frac{1}{2}$, which are very near the same dimensions this stone had when first found. The surface of it irregular, somewhat resembling the inequalities observed upon the surface of the brain; all the projecting parts of which are polished, from their friction against the sides of the stomach. It is of a dark-brown bilious colour, and very like to a species of *pyrites*; inasmuch that, unless it were taken in the hand, whereby their specific gravities may be determined, it might pass for one of that family; although this stone is by much the most specifically heavy I ever saw of this sort.

I imagine, that, a considerable time before the horse died, by some accident this stone received a great blow; for there appears to have been a piece broken out; and there are two large cracks not yet filled up; near which terminate several concentric circles. This stone seems to involve a smaller one, altho' no-where perfectly separated from it; but the outward is by much the hardest. In the centre are two holes, in which may be seen several hairs of the horse; but I have not been able to find any other extraneous body, upon which these *calculi* are usually formed.

Having, from sawing the stone, a quantity of it's powder, I was induced to an inquiry into it's constituent parts by way of analysis.

I first let fall 2 small pieces of this stone into water almost boiling: they immediately sunk, but arose again, and continued alternately rising and sinking a considerable time. This was occasioned by the quantity of

of air-bubbles, which the heat rarefied ; but the air was detained by the *mucus*, which seemed to connect the particles of the stone together ; and which, though diluted by the hot water, was tenacious enough to form bubbles of size sufficient to buoy up the pieces of stone ; the rarefaction growing greater, the bubbles burst, and the stone fell to the bottom ; but arose again, in like manner, at the expulsion of more air. The learned Dr *Hales* likewise found great quantities of air in the human *calculus*.

I infused 3ij of this powder in 3ij of boiling water : this infusion I filtered when cold. It was of a light brown colour, and of a bitterish saline taste. I calcined what remained of the powder after the infusion, till the whole was black, and then it weighed 3j gr. iij. I made the following trials with the infusion.

1. Mixed with syrup of violets, it became green.
2. With oil of tartar, the colour was deeper without ebullition, but the mixture sent forth immediately a strong urinous smell ; the same smell arose from rubbing some of the powder with oil of tartar.
3. With oil of vitriol, and spirit of salt, it lost it's colour ; but no ebullition ensued.
4. With a solution of sublimate in water, the mixture curdled, and let fall a light-grey sediment, leaving the liquor quite transparent.
5. With a solution of sublimate in lime-water, the mixture grew turbid, and let fall a deep-yellow sediment, in a much greater quantity, and of a deeper colour, than a solution of sublimate and lime-water alone.

From these inquiries it appears, that the stone is compounded of an earth, air, *mucus* of the stomach, and a saline principle bearing great resemblance to *sal ammoniac*.

2. The horse in which the large stone was found, belonged to a miller in this neighbourhood, and had been fed with bran only for several years. He was observed to be in pain sometimes, but never so bad as to be hindered from his work, till the day of his death ; when he was taken on the road with symptoms of violent pain, and wanted to lie down : however, the carter drove him home ; but, as soon as he had unharnessed him, the poor creature was seized with a great shaking, and dropped down dead immediately. The man who stripped him, observing a swelling in his belly, opened it, and found in the *colon* a very large stone, but presently broke it in pieces. I did not hear of this stone till the latter end of last summer, when a Gentleman shewed me a fragment of it ; which excited my curiosity to go to the mill where the horse died, to inquire for the remains. I found several pieces of it, weighing in all 16 i. 3vjss Troy weight. Some of them have been kept dry in the mill, but the greatest part laid abroad mixed with rubbish ; which, though exposed to the weather above 12 months, was not much altered, being only a little more brittle than the rest, and somewhat mouldered on the outside. About a fortnight ago, another piece of the same stone was

Of a very large stone, found in the colon of a horse ; and of several stones taken from the intestines of a mare ; with some exp. and obs. by Edw Bailey, M. D. of Havant in Hampshire. N^o. 481. p. 296. Oct &c. 1746. Read Dec. 11. 1746.

brought to me; weighing about ℥ viij Troy , containing near $\frac{1}{2}$ the *nucleus* and the innermost *laminæ* cohering together.

From all these fragments, and the description of the stone given me by those who saw it, before it was broken in pieces, it appears to have been of a spheroidal figure, about 16 inches in circumference, consisting of a *nucleus* and several *laminæ* or shells involving one another; some of them are parted from each other, but the rest stick so close together, that they cannot be separated without breaking. All the *laminæ* are composed of transverse *striae*, with their points converging like rays towards the centre of the *nucleus*. They are of a brown colour, and shine like resin. The *nucleus* is of an oval figure, and differs but little in its composition from the rest of the stone, having no other extraneous matter in it but a few pieces of straw, and small sticks, like the twigs of a broom; some of them appear intermixed with the *striae* throughout the body of the stone. The external surface of the stone, and those *laminæ* that have been exposed to the air, look of an ash-colour, are pretty even, but not very smooth, having many small holes in them. See Fig. 45, 46, 47.

About the beginning of last July, 5 large stones were found lying near one another in the intestines of a mare, which belonged to a carrier in this town, who had used her several years in his team. She was in good case, and always appeared to be sound and healthy; till one morning, being at grass, she was found lying on the ground in a great agony of pain, with which she was continually tortured for about 6 hours, without any relief from various remedies which were applied: at last she got up, and ran about the field like a mad creature, till she died.

I have seen but 2 of the stones which were taken from her; one of a triangular shape, the other oblong, a little depressed in the middle, bearing some resemblance to a horse-bean. (See Fig. 48, 51.) They are both similar in substance, and seem to be of the bezoar-kind, being of a closer texture than that above described, of an olive-colour, and finely polished.

The other 3, as I am informed, were of the same colour and texture, and one of them larger than either of these, and of the shape of that at Fig. 51.

Experiments. These 2 stones, being sawn asunder, looked like polished marble; and were found to contain a piece of an iron nail in the middle.

The triangular stone weighed gr. 75 above ℥ iijss Troy . The other, being the larger, weighed but $2\frac{1}{2}$ gr. short of ℥ xvi Troy weight.

A fragment of the lesser stone, which in the air weighed gr. $103\frac{6}{10}$, in water weighed gr. $42\frac{6}{10}$; so that this stone is in specific gravity to water as 170 to 100.

A fragment of the greater stone, which weighed in air gr. $83\frac{7}{10}$; weighed in water of the same degree of warmth, gr. $34\frac{7}{10}$; so that the specific gravity of this is the same as of the other.

A quan-

A quantity of the larger stone, weighing four ounces, being distilled in a coated retort, yielded gr. $37 \frac{4}{10}$ above ℥ij of a strong alkaline spirit, of a brown colour, such as is drawn from hartshorn; leaving a black coal weighing gr. $74 \frac{1}{10}$ short of 2 ounces; gr. $36 \frac{7}{10}$ being converted into air, and otherwise lost in collecting the produce of the distillation; a small quantity of black oil adhered to the neck of the receiver, and a few drops of this oil appeared in the spirit, when it was first poured off; but, after standing some time, fell to the bottom in the form of a black sediment. The black coal, calcined under a muffle in a very strong fire, lost but gr. 22. and became a white insipid earth.

A fragment of the great stone, which had lain exposed to the air and weather above 12 months, as mentioned above, weighing in the air gr. 58, weighed in water gr. $24 \frac{4}{10}$, after it stood a considerable time, that the water might enter it's cavities: so that this stone, tho' seeming of a loose texture, came out not much inferior to the other in specific gravity; this being to water as 165 to 100.

A portion of this stone, in a strong open fire, lost in calcination just $\frac{1}{2}$ it's weight; becoming, as the former, a white insipid earth; which, being infused in boiling water, made no alteration in it's colour, taste, nor smell.

℥iij of this stone distilled, produced gr. $24 \frac{1}{2}$ more than $\text{℥i} \frac{1}{4}$ of the like alkaline spirit as the former yielded, and left a black coal, weighing gr. $16 \frac{7}{10}$ above ℥ifs .

From this chemical analysis it appears, that these stones are compounded chiefly of earth, a large quantity of volatile alkaline salt and water, some oil, and a small quantity of air. From hence it likewise appears, that the component principles of these stones bear a nearer resemblance to those of hartshorn than that of the *calculus humanus*: for, according to Dr Hales's account, in his *Vegetable Statics*, Exp. N^o. 51. gr. 241 of deer's horn being distilled left a *calx* weighing gr. 128; viz. above $\frac{1}{2}$ it's weight; which shews that horn contains much about the same quantity of earth as these stones do: whereas the *calculus humanus*, when distilled, affords but a small quantity of earth, spirit, or oil; the greatest part of it being converted into air. ℥fs of the stone (*Fig. 51.*) being powdered and infused in ℥iv of boiling water, made it smell strongly of horse-dung, and gave it a disagreeable taste, while it was hot; but, when the infusion grew cold, it lost it's taste and smell, and the water, after standing some time, became, without being filtered, as pale and clear as before; nor did the mixture of it with oil of tartar, oil of vitriol, nor *aqua fortis*, produce any alteration in it. This experiment, being repeated several times, by infusing boiling water on the same powder, was attended with the same effects, tho' near 2 months passed between the first and last infusions. The powder, after the first infusion, appeared like mud of 2 different colours and consistencies; the upper part being softer, and of a lighter colour, the under of a dark-brown,

feeling hard like sand. This difference in the colour and texture of the sediment remained in all the infusions.

A small piece of the same, and another of the biggest stone, being let fall into boiling water, sunk immediately, and continued at the bottom, without rising at all, though the water was kept boiling a considerable time: which shews, that these stones are specifically heavier than the stone found in the stomach of a horse, which Mr *Watson* gives an account of *; and also, that their constituent principles are more firmly united together than those of that stone; 2 pieces of which, being let fall into water almost boiling, immediately sunk, but rose again, and continued alternately rising and sinking a considerable time: and, as that gentleman observes, the powder of that stone being infused in boiling water, the infusion, when cold and filtered, was of a light-brown colour; whereas the colour of the water was not changed in either of the above-mentioned infusions; neither did any ebullition ensue upon the mixture of them with oil of tartar, vitriol, &c.

I tried to dissolve these stones, by digesting small pieces of them in the strongest acid and alkaline *menstrua*, viz. spirit of salt, sulphur, oil of vitriol, *aqua fortis*, and capital soap-lees, &c. and at the same time tried the effects of those *menstrua* on several stones, which were given me by some of my patients, who voided them. They were softened by some of them, but not totally dissolved by any, except the oil of vitriol and *aqua fortis*: nor did the *aqua fortis* cause any ebullition in dissolving them, as it did in the solution of the *calculus humanus*, which was attended with a brisk ebullition, and hissing noise, arising from the eruption of the air-bubbles from it: which confirms what I observed above, that these stones contain but a very small quantity of air, and that their saline and oily particles are so closely combined with earth, as not to be extracted without a strong fire.

Explication
of the figures.

Fig. 45.

Fig. 45. represents a fragment of the stone found in the horse, containing part of the *nucleus* and the adjoining shells or incrustations; and *a*, The *nucleus* or center. *b b*, The first stone or incrustation formed upon the *nucleus*. *c c c c*, The second incrustation, covered with a smooth shell or coat *d d*. *e e*, The third incrustation, which appears through the broken shell of Fig. 46. *f f f f*, The smooth shell or coat of this incrustation, which forms the outside of Fig. 46. *b b b b*, The 4th or outermost incrustation, consisting of 3 layers or shells.

Fig. 46.

Fig. 46. shews the external surface of the 3d incrustation. *f f f f*, The shell almost intire, but broken off at *g g*. *e e*, Part of the third incrustation.

Fig. 47.

Fig. 47. shews a fragment consisting of part of the 3 outermost shells of the fourth incrustation. The letters *f f* and *b b* answer to those of Fig. 45. and Fig. 46. only the concave part *f i*, *f i*, is what fitted and joined to the round surface of Fig. 46.

* See the preceding paper.

The following figures shew several views of the two stones found in the mare.

Fig. 48. represents the triangular stone.

Fig. 48.

Fig. 49, 50. represent the same sawn asunder, shewing the divisions of the several shells, with the *nucleus* and the end of the iron nail projecting from the centre.

Fig. 49, 50.

Fig. 51. exhibits the larger stone intire.

Fig. 51.

Fig. 52. part of the same sawn off, representing the several shells, with one piece of the iron nail in the middle.

Fig. 52.

Fig. 53. the other piece of the nail that was sawn off.

Fig. 53.

N. B. One of the three stones mentioned above is lately come to hand ; it measures round 12 inches one way, and 11 another way. This stone, together with all the above-mentioned, and the several chemical preparations produced from them, are repositied in the *museum* of his Grace the Duke of Richmond.

3. It is now fourteen years since I took the stone out of the bladder of a very large mastiff, about 5 years old, belonging to the porter of his Majesty's dock-yard at *Portsmouth*. The dog died in about 3 days after receiving a kick from some one endeavouring to part him from another mastiff he was fighting with.

Concerning a stone taken out of the bladder of a dog ; which being cut a-

When I had opened the *abdomen*, I found it filled with bloody urine ; and having before heard that his death was supposed to be occasioned by the kick, I immediately thought the bladder must be the part hurt ; which, when I had cleansed the *abdomen*, I examined, and found this large stone, with the bladder contracted close to it on every side, and rent at the bottom about $\frac{3}{4}$ of an inch ; so that what urine came to the bladder was discharged into the *abdomen* ; which was plainly the cause of his death.

sunder had a piece of dog-grafs in it's center ; in a letter from Mr W. Fidge, Surgeon, at Portsmouth, to C. Mortimer, M. D.

When I first took it out it weighed 3x 3ijfs . It is not more than 2 months since I cut it asunder ; when, finding it formed upon (as I imagine it is) a piece of dog-grafs, I thought it would not be an unacceptable present to the curious ; therefore, having some affairs which called me to town, I brought it with me for that purpose.

Sec. R. S. N^o. 482. p. 335. Jan. & Feb. 1747. Read Jan. 8. 1746-7.

What is to be farther remarked is, that I did not find any the least particle of gravel or sand either in the kidneys or ureters ; and that all the bones (except the ribs and *cranium*) are more or less carious, as the bones you have with the stone.

XXXIV. I here send you to be communicated to the *Society*, if you think proper, an account of two pretended stones, said to be found in the head of the most venomous snake of the *East Indies* called *cobra de cabelo*, together with an account of what I have heard, and what I believe they really are. The first I have heard and do believe to be a stone found in the stomach or intestines of the rhinoceros ; not, that I know, taken notice of by any Natural Historian, excepting *Redi*. The

A letter from Sir H. Sloane, Bart. late P. R. S. to M. Folkes, Esq; P. R. S. containing accounts of place

the pretended
serpent-stone
called pietra
de cobra de
cabelos, and
of the pietra
de mombazza
or the rhino-
ceros bezoar,
together with
the figure of a
rhinoceros
with a double
horn. N^o.
492. p. 118.
Apr. & c. 1749.
Read April
20. 1749.

place where it is said to be found is on the S. E. coast of *Africa*, according to the information *Redi* had of it, and from which place I had the two horns figured in these *Transactions*, N^o. 470, by Dr. *Parsons*, which were tied together across, the better I believe to preserve the short skin that connected them on the nose of that animal, so that the strait and crooked horn might appear distinct, as they do in a very entire small brass medal of *Domitian* in my collection.

Whether the rhinoceros, who bore these 2 horns, be a distinct species of that animal from that of *Asia*, future travellers must determine.

These horns were given me by *Cb. Lockyer*, Esq; who was (as I have been told) sent in a ship of strength with a power given him by the *East-Indian* and *African* Companies, to go on their affairs to that unfrequented coast which common travellers have been afraid to go to because of the barbarity and cruelty commonly said to belong to it's inhabitants, and with which the *Egyptians*, and from them the *Greeks* and *Romans*, had a greater intercourse and knowledge than with the southern parts of *Asia*, where that animal is generally found with only one horn.

Pietra de serpenti di Mombazza Redi esperienze, Nat. p. 59. Tab. II.
Lapis serpentis de Mombaza, edit. Latin. p. 82.

Dr *Waldo* went into the *E. Indies*, on purpose to search after and collect the natural productions and curiosities of those parts, especially such as related to the cure of diseases, which he sent from time to time to his sister in *London*, with directions to shew them to the E. of *Pembroke*, Sir *Godfrey Kneller*, and myself, to sell. The two former not caring to buy several of them, they fell into my hands. Among the rest which I purchased were some of these stones, which were by him called *Rhinoceros-Bezoars*, which I supposed were taken out of the stomach or guts of that large animal.

These productions or bezoars, as they are commonly called, consist of several coats made up of several parts attracted by their centers, such as the stones of fruits, and other indigestible substances swallowed with it's food, after the manner of those found in the stomach and intestines of mankind, and other animals. The uppermost coat or layer of this bezoar is made up of several brown striated small knobs or tubercles something like low warts, distant from one another, and making it's outermost surface very unequal, as well appears by *Fig. 54*, and *55*. different from the other bezoars whose surface is generally smooth. Those I have of this bezoar are of different sizes and diameters, the largest about the bigness of an orange, heavy, and as hard as stone, and capable of being polished.

Redi relates great virtues belonging to them, as told by the bringers of them from the *East Indies*; such as, being tied to the hip or leg of a woman in travail, it helps her delivery, and without pain, even if the child

Fig. 45.

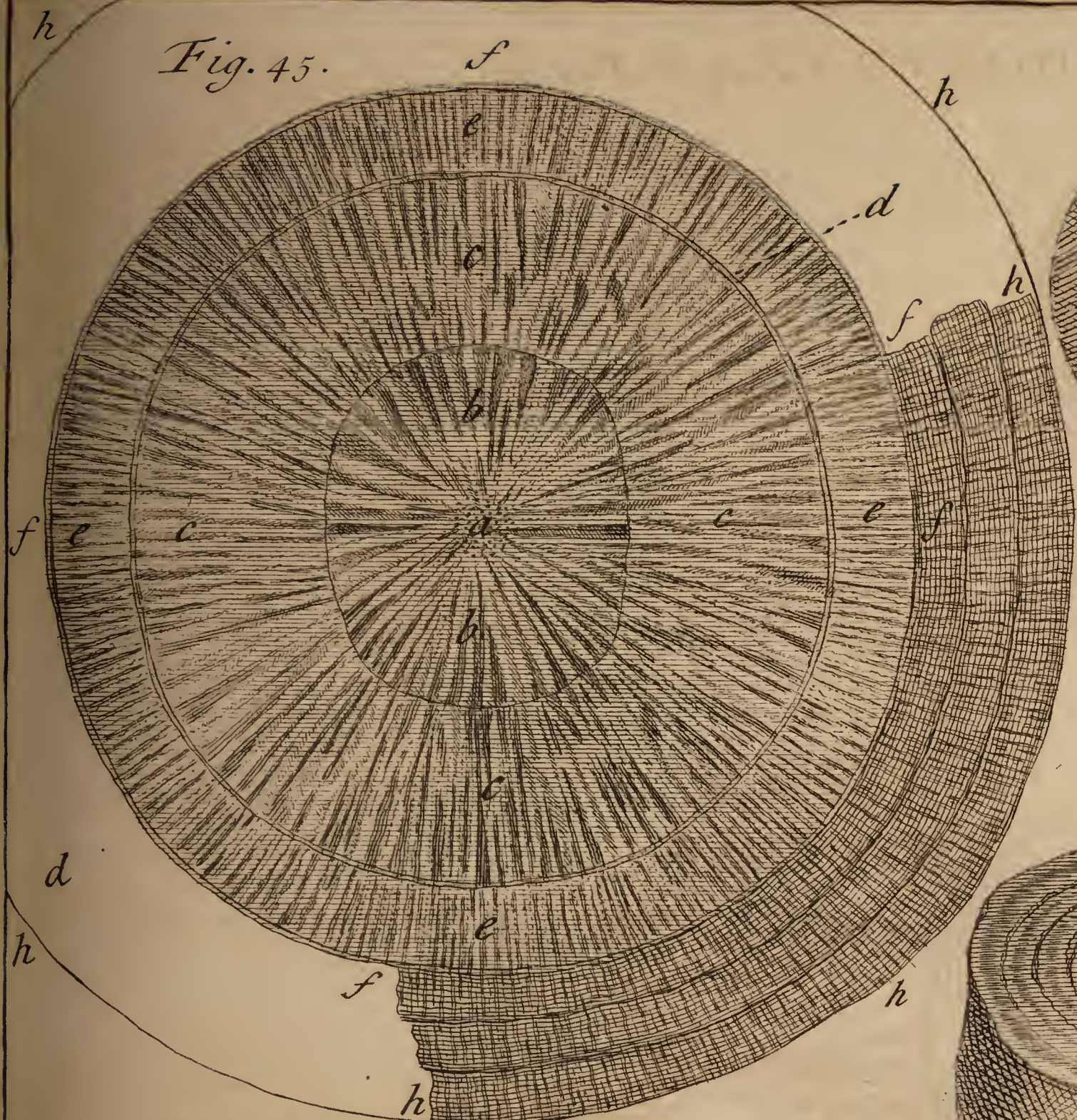


Fig. 48.

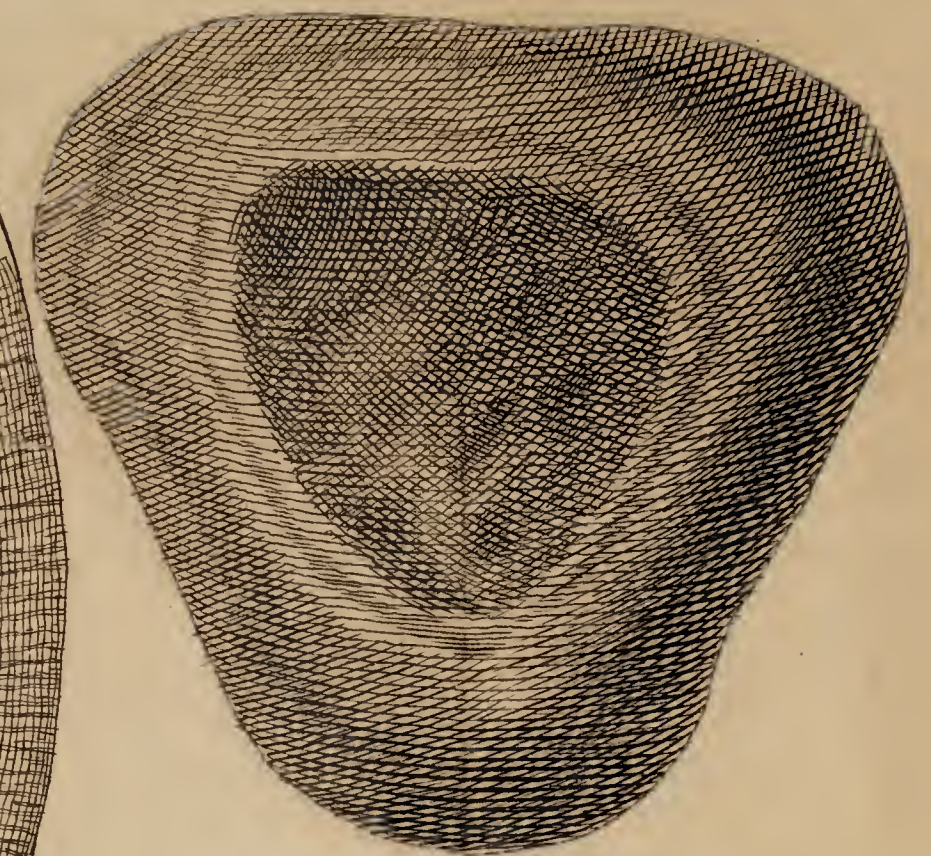


Fig. 53.



Fig. 51.

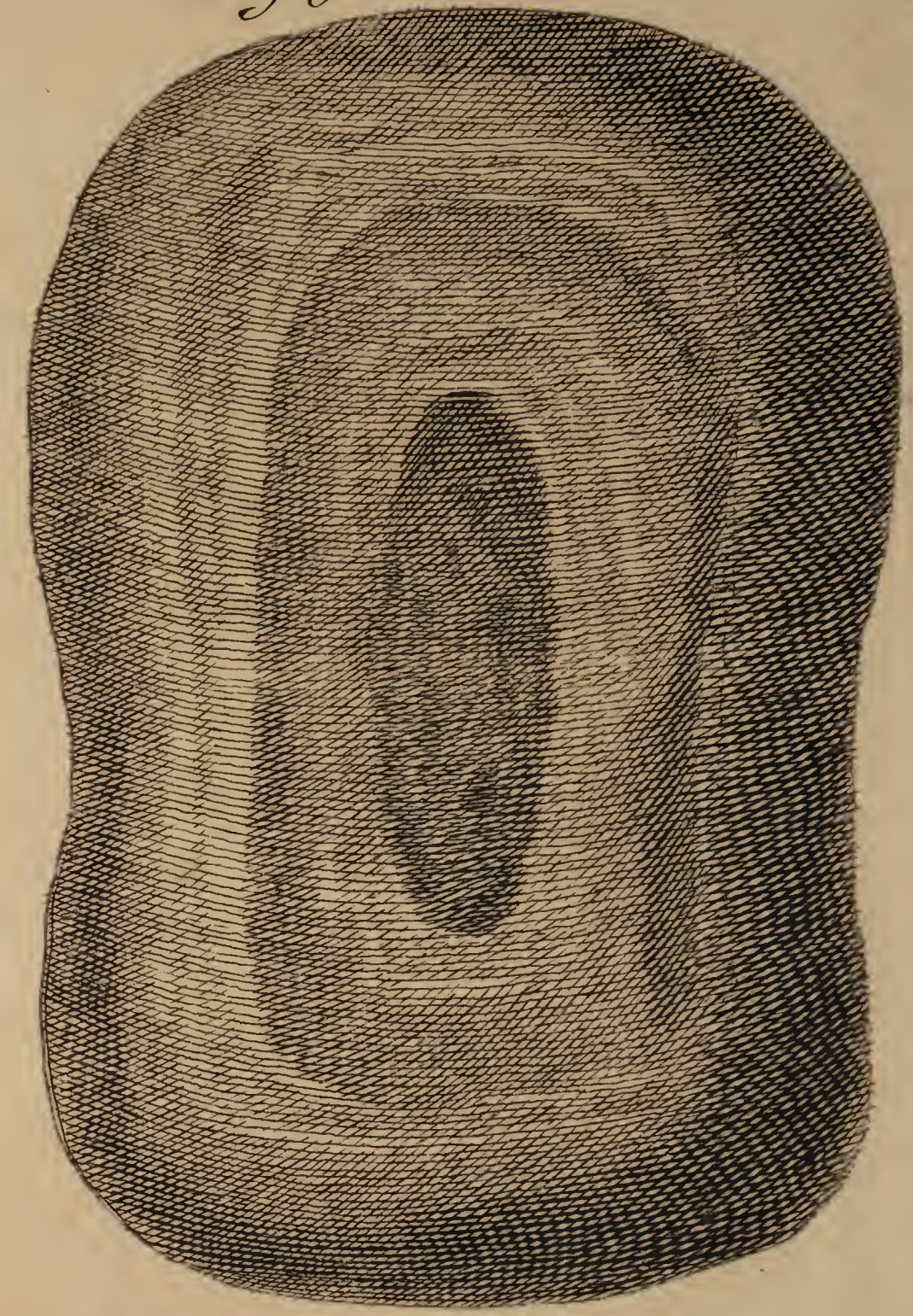


Fig. 49.

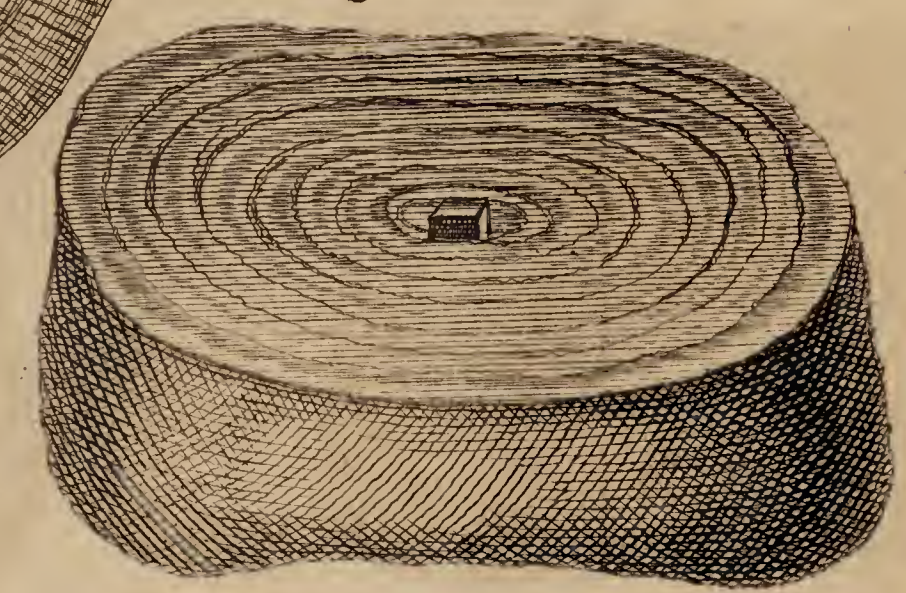


Fig. 50.



Fig. 46.



Fig. 47.

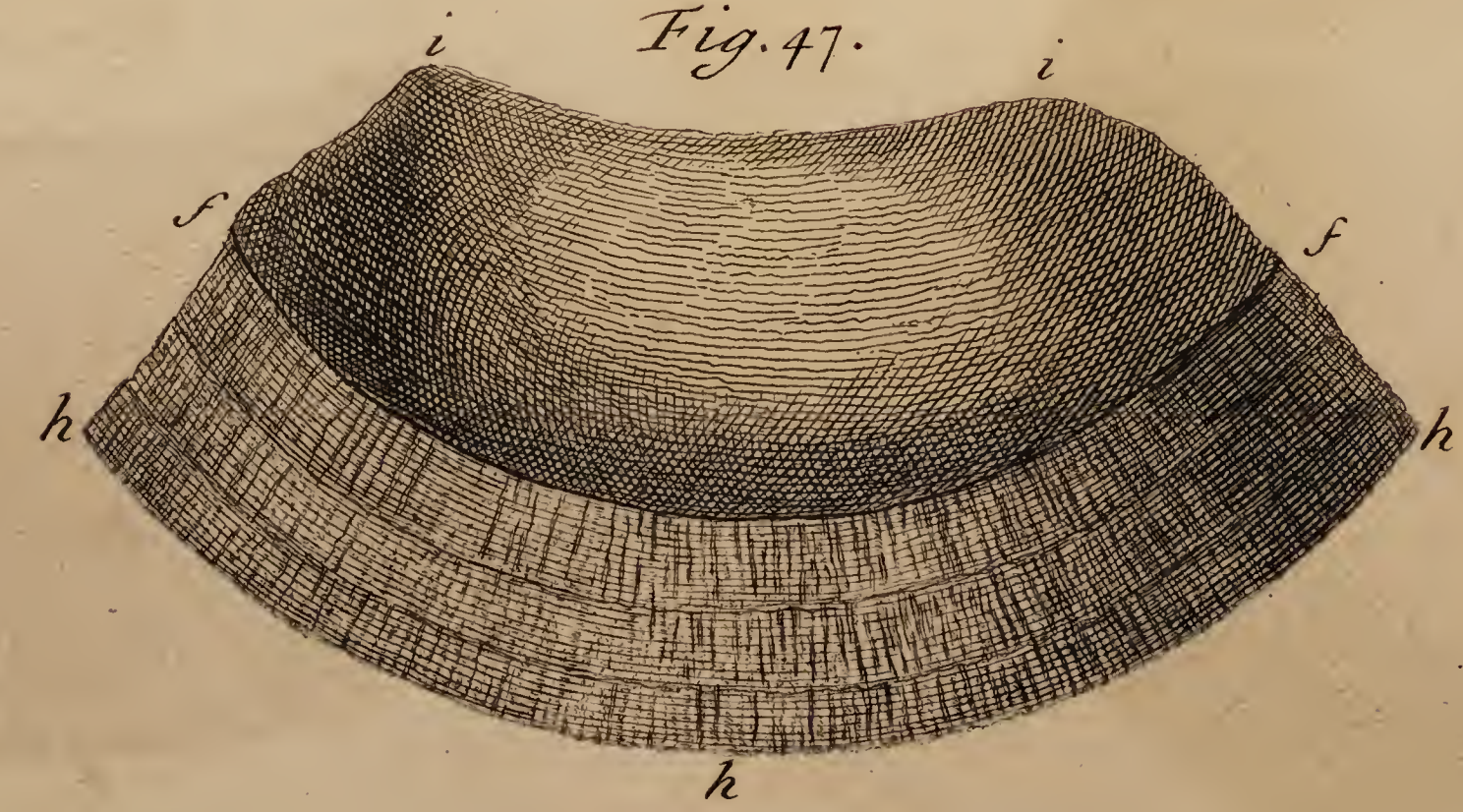
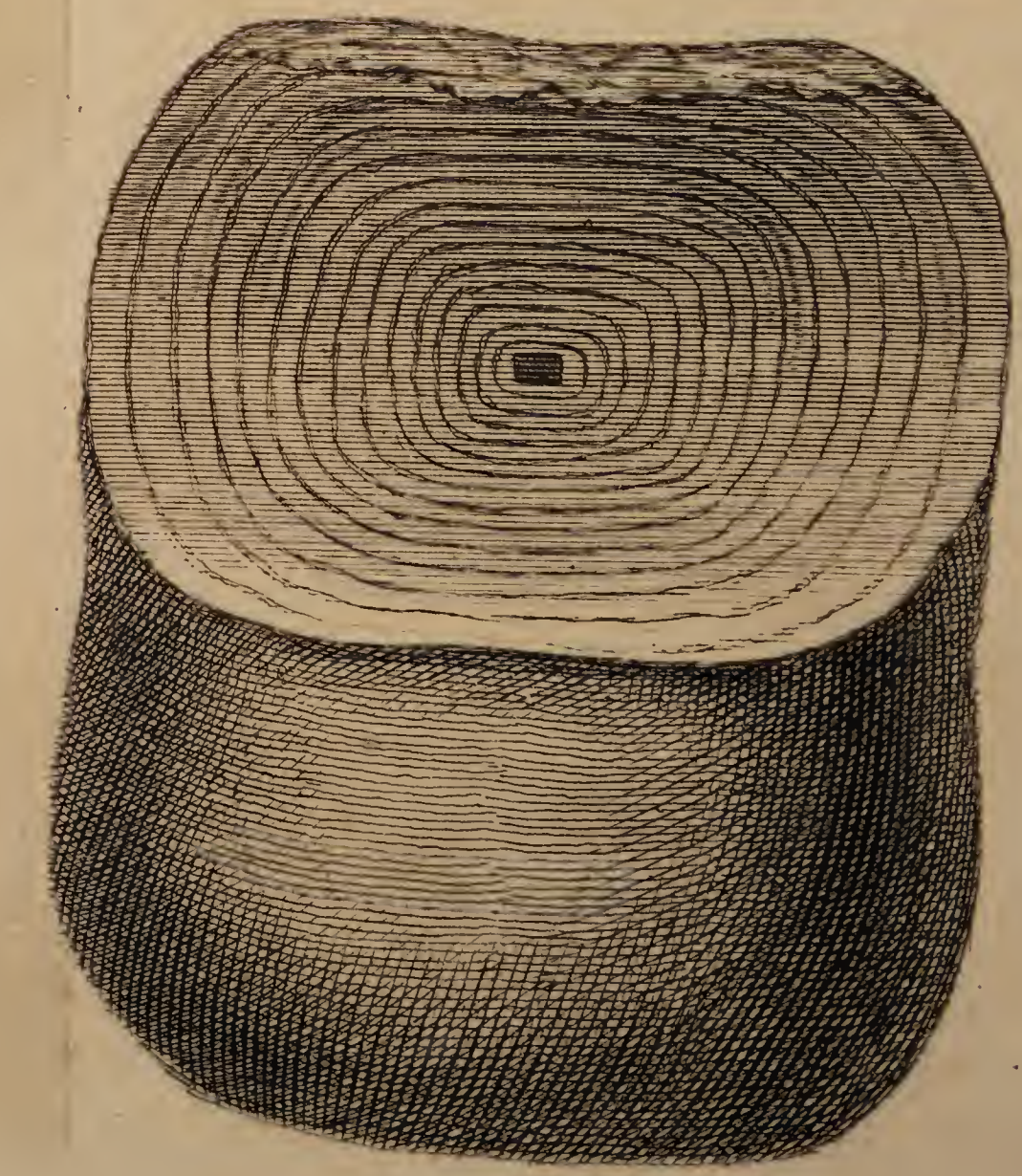


Fig. 52.



child be dead ; but with this caution, that, immediately after the birth, it should be removed ; for if it remains tied there, it brings away the womb, &c. and the woman dies.

This I believe to be attributed to them from their center's being sometimes loose, and rattling within, like an *ætites* or eagle-stone, as some of those I have do : also that it is good for intermitting fevers given in their drink at the going off of the fit ; and that it is good for the colic, and hypochondriac melancholy, as may be seen in that author, who I think is the only one, that speaks of it. It has been the most rare of any of these snake-stones, as they are called in the *Indies*, and so not taken notice of. I was informed there was one in *Paris* offered, to that great inquirer after natural productions the late Duke of *Bourbon*, at the price of 100 pistoles ; to whom I signified by some of his acquaintance, that I had more than one of them, and would make him a present of one, which I afterward did, lest he should be imposed upon by giving such a price, as some curious persons have often been in other things of the like nature.

Pietre del serpente Cobra de Cabelo * *Redi esperienze, Nat. p. 3. Tab. 1.*
Lapides serpentis Cobras de Cabelo dicti, ed. Lat. Pedra de Cobra,
Kempfer. Amœn. Exot. p. 396. Pierres de serpent. Biron Curiosit. de
la Natura, &c. p. 72.

Dr *Bateman*, formerly *Pref. of the Coll. of Physicians of London*, told me, with great admiration, that he had seen great effects (upon the bite of a viper) of the snake-stone or serpent-stone, as it is called, before King *Charles II.* who was a great lover of such Natural Experiments ; and that he knew the person possessed of the very stone he had seen tried, who he believed would part with it for money. Upon my desire and request to see him, he came to me, and brought with him the stone, which was round and flat, as the common ones brought by Merchants and others from the *E. Indies*, about the size of a milled shilling, but thicker, for which he asked 5 guineas, tho' it was broken. There are several of this sort, figured in *Fig. 61. a, b, c, d.*

Dr *Alex. Stuart*, returning from the *E. Indies*, brought from thence, among many other curiosities, some of these snake or serpent-stones, together with this account of them, which he had from a Father Missionary in the *East Indies*, “ that they were not taken out of a serpent's head, but made of the bones of the small buffalo in the *Indies*” (by which their coaches are drawn instead of horses) ; the bones being half-calcin'd or chard by the dung of the same buffalo. He gave me seve-

* Which signifies in *Portuguese*, the hooded serpent, because it has a membrane about it's head which it can expand like a hood : by others it is called the Spectacle-Snake ; for on the back part of it's neck is the representation of a pair of spectacles. See a figure of one in *Kempfer's Amœn. Exot. p. 567.*

ral pieces, with some of the snake or serpent-stones made out of them, and which I have in my collection of several shapes and colours.

I think the first who gives any account of them is *Francesco Redi* at *Florence*, who had them from the Duke of *Tuscany's* collections, and who, in his *Esperienze Nat.* tells great virtues of them, related by 3 *Franciscan* Friars, who came from the *E. Indies* in 1662. which were, that, being applied to the bites of the viper, asp, or any other venomous animals it sticks very fast till it has imbibed or attracted all the poison (as a loadstone does iron), as many people in the *Indies* believe, and then it falls off of itself; and being put into new milk, it parts with the poison, and gives the milk a blueish colour; of which *Redi* tells the success of those he figured.

Kempfer, in his *Amœn. Exot.* p. 396. speaking of this, says, it helps those bit by vipers, outwardly applied; and that it is not found in the serpent's head, as believed, but by a secret art made by the *Brabmens*; and that, for the right and happy application of it, there must be 2 ready; that when one has fallen off filled with the poison, the other may supply it's place. They are commonly, as he says, kept in a box with cotton, to be ready when occasion offers.

Biron says, that if the wound of the serpent has not bled, it must be a little pricked, so as the blood comes out, and then to be applied as usual. It comes from the kingdom of *Camboya*.

Fig. 54. *Fig. 54.* a *pietra de mombazza* $2\frac{3}{4}$ inches in diameter, weighing $10\frac{1}{2}$ oz. 3 dr. 13 gr. with large prominences or embossments on the outside.

Fig. 55. *Fig. 55.* another $2\frac{1}{4}$ in diameter, with smaller embossments on the outside.

Fig. 56. *Fig. 56.* the same sawed in two, and the section polished, wherein appears a common pebble *a*, of an ash-colour, as the core or center on which this stone was formed. *b b b b*, are several almost concentric lines, nearly answering to the shape of the pebble in the center, and resemble the different coats of an onion, shewing the progressive accretion of the several *laminæ* or *strata*, of which this stone is composed; and *c c c c*, pyramidal spaces of a darker colour, and more compact texture than the intermediate lighter-coloured spaces, whose bases arise at the outer circumference and form the embossments there, and whose points all tend towards the center of the stone; both the outside and inside of these stones are of a light oker-colour diluted with a little white; the pyramids being about 2 shades darker than the rest of the in- or out-side.

Fig. 57. *Fig. 57.* another of the same sort sawn in two, in the center of which is lodged a fruit or large seed, about the size and of the shape of an acorn, having a thick husk on the outside.

Fig. 58. *Fig. 58.* a coin of *Domitian* in small brass, having on the foreside, the figure of a rhinoceros with 2 horns growing out of his nose, the one above the other; which in the *Numismata Pembrokiana*, Part. 1. Tab. XVI. n. 68. the Engraver has made like a tusk or *dens exertus* of

of a boar, and in Part 3. Tab. 39. he has made the 2 horns on his nose like 2 tusks, and has likewise given him 2 horns close to his ears; so that he has made him a creature with 4 horns; and therefore it was thought proper to give an accurate copy of the medal, in order to clear up that famous passage of *Martial, Lib. de Spectac. N°. XXII.*

*Namque gravem gemino cornu sic extulit ursum,
factat ut impositas taurus in astra pilas.*

Which has for many ages puzzled the critics, all thinking that the rhinoceros was a real unicorn or animal, which never had any more than one horn*; and beside the double horns, or *geminum cornu*, in Sir Hans Sloane's Museum, I am told Dr Mead has got another *geminum cornu* likewise from Africa.

Fig. 59. is the reverse of the same medal, with this inscription IMP Fig. 59.
DOMIT AVG GERM and in the middle sc

Fig. 60. is the figure of the rhinoceros magnified, that the position Fig. 60.
of the 2 horns might appear distinct and plain.

Fig. 61. a, b, c, d, represent the *pietre de serpente cobra de cabelo*, of an Fig. 61.
ash-colour and black. In that marked b, the dark shade in the middle shews an hollow, which was part of the cavity of the inside of the bone. e and f are rough pieces of bones, half-calcined, porous, and not polished. The figure and description of the buffalo, whose bones they use for this purpose, are given in Mr Edwards's History of Birds, to which he has subjoined the figures and descriptions of some few rare quadrupeds. Plate 200.

XXXV. Dec. 1, 1745. a neighbour's large mastiff dog, mad, broke out in the night from the place where he was too carelessly confined; and, by a rotten back window, entered my stable, fell upon my horse, and bit him in many places, as shoulder, breast, and right nostril; which was indeed much torn. He bled largely. The town being early in the morning alarmed by this mad dog, and my horse being found loose, his collar broke to pieces, wounded in many places, and much blood scattered up and down the stable, it was too justly concluded the dog had fallen upon him.

According to *Desault's* method, and what Dr *James* says, in a letter I had from him on another occasion, will effectually prevent the ill consequences of this bite; I immediately ordered the wounds to be well rubbed with a mercurial ointment, *ex Axung. porc. 3vj. Argent. viv. 3ij.* About 3ij. were at times expended.

Next morning he was bled ℥ ij. or more; after which I gave him in milk *Lichen ciner. terrest. 3vj. Pip. nig. 3iiij.* 5 mornings successively; which I repeated at the end of a fortnight for 4 mornings more.

* See Vol. IX. P. iii. Chap. i. §. xxiv.

Account of a horse bit by a mad dog; in a letter from John Starr, M. D. to John Huxham, M. D. F.R.S. N°. 495. p. 474. May &c. 1750. dated Liskard, Jan. 10. 1749. Read June 21. 1750.

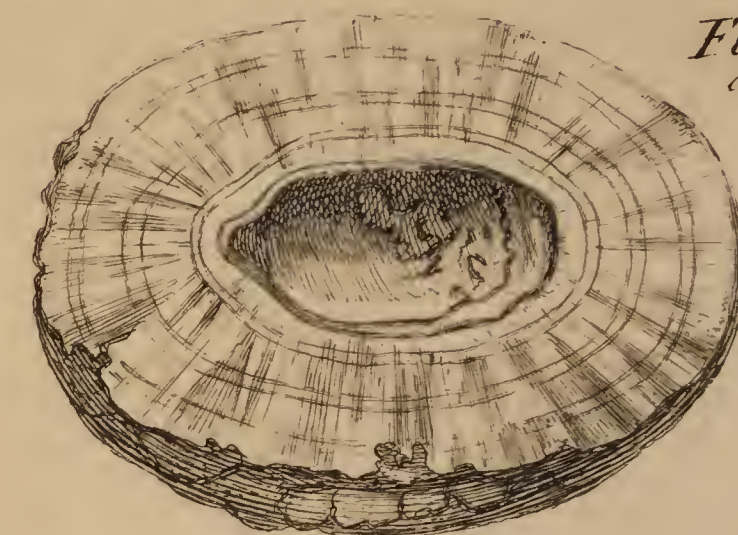
As the *Pulv. Antilyssus* was not in our shops, and no one in town knew the *Lichen* but myself, I went with my servant *Sunday* forenoon, the day of my horse's misfortune, to seek it. What I found was, I fear, too young; for it seemed just coming from the earth, and the leaves were scarce $\frac{1}{3}$ as big as at it's full growth. I got what I hoped might be sufficient; and, after cleansing, perhaps too hastily dried it at the fire, that it might be ready for use the next day.

The wounds healed up soon, without any other application; and the horse fed uncommonly hearty after a day or two (during which the fright had made him uneasy and fretful) and seemed to improve considerably in every respect. I omitted riding him for 20 days; but about the 20th rid him 2 short journeys only. He travelled chearful and brisk, and I took care not to heat him (for though the horse was old, I would not willingly have parted with him for 20 pieces). I saw him every day, but could in no respect discover any thing amiss.

Dec. 25, two days before the full moon, my servant told me, that in the morning he trembled much on entering the horse-pool, and refused to drink at the watering trough; but in the evening drank heartily at another well. This alarmed me; but considering that horses frequently refuse to drink there, and that he drank in the evening, I was somewhat easy; but ordered my servant, if he refused next morning drinking at one, to try him at the other; and if he refused at both, to let me immediately know it.

Dec. 26. As soon as he entered the horse-pool, he trembled all over in a most surprizing manner, and would by no means attempt to drink. The servant immediately returned with him. I ordered him to be led into a small pool of rain-water which stood in my court. The trembling returned; every muscle was strangely agitated; he looked as if he were melancholy on the water, smelt to it, but would not touch it. Being put into the stable, a bucket of pure clean water was brought to him; he eagerly thrust his mouth into the water, but, endeavouring to suck it, a convulsion seized him.

I was now satisfied he had a true *Aquæ Pavor*. He was bled to about 3 pints, musk 3ss. *Cinnab. Ant.* 3j. made into a ball with *Cons. Anthos.* was given him. In bleeding he once snapt at the Smith, though well known to him, having shoed him for years: and indeed this was the only time he attempted to bite any one. In about 2 hours after the musk was given to him, I offered with my own hands about 2 gallons of white water warm: he drank it off without the least difficulty or hesitation. Had I dissolved it in 3ij of nitre, I had certainly done well. Had the quantity of musk at first given been greater (for Dr *James* writes me, he gives the above quantity of the best musk in a watchfulness remaining after a febrile *delirium* is removed), or had I now again repeated the same ball, I am apt to think the horse might have been saved (this being the *Tonquin* method, even after the appearance of the *hydrophobia*); for he was as yet quiet and tractable. I went to him as usual,



1870

1871

1872

1873

usual, handled him, and he behaved as in his former health; but, being looked on as a mad horse, I saw every one was afraid to meddle farther with him. He eat both hay and oats heartily. In the evening, about 9, more of the white water was offered him, but he drank none.

Dec. 27. This night the madness increased much; for he had bit the manger as far as he could reach, and made it quite ragged. In the morning he frequently bit his breast where the wound had been; and when he happened to take hold, violently drew up the skin with his teeth. Both these things he did during the day at times, but most in the morning. I put a tub of water before him; he greedily ran his nose into it, but, endeavouring to drink, a dreadful convulsion seized him, which sometimes drew his buttock to the ground; at others his back was so hollowed with it, that his belly was brought almost down on the litter. During the convulsion he would groan in an affecting manner; and frequently cry out. As soon as the convulsion was over, he repeated his endeavours to drink with the same cruel event; and would, I believe, had the water stood before him, have repeated it the whole day. He still eat his allowance of hay and oats; but when not eating, he was continually thrusting out his tongue, and working with his lips, as if to moisten and cool them. His tongue was exceeding dry, and of a blackish brown colour on the surface. As he eat oats, I have sometimes lamented I had not mixed *Turpeth. mineral.* with them. I tried him with water about 9 at night; every thing was as in the morning; only the convulsion was, if possible, stronger, and more excruciating; for he groaned deeper, louder, and in a more affecting tone. His breath was exceeding hot; it came from his nostrils like smoke from a chimney-top; he expanded his nostrils as if he had been violently running; and the steam was visible for more than a yard distance.

Dec. 28. This night he broke his collar in pieces, broke down the partition by which he was separated from the place of my other horse, traversed the stable, attempted to get out; in order to which he beat down the under half of the stable-door; however, in the morning, being spoken to by my servant, he neigh'd, immediately went to his place, where he stood biting his breast and manger almost continually. His look was now become wild and furious, and about 10 o'clock I order'd him to be shot.

P. S. I observed he was always worse, every symptom being aggravated at the time the moon came to the meridian; which again, as the day advanced, in some degree abated.

XXXVI. 1. As to the distemper now reigning among the cattle, I am informed by the cowkeepers, that a cow shall be seemingly well, and feed heartily over-night, or in the morning, and give the usual quantity of milk; that in 12 hours time they shall all of a sudden abate in their milk near $\frac{1}{2}$, and entirely fall off their stomach, so as neither to eat or drink, and then gradually lose all their milk. As soon as they

Some account of the distemper raging among the cow kind in the neighbourhood of London, together

with some re-
medies propo-
sed for their
recovery; by
C Mortimer,
M D. Sec.
R. S. and Coll.
Med. Lond.
Soc. N^o. 477.
p. 532. Aug.
&c. 1745.
Read Nov. 21.
1745.

perceive this, they give them a warm mash of malt, or the following drench: "Take 3ij of caraway-seeds, boil them in a quart of water, and strain it; add a gill of whitewine, and $\frac{1}{4}$ of a pound of honey."

Their teeth are commonly observed to be loose; for which they lance the gums, and rub them with salt and vinegar. The very first day they have a huskiness, they breathe short, and wheeze, but have no great cough; for which they have blooded them, [in too small quantities] and rubbed their noses with tar, but with no success. Some hang down their heads, and run much at the nose; for which they lay a bag of scalding hot malt to their heads, tying it between their horns. This has sometimes relieved this symptom, but the beasts have not recovered. The second or third day most of them, not all, fall into a purging, groan much, and seem to be in great pain. The stools, I have seen, seem to be bilious, have cakes of gelly come away with them, and some were streaked with blood. They soon die after these stools come on. Those that are kept out in the cold air seldom live beyond the third day; those that are kept warm in houses, and cloathed, live 5, 6, or 7 days. Many of the cows, I have seen, have a wild stare with their eyes; the whites of the eye, and the skin of the eyelids, looked yellowish: their tongues looked white; they had no extraordinary heat in their mouths, at the roots of their horns (a place where they usually feel to judge of the heat of cattle), or in the *axilla* or arm-pit, if I may so call it. The *mucus* running from their nose is very thick and ropy: their milk is thick and yellow.

In the two I have seen opened, the flesh and blood looked much darker coloured than usual; the fat of the first looked yellow; the lungs were much inflamed in many places, and had several large blisters, 2 or 3 inches over, full of water, on their outward surface: there was no water in the *thorax*, little or none in the *pericardium*: the heart looked well, but the blood in it was not at all clodded, being exceeding fluid and dark-coloured: the paunch was very full of food, and greatly distended: the stomach looked well; the liver was full of scirrhus swellings and chalky knobs; the gall-bladder bigger than usual; the gall fluid, but dark-coloured; the intestines inflamed in many places; the fat about the kidneys was distended with air; the kidneys were found, as was the bladder and *uterus*. This cow was not with calf. On opening the skull much water gushed out. In the second cow the fat was not yellow; the lungs, heart, paunch, and stomach, were like the former; the liver was pale, flabby, not scirrhus: but the gall-bladder very large; the intestines inflamed, and in some places livid; the fat of the kidneys in this was found, but one of the kidneys was mortified. This cow was about a month gone with calf. The man who fleaed and opened these cows said, These were the general appearances in most he had fleaed; only that in some he found water in the cells of the cores of the horns. They flea off the hides, which they say are good to tan; and they save the fat to make tallow of. The fleaver told

told me, a poor man made a hearty meal of some steaks he cut off one of these cows, and that he was not sick with it *.

From these circumstances I think it evident, that this distemper begins by an inflammation of the lungs, attended with a catarrh or flux of humours from the nose; that in the progress of it there comes on an inflammation of the guts, and a purging, caused by an acrimony and overflowing of the gall, which ends in stools tinged with blood, exciting great pain in the bowels, and so brings on death.

Bleeding (in small quantities) has not been found effectual, nor in short any of the remedies yet made use of; therefore, having a chief regard to the ultimate effort of nature, which seems to be to carry off the distemper by an extraordinary discharge of gall, I hope the use of *crocus metallorum*, a medicine made use of with success in horses, and a great discharger of gall, as I have known its good effects in the jaundice in men, may be attended with success: I have therefore proposed to some cow-keepers to give to a cow, as soon as taken ill, one of the following balls.

“ Take *crocus metallorum* half an ounce † in powder; make it into a
 “ ball with dough or crum of bread moistened; give the cow a
 “ draught of bran and warm water after it, and repeat the draught
 “ after every purging stool.”

For the running at the nose, I am told, that pouring a pint of warm vinegar, with an ounce of salt, into the nostrils, has proved successful in making the cow sneeze, and discharge a great quantity of thick yellow *mucus*, and other matter, from the nose, after which the cow recovered.

For the shortness of breath, I have advised the giving “ whale-oil,
 “ treacle of sugar, each a pint; flower of brimstone ℥iv: give it in a
 “ mash of malt, or bran and water, twice or thrice a day.”

For the scouring, first give the *crocus*-purge above-mentioned; then give them every 6 or 8 hours the following draught.

“ Take whiting one pound, bruise it; pour boiling water upon it,
 “ a quart or more; let it stand to settle; pour off the clear water,
 “ and fling it away; then put a quart of warm water to the wet
 “ whiting; and add bole-*armeniaca* in powder ℥ij. Venice-treacle
 “ ℥i. *English* malt-spirits ℔ss.

These proposals being founded upon the appearance of the symptoms, I hope they will be attended with the wished-for success.

2. Since my former paper I have had opportunities of being present. *Further observations; by the same.*
 when 3 cows have been fleaed and opened; the lungs in all were inflamed and blistered, and the guts in some places inflamed, in others livid, the gall-bladders exceeding large: a collar-maker's man, who *Ibid. p. 549.*
 has been assisting in fleaing above a hundred dead cows, assures me, *Read Dec. 12. 1745.*

* I am assured, that a very sufficient experiment was made in our army in *Flanders* last campaign in favour of this.

† Or more, according to the size and strength of the cow; or as the first dose is found to operate.

these

these are the general appearances in them all ; except that in one he met with a large bag full of corruption, between the bag inclosing the heart, and the back-bone ; in another he found the gall-bladder quite contracted and shrivelled up, having little or no gall in it ; and in several he found scirrhus knobs in the livers.

Nov. 26. I desired Mr *Hill*, an Apothecary in *Westminster*, to accompany me to see a cow dissected, and to help me to examine every thing very carefully, having got her drawn into a shed, to defend us from the weather. When the skin was taken off, she appeared very fat ; the muscles looked of a darker colour than usual. On opening the *abdomen* the caul appeared very fat ; the paunch was greatly distended ; on making a puncture much wind gushed out : it had in it a great deal of food ; the inside looked well, and did not peel ; the second and third stomach, or the *omasum*, as also the fourth stomach or *abomasum*, were almost empty, but looked well ; the liver was firm, well-coloured, and sound, except a few scirrhus knobs about the size of nutmegs : the gall-bladder was exceeding large, and full of very fluid gall ; the guts were inflamed in many places, the *colon* and *cæcum* livid : I had the curiosity to have them measured ; from the *anus* to the insertion of the *cæcum* there were 12 yards (the *cæcum* was an ell long), and from the *cæcum* to the *pylorus* there were 52 yards. The midriff was much swelled and inflamed : the lungs were swelled, inflamed, adhered in some places to the *pleura*, and almost wholly covered with bladders of water : there was no appearance of any inflammation on the *pleura*, or in either the internal or external intercostal muscles : the windpipe was inflamed greatly throughout it's whole course, especially it's inside ; but the gullet, which lay so near it, was not in the least inflamed : the heart was of it's natural size, the *pericardium* full of very fluid blood, probably from the bursting of some branch of the coronary artery, caused by the extraordinary accumulation of blood in the right ventricle ; for the *vena cava*, and right ventricle of the heart, were turgid, and full of black coagulated blood, though this cow had been dead but 12 or 14 hours ; the lungs were likewise turgid with blood, but little or none was found in the left ventricle or *aorta* ; the obstruction seemed to have been so great in the lungs, that very little blood could pass through them from the right to the left ventricle of the heart, and therefore evidently evinces the existence of a confirmed peripneumony. All the membranes lining the nostrils, and the spongy bones thereof, were quite turgid with blood, and in the highest state of inflammation. The greater and lesser brain looked fair and well, seeming no way distempered.

I have not seen, in any cows I have examined, any cutaneous sores or exulcerations, nothing like the boils, carbuncles, &c. described by authors as the constant concomitants of the plague in men : nor does there seem to be any attempt of nature to sling off the distemper by any internal impostumation, or discharge, unless by the running at the nose, and by the bilious stools, or bilious urine. The few, which have recovered,

covered, have been such as have been kept within doors very warm, have been blooded once, twice, or oftener, have had warm mashes of malt and bran given them, and warm drenches of warm herbs, such as rosemary, wormwood, and ground-ivy, with honey or treacle, and have neither purged at all, or but little; and when they have not purged at all, their urine has been observed to be as high-coloured as *porter's* beer.

I am informed, by the Farriers and Cowleeches, that a horse or a cow will bear to have near 2 gallons of blood taken away without fainting. One cow, I have seen, within about a month or 6 weeks of her calving-time, was taken with the running at the nose, and shortness of breath; the owner of her immediately took away out of the neck 5 quarts of blood by measure, and gave her a warm mash of malt once in 6 or 8 hours: next day he cut her tail, and let her bleed 2 hours; the day after he took away 2 quarts from under the tongue, and so continued bleeding her, at 14 or 15 hours distance, for 7 times. She did not purge at all; her urine was as high-coloured as coffee at first, but grew paler and paler every time of bleeding: she soon recovered, now eats heartily, looks brisk, and has not flunk her calf.

The concern the cow-keepers are under for the loss of their substance, the various methods offered to them, and their want of judgment either to chuse the most rational, or their want of accuracy in making experiments, and following directions, is quite discouraging, and is the reason why none of them have pursued any regimen so steadily as to give one an opportunity of making conclusions from it: indeed several own to me, they are quite bewildered, not knowing which way to turn themselves, or whose advice to follow, what one says being quite contrary to the directions given by another. Some to whom I have given my directions have blooded once, have given the purge once; but have not given the oily drench, or have given this once, and have not repeated it; others have given the chalky drench once, and not repeated it, and have not followed the other parts of my instructions; so that I am sorry to find that I can have no satisfactory experiments made: yet, as the state of the disease seems so evidently to be a peripneumony, or inflammation of the lungs, windpipe, and nostrils, attended with a redundance of gall, I cannot forbear urging to the Public the following method.

- “ Give to all cows in general, while well, \mathfrak{z} ss or \mathfrak{z} i (according to the
- “ size of the cow) of *crocus metallorum*. As soon as a cow falls off
- “ her meat, give her another dose of *crocus metallorum*; and give
- “ her warm mashes of malt, bran, &c. When she runs at the nose,
- “ lay a bag of malt-meal, wetted with boiling water, upon her
- “ forehead and nose, tying it to her horns, morning and evening;
- “ pour warm vinegar and salt into the nostrils: if a short cough,
- “ or difficulty of breathing, comes on, bleed her one quart twice
- “ a day, for 3 or 4 days, and every 6 hours give the oily drench:
- “ if a

Of the Distemper among the Horned Cattle.

“ if a purging comes on, give another dose of the *crocus metal-*
 “ *lorum* ; if it continue, give the chalky drench every 6 hours,
 “ and if it does not abate in 24 hours, inject the same mixture by
 “ way of glyster ; and if the husky cough continues with the
 “ purging, give the oily drench one 3 hours, and the chalky
 “ drench the next 3 hours.”

Most of the cows which have recovered from this distemper recover their milk again, as their appetites mend ; but they are observed to have scabby eruptions come out in their groins and *axillæ*, which itch much ; for a cow will stand still, hold out her leg, and shew signs of great pleasure, when a man scratches these pustules or scabs for her.

I am informed, that some cow-leeches have given *coloquintida* and salt of tartar, each $\frac{3}{4}$ i in a quart of warm ale ; but I imagine it must be too griping a purge, and improper where the guts are inflamed. Indeed I have not heard of any cows recovering which took it. As for the cause of this distemper, I am still at a loss ; I think it cannot be owing to the food, because the cows which had it first in *Essex* eat only grafs, turneps, and hay or straw ; the cows about *London* eat, some, grafs ; all, grains and hay, some, little or no grafs, but live chiefly on grains, turneps, off-falls from the garden-grounds, and hay.

I am in doubt as to the air ; the spring and summer were very wet, and the ground very damp, the autumn was very dry and cold, the beginning of winter very damp and cold. The cows in *Essex* had the distemper in summer ; it first began about *London* in autumn : it has spread itself equally among cows which have lain in the fields a-nights, and those which stood in stables or sheds : it spread itself in *Essex*, at first into such farms where they bought in strange calves, or lean cows, at market, which they did not know where they came from ; but most probably from the hundreds where the disease first broke out ; but how it got thither, whether by importing any cattle from *Flanders*, I know not ; for surely there is too wide a tract of sea for any infectious *miasma* to be wafted over to that part of the country by the winds ! This is certain, the *viscera* concerned in respiration are the parts chiefly affected. It's spreading here in *England* has been progressive ; and therefore one may reasonably think it is not constitutionary in the air, for then it ought to be universal every-where ; but that it is contagious, and propagated by infected cows being mixed with well cows : therefore the not buying in calves, or strange beasts, but every Farmer keeping his herd by itself, must be a great means of preventing the propagation of it : and housing the cows a-nights may be a proper preservative against it.

A third account of the distemper among the

3. During the *Christmas* holidays, we sent for some milk, as usual, from the vineyard in *St James's Park* ; none of the cows belonging to that house having as yet caught the distemper, though three had already died

died in the *Park*: we used part of the milk for chocolate, and set part by for cream for the next morning: the milk had a rank sourish smell and taste like rank butter; the cream next morning was more so: we boiled the milk, which did not curdle; so we used the cream with tea, tho' the taste was not very agreeable. The milk boiled curdled in the tea; neither any of my family, nor a friend who drank of it, found any inconvenience from it. Upon sending the morning following for more milk, the people refused selling any, saying, one cow was taken ill, and another was near dry*. This was the cow whose milk we had had, and she died in 48 hours. Next day another fell ill, and was knocked on the head by the public officer, in about 48 hours after her being seized. I had the curiosity to see this cow opened, which was done the next day but one. The inflammation in general in this creature was greater than what I had before seen in any of those which died of the distemper: this cow had been blooded about 3 weeks before she was taken, and once as soon as taken: the cawl was greatly inflamed, the paunch inflamed, and the inner coat peeled off, especially that of the [*abomasum*] faidle; the guts were all inflamed; the liver was much inflamed in some parts, in others was turned livid; the gall-bladder was very large, and the gall very liquid: the lungs adhered, in many places, to the *pleura*, were greatly inflamed, and turgid with blood, and were in many places quite black; I did not find any of the watery bladders on the surface of these, as I did on all the others I had seen opened.

cows; by C. Mortimer, M. D. Fellow of the Royal College of Physicians, and Secretary of the Royal Society, London. No. 478. p. 4. Jan. & Feb. 1746. Read Jan. 9. 1745.

Here's an instance of the most surprisingly quick progress of this distemper, and to such a violent degree, that I do not think it in the power of medicines to have prevented death; but I think this case is still a further confirmation of the necessity of plentiful bleeding as soon as a beast falls sick, especially if a shortness of breath ensues: this cow was not come to the stage of purging.

From the distemper getting into the *Park*, I think there is reason to conclude it cannot arise solely from any fault in the food, because the pasture is always good there, and, from the great number of horses, always kept low; and the soil never dunged or manured; and the cows have plenty of hay in winter. How it got into the *Park* is very strange; there having been no fresh cows brought in there since *Welsh-Fair* in Aug. And this is further very observable, that though this distemper seems so very infectious among the cow kind, yet I do not hear any of the deer have fallen ill; which is much more likely to happen to them than to the horses, because they chew the cud, these do not. I humbly therefore suggest, whether it would not be the most likely means to put a

* I sent for some of the milk of the sick cow, after she had been about 12 hours ill: they could not get above oz. 2. which was as thick as cream, and yellow like cheese: it curdled, being put into Bohea-tea next morning. In about 3 days keeping, it turned of itself into a substance like cream cheese, without separating any serum. In 8 days it dried away to an hard cheesy substance, and in 14 days became quite dry, like the rind of *Gloucester* cheese: it smelt like rank butter at first, but never corrupted or stunk.

stop to the spreading of the distemper, to forbid any cows or calves, being brought to market, to be sold alive, or that any Farmers should buy in any fresh cattle for 6 months, or till it is found, that the distemper is entirely ceased ; and that all fat cattle should be kept carefully separated from the cows and calves, and that under severe penalties.

An appendix
to the fore-
going paper ;
by the same.
Ibid p. 7.

4. Upon my reading the foregoing paper, some Gentlemen present favoured the company with the following informations and remarks.

Mr *Theobalds* informed the Gentlemen present, that the first infection of this dreadful distemper among the cow-kind was brought over from *Holland*, in *April* 1745. by means of two white calves, which a Farmer at *Poplar* near *London* sent for in order to mix the breed ; and that the infection was got to *Maidenhead* in *Berkshire*, by two cows brought out of *Essex*, and sold at the fair there : that there was observable a very disagreeable smell in the clothes of persons, who had been very conversant with sick cows ; and that the infection had been propagated by means of sheep, who, it is presumed, carried it in their wool. Upon the mention of this scent in clothes, I remarked, that Dr *Lobb*, in his late book intituled, *Letters relating to the Plague, and other contagious Distempers*, in his letter to *John Milner*, Esq; p. 388. recommends to persons conversant about sick cows to wear a linnen garment (over their other clothes) wetted with a mixture of salt and vinegar ; and, *ibid*. p. 383. he gives many prudent useful rules to *Farmers* for the management of their sick cattle.

Dr *Parsons* said, That the cattle in the high grounds about *Hampstead*, *Highgate*, *Millhill*, and *Hendon*, had hitherto remained free from the infection ; but that it had spread all about in the lower grounds.

Mr *Hoffman*, a learned *Danish* Gentleman, said, This infection was first carried into *Denmark* by raw hides of cattle dead of this distemper, rubbed with wood-ashes, in order to preserve them fit for tanning, which were brought from *Flanders* : that some cows sickened in a few days after the unpacking of these hides in *Denmark* ; and that they have lost above 50,000 head of cattle in that kingdom.

At another meeting, Mr *Collinson* acquainted the company present, that a Farmer in *Essex*, who had the distemper among his cows, invited a neighbouring Farmer to come and assist him in giving drenches to some of his sick cattle ; the good-natured man went accordingly, and spent best part of the day with his neighbour, to lend him his help in his distress, little dreaming of what ill consequence this friendly act was about to prove to himself : for, being so many hours conversant with the diseased cows, so much of the infectious *effluvia* adhered to his clothes, that, as he was walking home, which was about a mile and half, his way lying thro' a field in which several of his own cows were feeding, he no sooner entered the field, but the cattle all left off their grazing, ran to the farther end of the field snorting and flinging up their noses, shewing the greatest uneasiness at their master's approach, and endeavouring, as much as possibly they could, to avoid him, as though they

they smelt something very disagreeable; and so indeed it proved to them, for the very next day many of them fell sick, and died in a few days.

A certain cow-keeper in *Totbill Fields, Westminster*, had 30 cows, out of which number 4 only have survived; 2 never took the infection, one had it and recovered; and he assured me, that one had the distemper 4 several times; for that, as soon as she was well for a week or ten days, she relapsed, and went thro' all the stages of the disease, but now continues well.

In *St James's Park* are kept 17 cows, of which number four were bought in new at *Welsh Fair*; out of these 11 are dead; 4 never had the distemper, and 2 recovered from it. These are the cows which were so plentifully blooded, mentioned above, and one of them, then said to be very big with calf, being recovered, went the proper time, had a living calf, and is well and thrives: indeed they knocked the calf on the head, because they wanted the milk.

I am informed, that a Farmer at *Little Chelsea*, who had but 10 cows, has not had any fall sick, tho' his neighbours had cows sick all around him. His management was, not to let any of his cattle have any communication with his neighbours; to keep them within doors, littered like horses with clean straw; to feed them with good hay, and give them plenty of clean water to drink; to turn them out every day at noon into his yard to air themselves; and, in the mean time, to clean out the cow-house carefully; removing all the litter, washing the pavement clean with a birch-broom, laying clean litter, and keeping them warm a nights.

As a contagious distemper among the cow-kind is no new thing, I thought proper to look into the *Auctores de re rustica*; but find none so full in the account of the pestilence among cattle as *Columella* is*. He advises, as soon as any signs of an infectious distemper are perceived, to drive the cattle immediately into a different air, at as great a distance as can conveniently be done; to separate the sick from the sound; and that there should be no intercourse between them, lest the infection be carried to the sound. If these cautions only were religiously observed by our Farmers, I think there is reason to hope the contagion would soon be extinguished. I should advise the building several small hutts with faggots and broom, at a distance one from another, in some fallowed field, and there keep a man constantly to attend the sick cattle, and to have every beast, as soon as it begins to sicken, removed into one of these hutts, as into an infirmary; by which means the cow-house will be kept clear from infection: and never let this man go near the well cattle, but keep them in the most distant pastures, and let them have hutts run up likewise to shelter themselves under from the inclemencies of the weather, providing them with clean straw to lie on. I

* Lib. vii. cap. 5.

heartily wish we had the experience to say with COLUMELLA, *Evincendi sunt autem quamvis pestiferi morbi; & exquisitis remediis propulsandi*. He recommends a drench made of a wheat-mash made with all-heal, eringo-roots, and fenel-seeds; and he says he has known as an immediate remedy, a rowel made in the ear with the roots of the larger black hellebore; and he says, that *Celsus* advises the pouring into the nostrils wine, in which misseltoe-leaves have been bruised.

These infectious diseases have not been confined to the cow-kind alone, but sometimes the contagion has been so virulent as to attack all sorts of brutes as well as men. *Ovid* mentions a dreadful instance in his *Metamorph.* lib. vii. l. 536.

Strage canum prima, volucrumque, oviumque, boumque.

And *ibid.* l. 538.

Concidere infelix validos miratur arator
Inter opus tauros; medioque recumbere fulco.

Virgil gives an account of such another contagious sickness in his *Georgic.* lib. iii. l. 515.

Ecce autem duro fumans sub vomere taurus
Concidit, & mixtum spumis vomit ore cruorem,
Extremosque ciet gemitus: —

Lucretius, mournfully describing the plague at *Athens*, of which *Thucydides* has left us so ample a relation, records the infection being likewise spread among the cow-kind: See lib. vi. l. 1129.

Consimili ratione venit bubus quoque sæpe
Pestilitas, etiam pecubus balantibus ægror.

Soon after the times of *Constantine the Great*, one *Severus Sanctus* a Christian Poet has left us a melancholy account of a murrain then reigning among the cows, in a *Latin* eclogue, intituled, *Carmen de mortibus boum*, which was reprinted at *Leyden*, in the year 1715. 8vo.

We see, by these accounts of the *murrain* among beasts and cattle, that this dreadful distemper has often accompanied or preceded the *plague* among the human species: what pains therefore does it behove us to take to prevent the spreading of this disease among brutes? and what warning ought man to take, lest the pestilence should come home to him?

A letter from
James Parsons,
M. D. F. R. S.
to the Pres.
serving to in-
troduce a re-
mark from
John Milner,
Esq; concern-
ing the bury-
ing of the

5. When the means for preventing the infection among the cattle were under consideration, burying them was thought the most effectual method to hinder it's progress; and, by way of improvement to this project, the addition of lime was imagined necessary, for the more speedy destruction of the distempered carcases. But some doubts arising, whether the lime might not exalt the putrid particles, and help to spread the infection, it was the opinion of several of the learned, that it was most safe, on that account, to bury them without it. This difference will probably be decided by the inclosed account of cattle buried both with

with and without lime, written by *John Milner*, Esq; one of the justices appointed to inspect into the affair, and one who has the good of mankind at heart as much as any person whatsoever. This Gentleman related the case to several others, who were met at a coffee-house where I was present; and, as I thought it concerned the public very much, I waited on him next morning to request he would permit me to lay it before the *R. S.* which he readily complied with, and gave me the inclosed paper for that purpose. I hope it will serve to prevent the practice of burying them with lime for the future, as this accidental fact makes it more than probable, that malignant particles may be sent up,* and spread through the air.

N. B. The cattle were buried 10 feet deep with lime. 8 feet deep without lime.

May 31, 1746.

MR *Stallwood*, a Farmer at *Hackney*, informed the justices, to whom the care of the distempered cattle was committed, that he had buried 13 cows very deep, with the quantity of lime appointed by the justices; and, observing his dogs to scratch and tear up the ground with their feet, to get at the cows flesh (the lime fermenting, and causing a foam, as he called it, or strong scent of meat to arise, which made the dogs so eager to come at it) he beat them off several times: but the dogs always returning as soon as he was gone, he, for some time, hired boys to keep them off. But that he had buried several other cows in another place, with their hides cut and flayed, without any lime (being ordered by the justices so to do), and the dogs never attempted to scratch or tear up the ground there, though it lay open to them equally with the other ground, and they often ran over it.

John Milner.

N. B. Two bushels of lime to each cow was the allowance.

XXXVII. I cannot as yet perfectly answer your enquiries concerning the *Russia* castor which is not all from the same animal, some of it being the *prostatæ*, *testes*, and kidneys of the beaver, gathered in the spring; but the true sort comes from quite a different creature, which resembles a wild goat; just by whose navel the castor is found like two glands. This I am assured from people who have seen it on the spot: but as they are quite unacquainted with Natural History and Anatomy, we must not trust to them too much: I hope soon however to procure an account that may be depended on.

Concerning the
Russia Castor,
in a letter
from James
Mounsey,
M. D. Phys.
to the Czari-
na's army, to
Mr H. Baker,
F. R. S. N^o.
493. p. 217.

Oct. &c. 1749. Read Nov. 23. 1749.

PAPERS omitted.

I. A continuation of an account of an essay towards a *Natural History* of *Carolina*, and the *Bahama Islands*; by *Mark Catesby*, *F. R. S.* with some extracts out of the tenth set; by *C. Mortimer*, *F. R. S.*

* Quick-lime renders the animal salts more volatile and pungent; as in the process of *Sp. Sal amm.* with quick-lime. See *Wilson's Chemistry*, Lond. 1709. 8vo. p. 332. *C. M.*

2. The

Nº. 486. p.
157.

2. The same continued, giving an account of the tenth and last set, by the same. This account concludes with the following words:

Thus ends the most magnificent work I know of; since the art of printing has been discovered: the descriptions are all given in both *English* and *French*, and the figures being drawn by the ingenious author after life, were afterwards etched by himself, and all the illuminated sets were coloured under his directions, and all touched up and finished by his own hand.

C H A P. II.

STRUCTURE, EXTERNAL PARTS, and COMMON TEGUMENTS of the BODY.

*An essay upon
the causes of
the different
colours of peo-
ple in different
climates; by
John Mitchel,
M. D. commu-
nicated by Mr
P. Collinson,
F. R. S. Nº.
474. p. 102.
June, &c.
1744. Read
at several
meetings, from
May 3. to June
14, 1744.*

I. **T**HE cause of the colour of negroes being a subject so little known, but so much inquired after, and withal so curious and useful, as to excite the particular attention and inquiries of the learned in *Europe*, particularly the Academy of *Bourdeaux*, in their prize-problems, to which they have received no satisfactory answer, as I have been lately informed; I have therefore ventured to offer my thoughts to you on that subject, having had frequent opportunities to make the proper and necessary observations; and which, at least, I hope, may not be unacceptable, that some one, of better skill, and more leisure, for such nice philosophical inquiries, may make more correct and fuller conclusions from them. This problem supposes the knowledge of the causes of colours in general; so that if I can deduce the colour of the skin from it's structure, &c. in the same manner, and for the same reasons, from which the great *Newton* deduces the colours of other substances, it is all I can pretend to, which will be as much as that branch of philosophy will permit: and as this problem will include the cause of the colour of the skin in general, I shall first inquire into the cause of the colour of white people; with a change from that colour in some preternatural affections, whose causes seem not well understood. This I shall do in so many propositions, that you may the better perceive how far each proposition is demonstrated, or of what consequence it may be in deducing the desired solution of the problem, concerning the cause of the colour of negroes.

PROP. I.
*The colour of
white people
proceeds from
the colour
which the Epi-*

The truth of this proposition will plainly appear to those, who consider, that the colour of white people is always more or less clear or vivid, as the skin is thinner or thicker, finer or coarser; that is, as it is more or less adapted to transmit the colour of the white parts below it. These parts are the *Parenchyma* of the skin, *Corpus reticulare*, *Papillæ nervosæ*, the

the limpid and clear juices contained in the vessels, and perhaps the in-dermis *trans-*
mits; that is,
from the colour
of the parts
under the Epi-
dermis, rather
than from any
colour of it's
own.
 ner *Epidermis* itself may appear thro' it's outer porous coverlet; all which parts we know are white, and are what appear so in white people. But this will be better confirmed, from the following considerations;
 1. The palms of the hands, lips, &c. where the *epidermis* and skin are so thin, as to transmit the colour from any thing below them, appear red, or of the colour of the red blood under them; especially in those in whom the skin is fine and thin; but where the skin is thick and coarse, those parts appear almost of the same colour with the rest of the body.
 2. The blushings of the cheeks, and their redness in fevers, seem to be another proof of this cause of their colour; for, in a moment, they change from a pale to a deep red; but no one will imagine, that the *epidermis* then changes it's colour, or power of reflecting the rays of light; but that it transmits the colour of the blood; which is, at such times, more forcibly driven into the capillary subcutaneous vessels, and shines thro' the *epidermis*; but, before, these vessels contained only a ferous liquor, and, accordingly, the skin appeared of that colour: which will further appear upon squeezing such red parts, which drives the blood out of them, and makes them appear white; whereas, on removing such pressure, they recover their colour, as the blood does it's place.
 3. The yellow colour of the skin in the jaundice is a further proof of this assertion; where the yellow bile is diffused thro' the vessels of the *cutis*, and appears thro' the *epidermis*; but no one will imagine, that the *epidermis* itself receives this viscid bile into it's vessels; which are so small, that many accurate Anatomists, as *Morgagni*, have denied it to have any vessels at all; and the most accurate could never shew them.
 4. The pale look of those, in whom the blood is viscid, or circulates with little force, shews, that the *epidermis* then transmits the colour of the juices and fibres below it, which are then unmixed with red blood.
 5. The same is manifest in those whose blood is poor and ferous, as the leucophlegmatic, &c. in whom the *epidermis* transmits the colour of the water or serum under it. From hence it appears, that the *epidermis* is a transparent membrane, which easily shews the colour of the parts under it, in the same manner as the *cornea* of the eye transmits the colour of the *iris*. But this will appear more plainly, from some considerations below; where we shall assign the cause of this pellucidity; and shew, that the numbers of pores in the *epidermis* necessarily make it transparent; and that the smallness of the particles, into which it is divided by them, make it unfit to reflect any rays of light, and consequently to manifest any colour of it's own. But to this some, perhaps, may object, that the *epidermis*, when taken off of the body, appears white, and consequently reflects such rays of light. But then we must consider, that it's pores and fibres are much contracted, and it's substance, consequently, rendered more dense, and fitter to reflect any colour; besides, it is then evacuated of those transparent limpid juices, which it before contained, from the vessels of the skin which pervade

pervade it; and which, as Sir *I. Newton* shews (a), will render any body transparent; especially since those juices, which then pervade the *epidermis*, are nighly of the same density with it; since all accretion and nutrition is from such. Accordingly we see, that when the *epidermis* is taken off of the body, it appears transparent enough for what we have assigned, especially it's external *lamina*. This may be further perceived, by holding the hands of some persons of thin skins, and much shrivelled, in a certain light; when you may perceive the colour which this membrane reflects, which is of a silver white, like all other pellucid *lamellæ*; very different from the colour which it transmits from the parts under it; of which colour likewise the scales of the *epidermis* appear, when rubbed off on black cloth, or when scaled off in a leprosy; a remarkable instance of which Dr *Turner* affords us (b): altho' it cannot be denied, but that the cuticle may reflect some small portion of the rays of light; which, however, seem to have but little share in occasioning the colour of the body, to what those colours have, which are transmitted thro' it, from the other less diaphanous membranes under it.

PROP. II. For the truth of the first part of this proposition, we need only appeal to our senses, and examine the skins of negroes when separated from the body; when not only the *cutis*, but even the *epidermis*, will appear to be much thicker and tougher, *cæteris paribus*, than in white people. But, because the substance and texture, especially of the *epidermis*, is not a little altered in anatomical preparations, and that in such a measure as to alter the texture perhaps, on which the colour depends, by boiling, soaking, peeling, &c. let us examine the skins of negroes on their body; where they will appear, from the following considerations, to have all the properties assigned: 1. In bleeding, or otherwise cutting their skins, they feel more tough and thick, than in white people. 2. When the *epidermis* is separated by *cantharides*; or fire, it is much tougher and thicker, *cæteris paribus*, and more difficult to raise, in black than white people. 3. Negroes are never subject to be sun-burnt, or have their skins blistered by any such degree of heat, as whites are; but, if we consider, that a black body retains more heat than a white one, or any other colour, it will be very plain, that their skins must be thicker or denser, *i. e.* more cartilaginous or callous, to award off this violence of the sun's beams. 4. Altho' their skins, in some particular subjects, should not be so very thick in substance, yet in winter, when they are dry, and not covered with that greasy sweat which transudes thro' them in summer, their skins feel more coarse, hard, and rigid; as they do in ardent fevers, with a dry skin. 5. Their exemption from some cutaneous diseases, as the itch, prickly heat or *effere*, which no adult negroes are troubled with, but those of fine and

(a) Opt. l. 2. Par. 3. Prop. III.

(b) De Morb. Cutan. p. m. IV.

thin skins are most subject to, shew the thickness or callosity of their skins, which are not easily affected from slight causes. 6. But not only the thickness, but likewise the opacity of their skins, will appear, from their never looking red in blushing, or ardent fevers with internal inflammations, nor in the measles, or small-pox; where altho' the blood must be forcibly impelled into the subcutaneous vessels, yet it does not appear thro' the *epidermis*. The like may be said of their veins; which, altho' large and shallow, yet do not appear blue, till the skin is cut. 7. In the jaundice, *anasarca*, &c. the skin of negroes never shews the colour of the parts under it; altho' visible enough in the eyes: of which I lately saw a more convincing proof in some negroes labouring under a bilious fever, in whom the *serum* of the blood, when let, was of a deep bilious yellow, but no yellow colour appeared on the skin, altho' plain enough to be seen in the eyes.

From hence might be deduced one plain cause of the blackness of negroes; for, if the colour of the skin depends on what it transmits, and the skins of negroes transmit no colour thro' them, they must, for that reason, appear black; according to the known doctrine of light and colours, that, where-ever there is a privation of light or colour, there, of course, ensues darkness or blackness. But, as most solid bodies, which are not pellucid, do generally reflect some colour, which we know no black body does, we shall next inquire into the particular make of their skins, by which they are rendered incapable to reflect, as well as to transmit, the rays of light.

For a proof of this proposition, we must examine the structure of the skins of negroes more narrowly, which may be done after blistering with *cantharides*, or after a scald or burn; when their skins have appeared to me in the following manner: the cuticle, which is separated, appears nighly of the same colour on the outside, as before such separation from the body; but, on the inner side, is almost as white as the same part in white people. This cuticle is almost always, in blistering with *cantharides*, divided into two *lamellæ*; especially on the thighs, where it is as thick almost as both the skin and scarf-skin of white people: the surfaces, by which these two parts or *lamellæ* of the *epidermis* cohere, are partly white, and partly black; for you may see many black fibres pervading the inner *lamella*, and perforating the upper one, which appear like so many black spots on these two surfaces, when separated from one another; but these black spots do not appear on the inner surface of this inner *lamella*; these fibres being, as it were, contracted within the two *lamellæ*, upon the external surface of this inner one. The inner surface of the outer *lamella* of the *epidermis*, or at least of the outermost of the two into which it is divided by *cantharides*, appears to be a whitish membrane, like the other membranes of the human body; except the forementioned black spots, which appear on this likewise, and the colour it receives from it's external black surface, which appears, in some measure, thro' the inner

Corollary.

PROP. III.

The part of the skin which appears black in negroes, is the Corpus reticulare Cutis, and external lamella of the epidermis: and all other parts are of the same colour in them with those of white people, except the fibres which pass between those two parts.

surface, and makes the whiteness on it appear very superficial. This outer *lamella* is thicker and tougher, and not so pellucid, as in Whites. By scraping these *lamellæ* of the cuticle of negroes, they may be made more white, and these black spots scraped off, by which the under *lamella* will become as white as any membrane almost of white people; and several white *striæ* may be scraped off from the outer *lamella*, by which both it's surfaces will become more nighly of the same black colour: from whence the cuticle would appear to consist of, or be composed of, many different *lamellæ*, and those of different colours, so that the external one only is black; which blackness is easily scraped off from the membranes, by any thing that will abrade the *fibrillæ*; but it is not to be removed by only soaking or wringing out the cuticle in any common *menstruum*, which might dissolve and extract any juices in it; from whence, by the by, this blackness would appear to proceed from these *fibrillæ* and scales, and not from any juices. But, as these minute nervous *fibrillæ* are distributed through and all over the other coarser membranes, so they very easily render them black, by insinuating themselves into their interstices *. Under the *epidermis* of negroes, when separated in a living subject, by blistering, appears, as it were, a third membrane between that and the *cutis vera*: this is the *corpus reticulare Malpighii*, which differs from the same part in white people in two respects; for, in negroes, it is of a black colour all over the body, where they appear black; and whereas, in white people, it is of a soft, pappy, or mucous substance, and can hardly be separated but in pappy flakes, in Blacks it is separated very often, by the force of epispastics, from both skin and cuticle, and may often be peeled off, like a membrane, from the *cutis*, as the *epidermis* is from it; whilst in other places, by a less force of the epispastic, it is closely adhering to the *cutis*, as the *epidermis* itself often is: this membranous expansion is of a much thicker substance than the same part in Whites; and from this seem to proceed the black fibres, which pervade the *epidermis*, and end in it's external surface. The *cutis* itself, which lies under this black membranous expansion, and to which it is closely connected, is of a white colour in negroes, somewhat like the skin of many brown-skinned white people; but when this black *corpus reticulare* is upon it, after the *epidermis* is separated, they appear, when both connected together, of a brown copper-colour, somewhat like the colour of an *Indian* or *Molatto*; some of the colour of the white skin below being transmitted through this thin black membrane: which seems to shew, in what manner the colour of these *Indians* and *Molattoes* may be occasioned, by the colour of the white membranes under their cuticles appearing partly and imperfectly thro' them, as the white skin does thro' this *corpus reticulare*. From hence, the formation of the *epidermis* seems to be more easily shewn, and more completely deduced, than from any preparation of it in white

* *Vide Newton. Opt. p. 222. Edit. Latin.*

people. For the external *lamella* of it manifestly arises from the *corpus reticulare*, by the intervention of the black *fibrillæ*, which we have shewn to pervade the inner *lamellæ* of the *epidermis*; and this *corpus reticulare* itself arises from the subcutaneous nerves, so nicely and accurately delineated by *Eustachius* *: each of the fibres from this *corpus reticulare* seem to form, or be expanded into, a small scale, at their termination on the outer surface of the cuticle, in the same manner as other vessels of the body, where not inserted into any part, terminate in a membranous *aponeurosis*. But these do not seem to be the only parts of the *epidermis*, as the white *lamellæ* of it are plainly different from the black; for as this black *lamella* is an expansion of the nervous *plexus*, which proceeds from the nerves of the skin, so it is very probable, that each series of exhaling and inhaling vessels, which, in like manner with it's nerves, proceed from the *cutis*, and terminate here, may be expanded into a membrane at their termination; of which vessels there seem to be 3 more kinds; the *vasa arteriosa* or *exhalantia*, the *venosa* or *inhalantia*, and the *vasa excretoria*, or sudorific ducts from the glands of the skin; each of which proceed from the *plexus* of vessels or glands, of which the skin is composed; and, perforating the *corpus reticulare*, terminate in a thin membranous expansion; which, from the several *lamellæ*, or *strata*, of scaly membranes, of which *Cowper* and *Rysch* have observed the *epidermis* to be composed: so that, as the *plexus* of nerves, which form the *corpus reticulare*, is expanded over the *plexus* of veins and arteries which compose the *cutis*, to make them sensible, by their exquisite sense, of any external injuries; so the membranous expansion of these nerves, at their final termination in the outer *lamella* of the *epidermis*, is laid over the open orifices of these vessels, at their final terminations, to keep off the injuries they might be exposed to, by an immediate contact with the open air, either in obstructing their orifices, exsiccating their substance, or exhaling their liquid contents too fast, if deprived of such a cover. From hence it will appear, how far the *epidermis* may be said to be vascular or not; and that it is very probable, that this outer coverlet or external *lamella* of it, is a solid nervous expansion, enjoying no sort of vessels, any more than the ultimate *fibrillæ* of the nerves themselves can be supposed to do.

It has been the general received opinion, since *Malpighi*'s time, that the cause of the colour of negroes is a juice or fluid of a black colour, which lies between the *epidermis* and *cutis*, in some aqueous vessels, which serve to lubricate those parts; which opinion I would be willing to submit to, as others seem to have done, as far as the best authority ought to go; but *Malpighi* seems to propose this, rather as a probable opinion to be more thoroughly examined, than as an established one to be confided in †: and I must own I was formerly of that opinion,

PROP. IV. The colour of negroes does not proceed from any black humour, or fluid parts contained in their skins; for there is none such in any part of their bodies, more than in white people.

* Tab. Anat. XXI. XXII. XXIII.

† Epist. Anat. Edit. Lond. p. 26.

thinking that the sulphureous oily parts of their fluids were more attenuated and exalted by the heat of the sun, and so rendered black, as we know happens to oil when well boiled, and to our tongues in ardent fevers. But this opinion needs only to be well and more thoroughly considered, to be confuted: for, if we consider the ill qualities, and pernicious effects to our bodies, of any such exalted sulphureous juices, no one will imagine that any animal can live in health, with any such fluids in his body; since all the fluids of the body constantly circulate, and communicate with each other; for such sulphureous juices seem to be the cause of black tongues in acute diseases, and of the blackness of gangrenes in some measure, which we know soon prove fatal, unless removed. Besides, these juices must be secreted from the blood, which seems to have no more disposition to turn black, in black than white people: and, as these black juices lie in the skin, it is very probable, that they might often be exhaled, especially in sweating; and might leave the skin destitute of its black colour, in some measure, at such times; which it never is, but appears rather blacker at such times, than any other. And as this humour must be secreted from the blood, and constantly exhaled and renewed again, it is very likely, that it would be often obstructed in its secretion, or altered in colour, in some morbid cases, like the other humours, as well as evaporated sometimes; which however we never see it to be. But, however well received this opinion may be, as being consistent with common notions, yet it is very unphilosophical to assign any thing as a cause, which does not appear to exist; and I believe that none will be able to shew the existence of any such black humour in the skins of negroes; for their sweat, or the water in their blisters, which would probably contain some of this black humour, was there any such thing, is as clear and white as in white people: besides that the existence of any vessels in the skin, which are supposed to contain this humour, is not well agreed to by Anatomists. Mr Cowper says, he never could find any such, altho' carefully sought for*; and none else have ever been able to shew them: for the common Anatomists take the *corpus reticulare* to be a mucous substance, as they call it, which serves to lubricate the *papillæ nervosæ*, and contains this black humour; but, altho' it is of a soft pappy substance in Whites, yet it cannot be called a fluid *mucus*, any more than the substance of the large nerves, or of the brain, from whence it arises, and which are of a softer and more mucous substance than it; besides that, in negroes, it still more resembles a solid membrane, being to be torn off like the *epidermis*; and as it arises from the nervous *involucra*, or expansion of nervous fibres, it probably contains no fluids, or none but what are most pellucid and clear. Moreover, whatever this supposed black humour may be, or in whatever it may be contained, it must be opaque, and the fibres or vessels of the *epidermis* must be transparent, to shew this colour thro'

* Tab. Anat. IV.

them; as we have shewn, that the skins of negroes, but not of Whites, are opake (*Prop. I. II.*): but it is very probable, that none of the subcutaneous juices are opake, they being the ultimate secretions of the subtilest fluids of our bodies, which, instead of being opake and black, must rather become pellucid and white; *Etenim corpora omnium opacissima, si partes ipsorum in summam usque tenuitatem comminuantur, evadunt continuo plane perfecteque pellucida* *. And the skins of negroes, being more callous or cartilaginous, must contain rather more subtile, and pellucid aqueous juices, than those of white people. Furthermore, if there was any such thing as a black humour in the skins of negroes, no doubt but it might be drawn out by some means or other; but, altho' I have macerated the skins of negroes, and particularly the *epidermis*, in warm water, which readily dissolves the juices of the body, yet I never could extract any black juices from them, by any such maceration, or even by a more powerful expression; no more than Mr *Litre* (as is related) could do, by more powerful dissolvents. Nothing seems more likely to extract this supposed black juice, than the action of fire, or *cantharides*, upon the vessels which contain it, which abrade and tear the vessels and fibres of both the cuticle and *corpus reticulare* from the skin, but leave them both as black as ever they seem to have been, altho' they would, no doubt, extract whatever juices they might contain; as we see plainly they do, by the large blisters, raised by such applications, from the abraded vessels spewing out their liquid contents; in which blisters there are no tokens of any black humours in negroes more than in Whites, as I have often found upon proper trials; altho' if there was any such black humour contained in their subcutaneous vessels, there is no doubt but it would appear, in some measure, in the water of their blisters, as well as the yellow bile does, when diffused thro' the blood, and upon the skin, as I had lately some opportunities of observing. But it seems to be a total overthrow of this opinion, that the blackness of the skin in negroes should proceed from the *corpus reticulare*, and *lamella externa epidermidis*, as we have proved, *Prop. III.* For I believe that none, who maintain this opinion, will or can, with any pretence of facts, or shew of reason, suppose, that these nervous, scaly, and dry parts contain any such juices, if they contain any at all, as it is most probable they do not, being the ultimate expansions of nervous *fibrillæ*, at their final terminations; for no other parts appear black, but these two; whereas did this blackness proceed from any coloured juices, the other parts or membranes of the skin and cuticle, which seem more fit to receive such coloured juices, would appear black likewise. From what has been said, we may see how well their opinions are grounded, who derive the cause of the colour of negroes, from an addition of the bile, or other atrabilious humour, as they are pleased to talk.

* Newton *Opt.* lib. II. Part 3. *Prop. IV.*

PROP. V. The epidermis, especially it's external lamella, is divided into two parts, by its pores and scales, 200 times less than the particles of bodies, on which their colours depend.

Sir *I. Newton* informs us, that the particles of bodies, on which their colours depend, are about 600 times less than those which can be discerned with the naked eye; *Opt. lib. II. Part 3. Prop. 7.* But *Leeuwenhoeck* shews, that a portion of the *epidermis*, no bigger than what can be discerned with the naked eye, is divided into 125000 pores; which pores must divide such a portion of the skin as can be discerned with the naked eye, into 125,000 particles; therefore each of these parts of the skin, between its pores, must be about 200 times less than those particles, on which the colours of bodies depend; for $\frac{125000}{600} = 208\frac{1}{3}$; not to mention, that such a portion of the *epidermis* is further divided into 250 scales, which must increase the number of parts into which it is divided. Nor will any one think, that the smallness of these parts and pores exceeds credibility, who considers, that they convey the minutest particles of our last digestions; and were they even large enough to convey the particles of many waters, it is very probable that all our fluids might in time evaporate through them. Nor is it any thing to our present purpose, whether these numbers be mathematically exact or no; all that I would endeavour to prove, is, that the parts into which the cuticle is divided, are less, in some proportion, than the particles of bodies on which their colours depend.

PROP. VI.

Problem. To determine and explain the proximate cause of the colour of negroes, Indians, white people, &c. from the foregoing propositions.

We have shewn above *Prop. I.* that the colour of white people depends upon the colour which the *epidermis* transmits, and not on what it reflects; this pellucidity of bodies proceeds from the number of interstices between the particles which compose them, and the extreme smallness of those particles; for in order to render a body of any colour, or fit to reflect the rays of light, it's particles, and the intervals between them, ought not to be less than a certain magnitude*; else they become incapable of making any reflexions, from their common surfaces, *i. e.* of appearing coloured: but, by *Prop. V.* the cuticle is divided into parts, and pores or intervals between these particles, far less than those on which the colours of bodies depend; that is, too small to reflect any rays of light from their common surfaces, or to appear coloured from such reflected rays: but, as such porous bodies are always transparent, so the *epidermis* is transparent enough, to shew any colour reflected from the parts below it: so that we must look upon the *epidermis* of white people to be a transparent pellicle, of too subtile or rare a substance, and too minutely divided, to reflect any rays of light from it's surface; but consisting of numbers of pores, which readily transmit those rays, through it's thin and rare substance, by which it shews the colour of whatever parts are below it; on which the colour of white people depends. But, as there are numbers of scales, or several *strata* of scaly *lamellæ* in the *epidermis*, so this transmission of the rays of light, from the subcutaneous parts, must be imperfect, some of these rays being

* *Newton Opt. Prop. IV.*

being intercepted in passing through the several *lamellæ*; and the thicker the cuticle is, *i. e.* the more there are of these *lamellæ*, or the denser their texture, the more the light will be intercepted in passing them, and the more the colour of the skin will degenerate from the pure white of the membranes below it. This is agreeable to experience; for Mr *Cowper* tells us, in his *Anatomy*, that the thickness of the skin proceeds from the number of the *strata* or layers of scales which compose it: and we may daily observe, that those who have such thick and coarse skins, are never of so perfect and pure a white, as they who have a thin and fine skin (as * *Cowper* observes). But the reason why such thick-skinned people appear of a yellowish or tawny colour, will be plain, from *Newton's* observations (*Opt.* lib. II. p. 1. *Obs.* 9. and 20.); where he shews a faint yellowish colour to be the one that proceeds from an imperfect transmission of a white; for no one can say, but that both the internal membranes and humours of such swarthy people are of the same colour in time of health with those of the perfectest white skins, as well as they are in negroes. And this seems to be the cause of the pale yellow of dead bodies, whose skins are not perspirable, and, consequently not so transparent, as in a living subject. From this account of the cause of the difference in colour among those people, that are white, we may account for the cause of the colour of *Indians*, and other tawny people, who seem to differ from one another in colour, and from white people only in degree, as they have more or less of this tawny yellow proceeding from the imperfect transmission of a white in their colours: thus if we proceed from the swarthiest white person to the palest *Egyptian*, from thence to the fairest *Mustee*, *Molatto*, *Moor*, &c. to the darkest *Indian*, we may plainly see, that they differ from one another only *secundum majus vel minus*, according as they have more or less of the original white in their colour: and as we have shewn this tawny colour in white people to proceed from the thickness of their skins, obstructing the transmission of the rays of light; so it is very plain, that the same tawny colour, in these other tawny people, which seems to be of the same kind, but different in degree, must proceed from a like cause, that is, the thickness of their skins; and, accordingly, it will be found, that all such people have skins of a thickness proportional to the whiteness or darkness of their colours. The particular manner in which this opacity, or imperfect pellucidity of bodies is brought about, Sir *I. Newton* explains to us (*Opt.* ib. *Prop.* II.); where he shews, that the opacity of bodies depends upon the multitude of reflexions that are made in their internal parts; but it is very plain, that the thicker the skin is, the more reflexions the rays of light must suffer in passing through it, by which they will be extinguished, in proportion to the number of such reflexions; that is, the more opaque, or less white, it must appear: so that, although the particles, of which

white

* *Cowp. Anat.* TAB. IV.

white and dark skinned people are composed, may not be very different from one another, as they seem not to be; yet a greater number of such combined particles, or more *strata* of them, in thick skins, and the smallness of their intervals in skins of a dense texture, will increase the number of reflexions made in their internal parts, or the opacity of them; which renders them less white, since their whiteness proceeds from the number of the transmitted rays. In the same manner, by which we have accounted for the colours of tawny people, may we account for the colour of those that are black: for, if the skin appears darker and darker coloured, the more the rays of light are intercepted by it, of course it must follow, that when the rays of light are entirely intercepted by a body of the same structure, (which the skins of negroes seem to be) it must be quite black; for blackness always proceeds from a suffocation of the rays of light, as those versed in the doctrine of light and colours are well acquainted with: but we have shewn above (*Prop. II.*), that the skins of negroes transmit no colour or rays of light thro' them, on account of the thickness of their substance, and density of their texture, in the same manner as they are imperfectly transmitted in some white or tawny people, whose skins appear to be of the same structure with those of negroes, and to differ from them in nothing, but in degree of thickness and density, and in colour; which different density may therefore probably be one, if not the only cause of this difference of colour. So that the thickness and density of the skins of negroes seems to be the grand cause of their colour, in the same manner as it is of *Indians*, *Moors*, &c. Which may be further confirmed by the following considerations: 1. in the cicatrices of their ulcers, the thin and tender new-formed skin appears whitish, nay, perfectly white, in some, especially on the shins, or those places where these cicatrices are thin; but, where the skin is thick, or when these cicatrices turn more thick and callous, they grow blacker in proportion; as in those places where the scars grow thicker than the rest of the skin, they are likewise blacker. 2. The colour of the water, contained in the blisters of white people, may be plainly seen thro' their cuticles, especially if tinged yellow, which cannot be perceived in the blisters of negroes: a plain proof, that their cuticles are not transparent, as those of white people are. 3. Infant-negroes, whose skins differ from adults only in the thinness and rarity of their texture, look whitish, in comparison to adult negroes; but grow black, as their skins turn thicker and denser. These infant negroes, labouring under an *icterus*, look of a yellow colour, all over their body; which the adult do not, except in the eyes, as I had lately an opportunity of observing: a plain proof, that the colour of the skin proceeds from the colour which is transmitted thro' it; and that the skins of adult negroes transmit no colour of any sort. 4. But that the thickness of the *corpus reticulare*, the part which appears black in negroes, by *Prop. III.* may and does make it black, *Malpighi* gives us an instance in the said part, in the tongue of a beef,

in which it appears black, on the middle of the tongue, where it is thick; but is white on the edges and cheeks, where it is very thin *. As for the manner in which this blackness or opacity is occasioned by a thick or dense skin, it will appear from what has been said about the skins of tawny people: and it is very easy to conceive how the rays of light are intercepted, in passing through the thick and dense skins of negroes, which easily pervade the thin and rare cuticles of Whites. But, as the skins of negroes are of a denser texture than those of Whites, they will be more apt to refract the rays of light; for the denser the body, the greater the power of refracting †; and the greater the refraction of any body is, the more apt it will be to absorb the rays of light; which is another property of opaque bodies, by which they become black: *Porro quo corpora videantur nigra, necesse est multi radii intercipientur, restinguantur, et inter ipsa intercidant* ||. Two properties are generally assigned to all black bodies, to be opaque and porous: now the skin is well known to be porous; and we have shewn, that the skins of negroes are opaque; to these we may add a third necessary property of black bodies, the minuteness of their particles: for, as Sir I. Newton shews, *Ad nigrorem exhibendum, particulae adhuc minores esse debent omnibus illis, quae colores cujuscunque modi exhibent: nam particulae omnes majusculae plus reflectunt luminis, quam ut nigrae possint videri* §; which smallness of particles we have above demonstrated in the skin (*Prop. V.*); and it is probable, that, in negroes, the particles of the skin between the pores are smaller than in Whites; as the pores themselves are so: from which smallness of it's particles, the skins of negroes cannot reflect the rays of light; another cause of their being black. So that, from the whole, we may conclude, that the proximate cause of the colour of negroes is threefold; the opacity of their skins, proceeding from the thickness and density of their texture, which obstructs the transmission of the rays of light, from the white and red parts below them; together with their greater refractive power, which absorbs those rays; and the smallness of the particles of their skins, which hinder them to reflect any light. *Q. E. I.*

I doubt not, but that those who are used to account for the colours of all consistent bodies, from the reflexion of those colours from their surfaces, which is the common way of accounting for the colours of solid substances, without noticing the colours of pellucid bodies; and are used to derive the different colours of the skin from differently coloured fluids, appearing through it's pellucid vessels, as in most morbid cases; may not be entirely satisfied, at first, with this account of the different colours of human bodies being occasioned, as I have affirmed, by the more or less perfect transmission of the rays of light thro' their cuticles; which, from the different thickness and density it is observed

* Malpighi *de Lingua*, p. 15, 16.

|| Newton *Opt. Prop. VIII.*

† Newton *Opt. Prop. X.*

§ *Ibid. Prop. VII.*

to be of, is more or less fit to transmit those rays; the more of which being intercepted, the darker the skin appears; till, at last, being entirely intercepted by the thickest skins, as those of negroes are, it appears, of no colour, or looks black. But let such consider, that if the whiteness of the skin, in Whites, proceeds from the thinness of it's substance, rareness of it's texture, the numbers of it's pores, and minuteness of it's particles, by which structure it transmits the rays of light; as is very probable, from this and Prop. I. there will be no reason to suppose, but that the colours of *Molatto's*, *Indians*, and negroes, proceed from a similar or like cause, and not from any particular new-induced texture, by which their skins may become less able, or entirely incapable, to reflect the rays of light; since their colours seem to differ from one another only in the degree of one and the same colour; and the structure of the skin seems to be entirely the same in both, except in the different density of it, which seems more fit to induce a change of colour from the different transmission of the rays of light, than from the different reflexions of it: for the ultimate component *strata* of scales, of which the cuticles of both white and black people are composed, seem to be entirely the same in both, or, at least, but little, if at all, altered in their reflecting powers, whatever they may be in their opacity, or pellucidity, from the different assemblage and combinations of them: add to this, that the colours of even the fairest skins look more like a colour proceeding from transmitted, than reflected light; being more languid, or not so brilliant, as those colours that proceed from such reflected rays: for this reflexion, even from the *epidermis* itself, is of a shining silver white, as we have above remarked. Moreover, the denser and thicker the body is, whose colour proceeds from reflected light, the more vivid it's colour will be, such bodies being most capable of reflecting the greatest quantity of the rays of light; but we have demonstrated above, that the thinner and rarer the skin is, the more clear and bright it's colour is; which does not therefore, in all probability, proceed from such reflected light. I know very well, that the different smoothness or roughness of the surface of bodies tends much to brighten or obscure their colours; but the darkest skins, as well as those of negroes, seem to be as sleek and smooth on their outer surfaces, as those of the fairest colour. But, as this cause of the diversity of colours, which we have assigned, seems to be the most simple, and easily effected, like the operations of nature constantly observable in other things, so it is most consistent with the doctrine of colours in many other respects; for, were not our colours to proceed from the parts under the *epidermis*, which are always of the same colour in all people of the most different complexions; but the skin was to be a solid opaque body, which reflected our colours from it's surface, like most other coloured bodies; we should then, in all probability, have people of all the different colours in the rainbow, and that in one and the same nation; for the skin is seldom observed to be of the same thickness in different persons of the same

same nation: but Sir *I. Newton* shews us, that those bodies, whose colours proceed from reflected light, when they alter their thickness and density, change their colours, not to a more or less perfect one of the same kind, but to one of a different kind *: thus a thin plate of talc, whose colour proceeds from the light reflected from it's surface, alters it's colour, according to it's thickness, to all the different primary colours; and one of these plates, of a pale yellow colour, laid over another one of a blue colour, turns of a deep purple †: this would probably be the case of our skins, was it's colour to proceed from reflected light; since there are many different *strata* of scaly plates laid over one another in the skins of different persons, even of the same nation, as Mr *Cowper* informs us ‖: and every one may perceive, that the skins of different persons are of different densities; and much more those of different nations and complexions: but when the diversity of our colours proceed from the different transmissions of the rays of light, from one and the same coloured bodies, such different thicknesses of our skins will only make our colours vary from one another *secundum majus vel minus*, only in the degree of one and the same colour; by which alone will negroes, *Indians*, and white people, or each of a sort, differ from one another in colour; and, consequently, their different colours will proceed from causes more uniform and alike, agreeable to the exact symmetry of nature in the variations of other things of the same kind; so that, however different, and opposite to one another, these two colours of Black and White may appear to be to the unskilful, yet they will be found to differ from one another only in degree; since whiteness proceeds from a reflexion or transmission of the rays of all colours; but blackness is brought on, by an extinction or suffocation of those same mixed rays, which, probably, in the black bodies, are reflected or transmitted in some small quantity, as they are in larger or smaller quantities, in proportion to the whiteness of bodies §: and hence it is, that one of these colours is more easily changed into the other, than to any other colour; and where any body loses it's white colour, it of course turns black, without any other cause concurring, but a bare loss of it's whiteness **. From whence we may justly infer, 1. That there is not so great, unnatural, and unaccountable a difference between negroes and white people, on account of their colours, as to make it impossible for both ever to have been descended from the same stock, as some people, unskilled in the doctrine of light and colours, are very apt too positively to affirm, and, without any scruple, to believe, contrary to the doctrine (as it seems to be) of the sacred pages. 2. That the *epidermis*, besides it's other uses, tends to preserve the uniformity of the colours of people throughout the world.

* *Newton Opt.* p. 195, &c.

† *Idem ibid.* p. 196.

‖ *Anatom.* Tab. IV.

§ *Newton Opt.* ubique.

** *Idem ib.*

PROP. VII.

The influence of the sun, in hot countries, and the ways of life of the inhabitants in them, are the remote causes of the colour of Negroes, Indians, &c. And the ways of living, in use among most nations of white people make their colours whiter, than they were originally, or would be naturally.

My design in this essay was, not to treat fully of every thing relating to the causes or effects of the colours of people, but only to inquire into the particular make and structure of the skin, by which it comes to appear of so many and so diverse colours in the different people of the world; and to explain, from *Newton's* doctrine of light and colours, in what manner these different colours are produced from this particular make and structure of the skin; which is, what I take to be, the grand question relating to the unknown cause of the colour of negroes; which has been often attempted to be determined, but with little satisfaction to the learned: for which reason I have been more particular and explicit in delivering any thoughts about it, as I have never known or heard of any such to have been delivered by any others. As for what relates to the remote causes of the colours of negroes, it has been generally supposed, although not universally believed, that the power of the sun in hot countries is the principal, if not the only, agent in producing this effect: but, as the authors of this opinion seem not to have understood what effect or alteration is produced in the make of the skin, in order to render it black, so they have not been able to satisfy any one in this point, and far less to vindicate their opinion from many material objections; for it would be very difficult, if not impossible, to shew or to prove, in what manner the sole effect of blackness is occasioned by the power of the sun, but not so difficult to shew how it may make the skin thicker or denser; which we shall endeavour in the next place to do, by shewing in what manner the power of the sun is able to cause that thickness of skin, which we have assigned as the immediate cause of it's black or tawny colour. But as this subject is much plainer, and more obvious, than the other, concerning the make of the skin, or the immediate cause by which it's blackness is occasioned; so I shall not dwell long upon it, but only give the principal heads of these arguments, which serve to prove this proposition: which arguments are of two sorts: philosophical and historical; the first of which I shall chiefly touch upon, being what are least understood, or, at least, hardly to be met with any where else. The proof of the first part of this proposition will consist, chiefly, in shewing what effect of the sun it is which deprives the skin of it's white colour, rather than what it is which causes it to be black; for, to prove the cause of blackness, is the same as to prove a negative; blackness being a negative with regard to colour. The skin then is deprived of it's white colour by the force and influence of the sun, these 4 ways.

1. By being rendered opake, from a dissipation of it's more aqueous and pellucid juices. The known effect of the sun's heat, and which will render all bodies opake: *Nam corpora ea, quæ sunt maxime pellucida, poterunt, occultorum suorum meatuum evacuatione, satis opaca evadere**.

2. By a concretion of it's vessels and glandules, from this dissipation of

*-*Newton Opt.* 16. PROP. III.

of their aqueous contents, which renders the skin both thicker and denser, or more callous or rigid. For the skin being designed as a defence to the other subcutaneous parts, as the *epidermis* is to the *cutis*, they both wonderfully accommodate themselves to the nature and force of external injuries, so as to become capable to defend the body from them; as we see in Smiths, &c. constantly used to handle hot and hard things, who have the skin of their hands become so thick and hard or cartilaginous by it, as to be able in time to handle even hot irons: and thus it is, in a great measure, with the skins of negroes, *Indians*, &c. constantly exposed, and generally naked, to the scorching heat of the sun in a perpetual summer.

3. By a new accretion of many new membranes, which render it thick and opaque. For the sun-beams act as a vibrating force, or external friction, upon the skin, which derives fresh supplies of juices to it; by which new membranes, or *lamellæ*, are formed, in the same manner as the *epidermis* is renewed when abraded, which is very soon and easily done. This is the sense of a very great Philosopher; *Calor solis homines quibusdam in regionibus nigricante colore tingit, ut in Æthiopia, Guinea, &c. Non esse illud ignis effectum ostendunt vitrarii, continuo ad ignem occupati. Ratio, fortasse, quod ignes in sanguinem & spiritus agat ut exhalent, hominesque sic reddet pallidos & subfuscus: benignior autem solis calor sanguinem duntaxat in corporis extimas partes prolicit, concoquitque eum potius quam eliquat* *. Which derivation and concoction of the humours on the surface of the body must occasion a thickness of their skins, as well as of their lips, and other muscles, especially of their face.

4. By increasing those parts or principles, in the composition of the *epidermis*, which have the greatest refractive powers. As the terrestrial, and fixed saline; but, especially, the tenacious sulphureous, which refract and absorb light more strongly than any other substances†; whilst the more transparent and pellucid principles, as the aqueous, spirituous, and volatile saline, are evaporated by the heat, which causes the other more fixed principles to be accumulated in greater quantities, and combined in larger collections; and these particles, being likewise more comminuted by the sun, will on that account be black, as happens to oil when well boiled.

From what we have said above, about the immediate causes of the colours of the skin, it will appear, that these several effects of the sun's heat contribute to make it of a darker colour; and no one will doubt, I believe, but that all of them, conspiring together may make it quite black. To these, perhaps, might be added another effect of the sun's power, a peculiar *necrosis* of the *epidermis*, occasioned by the forcible vibrations, contractions, and exsiccations of it's fibres by the sun-beams, which cause it to turn black, as these, or the other parts, do by the heat of an inflammation or a fever, in gangrenes, black tongues, &c. from whence
only

* Bacon *Hist. Natur. Cent. IV.* 399.

† Newton *Opt. Prop. X.*

only the nervous parts of the skin come to be black, and more hard and callous, and less pellucid, than the rest; and the skins of negroes, besides their callosity, become more insensible than those of Whites. But as there are many degrees of whiteness and blackness in the colours of the people in the world, depending upon the different densities and thickneses of their respective cuticles, as we have above shewn, it may not be improper, in the next place, to inquire into the more peculiar causes of this diversity, which will be found to be such as increase or diminish the power of the sun's heat, or it's influence on the body; by which the only material objection that has been brought against this proposition may be answered; *viz.* that the sun's heat is not the cause of negroes, because several nations of people, in the same latitude with the negroes in *Africa*, are not made black by it. The causes of this diversity may be referred to two heads; *viz.* 1. The nature and temper of the country. 2. The ways of living in it. Under the first may be included the following particulars:

1. The nature of the soil, and situation of the country, with regard to mountains, waters, &c. which very much alter the power of the sun's heat; for the differing degrees of heat; and cold, in different places, depend, in a great measure, upon the accidents of the neighbourhood of high mountains, whose height exceedingly chills the air brought by the winds over them; and of the nature of the soil, which variously retains the heat, particularly the sandy; which, in *Africa*, *Arabia*, and generally where such sandy deserts are found, do make the heat of the summers incredible to those who have not felt them; as the learned Dr *Halley* has remarked. Whence it will appear, that the heat or influence of the sun is not always the same in the same latitudes, as they imagine who start this objection to this proposition; but that in *Africa*, where the people are black, the soil is as intemperately hot as the climate, occasioned by the scorching heat of it's sands, according to the just account of *Lucan*,

—per calidas Libyæ sitientis arenas;

agreeable to the accounts of all travellers and historians, especially those who talk of it's inland parts, where people first began to turn black. This heat of the soil must much increase the heat of the sun, and it's power upon the body: and if the sun is the cause of blackness, must make the people blacker in such places than any where else; which we see to be true of the negroes in *Africa*, who are much blacker than the *Indians* of *Asia*, or *America*, who live in the same climate, but inhabit more temperate countries. This power of the sun will be much increased in such sandy soils,

2. By the scarcity, if not entire absence, of large, spreading, succulent plants; which afford, in other moist and more fertile soils in hot countries, agreeable cooling shades, or a moist cool atmosphere, from their exhalations, which take off much of the scorching heat of the sun; which *Lucan* observes to be the case in *Africa*:

Hoc tam segne solum raras tamen exerit herbas.

3. The

3. The want of water must much increase the heat of the body, if not of the sun; and conspires to the same effects, as the more immediate heat of the sun itself. This is well known to be the case in *Africa*, from the many caravans that perish for want of water in travelling thro' its midland parts (*vide* Geog. Nat. Leo *Hist. Africæ*; as well as the account we have of *Cato's* march thro' it, and many other travellers): besides, it rains so seldom in many places of *Africa*, as to make it generally believed, formerly, that it never rained there at all; which must much more exsiccate the body, and parch the skin more powerfully, in these sandy regions, where no rain ever falls, but at a certain season or two in the year, than in more temperate regions, although in the same latitude.

2. The ways of living in many hot countries, particularly in *Africa*, do very much contribute to increase the influence of the sun upon the body, or to thicken and harden the skin, upon which its blackness depends. These customs are,

1. The custom of going naked among most of the nations of *Africa*, especially those that are black, both in former days, as well as at present.

2. Living not only without cloaths, but also without houses, in a very barbarous and rude manner, little better than the wild beasts; as the *Casars* do at this day all over *Africa*; and was the custom of the *Nomades*, *Troglodytæ*, *Numidians*, and many other barbarous nations of old *.

3. The custom of wandering up and down in these sandy deserts, in the scorching heat of the sun, stark-naked, with no house or cool shade to retire to, nor water to refresh themselves with, or cool their bodies in:

Nulla domus; plaustis habitant; migrare per arva

Mos, & errantes circumvectare penates.

4. The custom of most people in hot countries of anointing their bodies with some greasy, and unctuous epithems, to defend their skins from the scorching heat of the sun, will be found likewise to increase the darkness of their colours.

On the contrary, the customs and ways of life in use among the *Europeans*, and other nations of fair complexions, contribute to render their skins whiter than they otherwise would be, or than they were, in all probability, originally. These customs seem to be, an almost constant confinement, or rather imprisonment, from the open air; warm and soft cloaths; warm beds; sitting by fires; the custom of bathing much in use formerly; a more succulent and nourishing diet; excess in strong liquors; frequent sipping of warm thin liquors; and, in general, more luxurious and effeminate lives; all which, with the absence

* *Lud. Hist. Æthiop. lib. 1. cap. 14. Plin. Hist. &c.*

of the sun, or defences from it, tend to soften, moisten, and relax, the fibres of the body, and to render the fluids more thin and watery; and, consequently, the membranes composed of them, such as the skin is, must be more clear and transparent; on which, we have shewn, it's whiteness depends; and, accordingly, we constantly see, that people of such constitutions, or ways of life among us, are always the whitest. We might, indeed, consider the effects of cold upon the skin in these northern climes, where the people are white, were it not that those, who are the fairest among them, are the least exposed to it, and seldom or never feel it's effects; but the whiteness of their complexions seems rather to be occasioned by muffling themselves up against the cold, than from being exposed to it's influence: for, as the *cuticula* is a sort of cloathing to the other membranes of the body, and, by preserving the whiteness of them, serves, besides it's numerous other uses, to keep up an uniformity and harmony in the colours of people; so there is no doubt, but that the cloaths wherewith we cover it, preserve it's whiteness, or render it whiter, as every fair-one knows: so that the different customs of different nations, in this respect, will tend very much, besides other causes, to make that alteration and diversity so observable in their complexions. So that it seems to be but a small objection, if any at all, to this proposition, That the natives of *Canada*, although but a cold and northern clime, are of a swarthy colour, whilst others, in the same latitude in *Europe*, are white; for the customs and ways of life of these last seem very much to increase, if not occasion, the whiteness of their colours; whereas the hard lives, and savage customs, of these *Canada Indians*, especially their going stark-naked all over *, seem to have no tendency to soften their skins, or refine their complexions; not to mention their custom of intermixing with the captive women of southern nations. But as the *Canada Indians* are the most northern, so they are the palest, of all *Indians*. Hence it will appear, that the power of the sun's heat in hot countries, and it's more immediate application to the body, or the increase of it's force, by the nature of the soil, or ways of life, is the remote cause of the blackness, and the different degrees of blackness, of the inhabitants of the torrid zone: whereas the luxurious customs, and the effeminate lives, of the several nations of white people, in the northern climes, are the remote causes of their respective fair complexions. Q. E. D.

Altho' I have gone much further in this epistle, than I expected or intended, or my time would well permit, or the nature of an epistle would well bear; for which reason I have passed over what others may have imagined to be the cause of the colour of negroes, and have abridged every thing as much as I well could; yet I cannot but take notice, that as the knowledge of any cause is always conducive to investigate, and account for, many effects depending on, or proceeding

* La Hontan. *Voyage*, vol. I. lett. 16. vol. II. cap. 1.

from, these causes ; so this account of the structure of the skins, and colour of negroes, and other tawny people, if duly and attentively considered, will lead us to the knowledge of many intricate *phænomena*, in nature and diseases, either unknown, or not so easily accountable for before ; whose consequences, as they are of the greatest concern to us, so I am unwilling to pass them over entirely in this place ; although a particular discussion of each would require so much more room, as to make the tail of this discourse bigger than the body. I shall, therefore, only give you the principal heads of what may be rationally deduced from the foregoing propositions, as so many corollaries from them, reserving the particular discussion of each to another opportunity ; which as I have already framed to myself, from many convincing observations, which I have made here in *Virginia*, so I shall present you with them, if you find these my reasonings and observations either agreeable to the rules of sound philosophy, or of public utility.

White spots on the skins of negroes are as common, and proceed from *Coroll. I.* the same causes with red spots on white people ; viz. a distention, dilatation, and consequent rarity or pellucidity, of the *vascula* of the *epidermis* : from whence the physical causes of the total whiteness of some negroes, at their birth, may be accounted for *.

The hair of negroes becomes short, stiff, and frizzled, from the exsiccation of it's substance, and it's excrementitious moisture, by the heat of the sun ; together with the thickness and density of the *pericranium*, which hinders it to be further protruded. *Coroll. II.*

Many morbid discolorations of the body proceed rather from a preternatural thickness and density of the membranes of the skin, than from any humours lodged in them ; as is commonly supposed ; and may be accounted for in the same manner, as the different complexions in time of health. *Coroll. III.*

The bodies of Whites are more perspirable, than those of negroes, but perspire less in hot weather, and more in cold. *Coroll. IV.*

White people are most healthy in cold, and black or tawny people in hot countries ; each being subject to disorders, on a removal to these respective climes. The causes of the diseases of white people in hot countries are often opposite and contrary to such as proceed merely from heat, which exalts the fluids, exsiccates the solids, and quickens the circulation, occasioning severe acute diseases ; but the thin and rare skins, and large pores of white people, make them subject to too large cutaneous evacuations of the most subtile and active fluids ; by which the body is infeebled, and comes to be in an imbibing state, both on it's external and internal surfaces ; and too readily imbibes the humidity of the air and aliment, without a previous digestion ; causing a cold and humid, rather than a hot and dry, state of the body ; from whence proceed their lingering acute, and obstinately

* *Vide Hist. Caricleæ apud Heliodor. of which I have seen an instance in Virginia.*

chronical, maladies, more frequent in hot countries than the former, especially among the Whites. Negroes, notwithstanding their harder usage, are more apt to have their perspiration obstructed in cold weather, and contract fevers from thence; whereas, in hot weather, their thicker hides serve as a coat, to keep off the power of the sun, and preserve the body against the moisture of the air, so remarkably great, and very pernicious, in all hot countries, especially at certain seasons, which are always sickly. Hence, white people should be best cloathed in hot weather, and Blacks in cold; a thing much neglected in *Virginia*, though the cause of one half of the untimely deaths of both sorts of people in it.

Coroll. VI.

The perspirable matter of black or tawny people is more subtile and volatile in it's nature; and more acrid, penetrating, and offensive, in it's effects; and more of the nature, and more apt to degenerate to a contagious *miasma*, than the milder *effluvia* of Whites. The contagion of pestilential fevers proceeds from a subtilization and volatilization of the perspirable humours, by the effects of a preceding fever, as often, if not more often, than from any external putrefaction, or mineral exhalation. Hence this acrimony of the perspirable humours of black and tawny people makes them subject to malignant and pestilential fevers, from the same causes which breed only putrid benign fevers among Whites; and in them these fevers are more apt to turn contagious, as they themselves are to be infected with such contagion. From hence seem to have proceeded the first seeds of the measles and small-pox, with the *African* or true plague. From hence likewise proceeds the rank smell, or peculiar *fætor*, of dark-skinned people.

Coroll. VII.

This acrimony of the perspirable humours, with the thickness and density of the skins of black and tawny people, or imperspirability of their bodies, makes them subject to many severe cutaneous diseases, accompanied with a contagion, which white people never feel, but by infection from them; and then these diseases appear in other shapes, with milder symptoms, than in the dark-skinned people which breed them. These diseases, which I have observed among them, may be referred to the *Elephantiasis Græcorum*, or *Lepra Arabum*, two species of which are called, the *yaws*, and the *joint-evil*; with some others, not named, appearing in obstinate subcutaneous ulcers. But the *Elephantiasis Arabum*, to which the negroes are likewise subject, is not a cutaneous distemper, as has been thought, but a peculiar kind of cachexy, accompanied with an atrabilious cacochymy, as in those afflicted with the hæmorrhoids; that being much the same distemper in the legs, as this is in the hæmorrhoidal veins. The peculiar diseases of white people analogous to these of the Blacks, and which the Blacks never have, are the *Lepra Græcorum*, at least with furfurous desquamations, the itch, scurvy, effere, and some smaller ones of that kind. This cutaneous malady of the negroes, called the *yaws*,
laid

laid the first foundation of the *Lues Venerea*; which became to differ from it only by the part affected, and the particular manner of receiving the infection, after being transplanted into another colder clime, on people of a different complexion; the virulent acrimony of the cutaneous contagion being inviscated, and consequently mitigated, by the *semen* which received it; the subtiler parts of the contagion being likewise exhaled in the white people, on account of the perspirability of their bodies, although the distemper was driven more on the internal organs, upon account of the coldness of the climate; and so appeared to partake less of a true cutaneous malady, after this *Lues Venerea* was first propagated to *Europe*. Hence it is, originally, a cutaneous malady, only to be cured as such; the venom which attends it, and gives rise to it, being to be evacuated, most surely and effectually, by the pores of the skin, as it was originally bred by the acrid *effluvia* which pass thro' them. Hence the nature, origin, progress, alterations, and different success of divers methods of cure of this *lues*, may be accounted for; and the most rational methods of cure deduced.

From what has been said about the cause of the colour of black and white people, we may justly conclude, that they might very naturally be both descended from one and the same parents, as we are otherwise better assured from Scripture, that they are*; which may remove the scruples of some nice philosophers on this matter, who cannot or will not believe even the Scriptures, unless it be so far as they can be made agreeable to their philosophy: for the different colours of people have been demonstrated to be only the necessary effects, and natural consequences, of their respective climes, and ways of life; as we may further learn from experience, that they are the most suitable for the preservation of health, and the ease and convenience of mankind in these climes, and ways of living: so that the black colour of the negroes of *Africa*, instead of being a curse denounced on them, on account of their forefather *Ham*, as some have idly imagined, is rather a blessing, rendering their lives, in that intemperate region, more tolerable, and less painful: whereas, on the other hand, the white people, who look on themselves as the primitive race of men, from a certain superiority of worth, either supposed or assumed, seem to have the least pretensions to it of any, either from history or philosophy; for they seem to have degenerated more from the primitive and original complexion of mankind, in *Noah* and his sons, than even the *Indians* and negroes; and that to the worst extreme, the most delicate, tender, and sickly. For there is no doubt, but that *Noah* and his sons were of a complexion suitable to the climate where they resided, as well as all the rest of mankind; which is the colour of the southern *Tartars* of *Asia*, or northern *Chinese*, at this

Coroll. VIII.

* Genes. III. 20. IX. 19.

day perhaps, which is a dark swarthy, a medium between black and white: from which primitive colour the *Europeans* degenerated as much on one hand, as the *Africans* did on the other; the *Asiatics* (unless, perhaps, where mixed with the whiter *Europeans*) with most of the *Americans*, retaining the primitive and original complexion. The grand obstacle to the belief of this relation between white and black people is, that, on comparing them together, their colours seem to be so opposite and contrary, that it seems impossible that one should ever have been descended from the other. But, besides the falsity of this supposed direct contrariety of their colours, they being only different, altho' extreme, degrees of the same sort of colour, as we have above proved; besides this, I say, that is not a right state of the question; we do not affirm, that either Blacks or Whites were originally descended from one another, but that both were descended from people of an intermediate tawny colour; whose posterity became more and more tawny, *i. e.* black, in the southern regions, and less so, or white, in the northern climes: whilst those who remained in the middle regions, where the first men resided, continued of their primitive tawny complexions; which we see confirmed by matter of fact, in all the different people in the world. Agreeable to this, we see that the heat of the sun will tan, as the saying is, the fairest skin, of a dark swarthy, even at this day; in which there is some degree of blackness; or, at least, this may well be said to be a tendency to their primitive swarthy complexions; *Rubescere cum nigredine quadam incepit*, says *Sennertus* *. So that if the heat of the sun will turn a white skin swarthy, as nobody in hot countries can doubt, the same cause might turn the swarthy and tawny black; for the effect seems to be the same in one as in the other, and may therefore be produced by one and the same cause. As for the black people recovering, in the same manner, their primitive swarthy colours of their forefathers, by removing from their intemperate scorching regions, it must be observed, that there is a great difference in the different ways of changing colours to one another: thus Dyers can very easily dye any white cloth black, but cannot so easily discharge that black, and bring it to it's first colour: and thus, although the skins of white or even swarthy people, are easily affected by the greater power of the sun's beams than what they have been used to, and thereby become black; yet they are thereby rendered so thick and hard, or tough and callous, as not to be so easily affected, or readily wrought upon, to render them again of their original swarthy or pale colour, by any of those causes, as the absence of the sun, coldness of the climate, or ways of life in it, which we have supposed to be the causes of the fair complexions of the *Europeans*; although, I believe, it has never been tried, what effect these luxurious customs,

* *Prax. Med. lib. v. part 3. cap. 1.*

or soft and effeminate lives, which we have supposed to be the causes of mankind's turning to so tender and delicate complexions as the *Europeans* have, and to be the cause of all whiteness in the complexions of men, or changes from a dark to a fairer complexion, might have upon the colour of negroes; but this we are assured of, that they are not of so deep a black, in cold northern, as in the hotter southerly regions. Besides, we want not some convincing instances, from the gleanings of the few historians I was furnished with here, to shew that such changes have happened in the memory of men, and within the compass of those records we have of time; for we could not suppose it to have happened all at once: thus *Herodotus* tells us*, that the *Colchi* were formerly black, with frizzled hair; which (he says) he relates rather as a thing well known before, than a bare report; but there is no sign of any blackness in the complexions of their descendants, they being rather, especially about *Circassia*, reckoned some of the fairest people in the world at this day. Captain *Smith* tells us, that, even in *Virginia*, an *Englishman*, by living only 3 years among the *Indians*, became “so like an *Indian*, in habit and “complexion, that he knew him not but by his tongue †:” and what might his children have turned to in a succession of many generations, by these same ways of life, which had so altered him in 3 years? The *Moors* and *Lybians*, being driven out of *Africa*, upon the *Turkish* conquest, retired to the land of the *Negroes* ‖; but are no more to be found there of their original tawny colour. The King of *Gualata* is supposed to be lineally descended from these tawny *Moors*, but is even blacker than the original negroes **. The *Habessinians*, who came from *Arabia* originally ††, are no longer of their swarthy complexion, but have got the black complexion of the *Ethiopians*, whose country they possess ‖‖. The *Mosemleeks* of *Canada*, who wear cloaths, and are more civilized than the other savages their neighbours, who go stark-naked, are so much more refined in their complexions by this usage, as to be taken for *Spaniards*; and not *Indians* §§. Nay, the *Spaniards* themselves, who have inhabited *America* under the torrid zone, for any time, are become as dark-coloured as our native *Indians* of *Virginia*, as I have been an eye-witness: and were they not to intermarry with the *Europeans*, but lead the same rude and barbarous lives with the *Indians*, it is very probable, that, in a succession of many generations, they would become as dark in complexion.

* Euterpe, 104.

† *Hist. Virginia*, p. 116.

‖ *Leo Hist. Afric.* par. 1.

sect. 3.

** *Moore's Travels*, 214.

†† *Ludolph. Hist. Æthiop.* l. 1. c. 1.

‖‖ *Idem*, l. 1. c. 14.

§§ *La Hontan, Nouv. Voyage*, lett. 16. vol. 1.

Concerning a
Plica Poloni-
ca; in a letter
from Mr Jo-
seph Aines.

F. R. S. Sec.

Soc. Antiquar.

to C. Morti-

mer, M. D.

Secr. R. S. N^o.

483. p. 556.

Mar. &c

1747. Read

May 28.

1747.

II. June 22, 1746, in the morning, Mrs *Hannah Coomes*, a neat old woman, whose hair (or *Plica Polonica*, as it is called) I shewed the *Society* last *Thursday*, came and gave me the following informations.

That she was of a genteel family in *Staffordshire*, who had suffered much in the civil wars; and that her mother had her hair grow in the same manner, whose maiden name was *Alice Goldsmith*; but her own maiden name was *Hannah Bunby*, born in the Hay-market, in the parish of *White-Chapel* and baptized at *Aldgate* on a *Saturday* the of *June* 1645. Her mother, having such sort of hair, used to comb her's much to prevent it, till sometimes the blood came: when she was about 14 years old she perceived it to grow thick just about the back part of her head, and at length grew to this matted long substance I now saw it, of 109 inches long. She says she has had 4 husbands; the first *Nicholas Woodcock*, to whom she was married when about 28 years old, and had 4 children by him; all died young; but observed nothing of their hair growing so.

C H A P. III.

The H E A D.

A Schirrus
of the Cere-
bellum; by
Albert Haller,
F. R. S. Ar-
chiat. Reg.
& Med. Prof.
Gotting. N^o.
474. p. 100.
June, &c.
1744. Read
June 14.
1744.

I. A Poor girl was brought into the theatre, Jan. 21. 1744, who seemed to be about 6 years old. Her body was very much emaciated, and many *schirri* were observed in the mesenteric, inguinal, and bronchial glands. The left lobe of the *cerebellum* almost wholly adhered to the *dura mater*. On making an incision, I found an enormous *schirrus*, 2 inches in diameter on each side, into which the whole either medullary or cortical substance of the *cerebellum* had degenerated. The tumour was of an uniform thickness, fibrous, almost of the form of a kidney, and easily divided; no vessel appeared on dissection, nor did any mark remain of the cinereous *cortex*, or of the medullary *arbusculæ*.

Whatsoever was the origin of so great a disorder, it is plain, that the middle part of the *cerebellum* in this poor girl did not become useless in a short time; and yet the poor creature continued to live by begging from door to door.

There is one history of a schirrous *cerebellum* found in a stupid boy, in the *Mem. de l' Acad. R. des Sciences*, 1705. n. 13.

An account
of a remark-
able cure per-
formed on the
Eye of a young

II. *Jane Wilson*, a girl now 18 years of age, about 7 years ago began to have her left eye turned towards the temple, occasioned by some tumour betwixt the globe and the orbit. This tumour, for some years, did not appear outwardly; but, increasing by degrees, at last a hard swelling

swelling appeared externally, reaching from the great angle almost to the little angle under the lower eyelid, and $\frac{1}{2}$ an inch down on the cheek: it had forced the globe of the eye almost out of the socket, so that the pupil of that eye was, by measure, above $\frac{3}{4}$ of an inch further from the nose, than the pupil of the other eye; and the eye was more jetting out in proportion; so that it seemed to be out upon the temple, and quite immoveable; which, with the tumour, made a frightful sight. The patient had frequent pains in her head; but what was most surprising, the sight of that eye was not lost, though a good deal impaired. I shewed this patient to Mr *Alex. Monro*, Prof. of Anat. *Edinb.* whose abilities are universally known; who, after examining it very narrowly, gave it as his opinion, that this tumour had begun at the bottom of the orbit; and that the extirpation would be exceedingly difficult; and, as it seemed to be an incysted tumour, if any of that *cystis* remained at the roots, it would be apt to sprout up again: but, withal, concluded, that there was room for a trial; and it would be a pity not to do something in order to save the patient's eye, and, probably, her life, which would be in danger, if the tumour continued to increase. I likewise shewed her to several other eminent Gentlemen of the Faculty, who were all pretty much of the same opinion. Notwithstanding of this, considering the great risque that the patient run, if something was not speedily done, I resolved to undertake it: having had a case of the like nature, but in a lesser degree, under my care about 12 years ago in *London*, the extirpation of which I performed without any bad consequence; and, upon consulting my old master *St Yves's* book, I found almost a parallel case to this girl's which, he says, he extirpated with success; and, as he was an honest man, I knew I could trust to him.

On the 19th of *June* last, in presence of Dr *Lewis*, Dr *Dundas*, Dr *Mac-Farlane*, Dr *Young*, and Mr *Cunningham*, Surgeon, I performed the operation in the follow manner:

I turned the patient backwards on a chair upon an assistant's lap, her head supported by pillows; then, keeping the skin tense with my fingers, I made an incision about an inch long with a small razor, beginning at the greater angle, and following the direction of the fibres of the orbicular muscle towards the lesser angle. I then passed a crooked needle armed with silk thro' the middle of the tumour as deep as I could go; and, raising the tumour with the silk, with a fine bistoury I separated all the lateral adhesions from round the tumour; and, with the point of my scissars, I cut the deeper adhesions, which I could not so well reach with the bistoury, and brought away all that the thread had hold of. This seemed to be a tough membranous substance, independent of the real tumour; for, after this was quite taken out, there appeared a regular tumour, of a spherical figure, smooth and even, about the bigness of a small pigeon's egg: I passed the needle through the middle of it, as I had done before, and plunged a lancet into it as deep as I could, in order to let out any fluid matter that might be contained therein.

woman in
Scotland, by
Tho. Hope,
M. D. commu-
nicated from
Dr Mead.
N^o. 474. p.
194. June
1744.
Read Dec.
13. 1744.
Fig. 61.

therein, but found nothing but a carnous substance; then, lifting up the tumour by the thread, I dissected it, with great care and caution, from the adjacent parts, as far as I could; in doing this, I found several strong callous attachments on the side next to the globe, which felt almost as hard as a cartilage, and obliged me to change 2 or 3 instruments. I then, with the point of my scissars, cut the inward adhesions at the roots, and brought the tumour away entire: upon putting in my finger to the bottom of the orbit, I could feel several hard callous substances still remaining; and keeping my finger upon them, I slid a crooked needle armed with silk round the point of my finger, with which I hooked those callous roots; then, making an assistant raise the thread. and directing the scissars upon the point of my finger, where I felt the said roots, with two or three snips I cut them quite away; so that I left the bottom even, and intirely free, as far as I could judge. All this while I had no great effusion from any artery, but a good deal of black grumous blood from the varicose vessels, I dressed it up the first time with dry lint, which I did not take off till the third day; when I found a soft swelling in the eye-lids and *conjunctiva*, with a slight inflammation, and a pain in the forehead. I dressed the wound with a soft doffel dipt in common digestive and warm brandy, and ordered an emollient fomentation to be applied every two hours: the pain in the forehead, and the swelling continued for 3 or 4 days, without any appearance of matter. I then touched the bottom of the wound with the lunar caustic, and some hours after, there followed a pretty large discharge of blackish blood, and immediately her head was relieved, and the swelling subsided: a bloody *sanies* continued to issue out the 2 following days, for which I injected warm water, with a little brandy and honey of roses, after which it came to a pretty good digestion: as some spongy soft substances began to appear, I touched them with the lunar caustic, and the wound filled up apace. The eye still continued immoveable, the *musculi abductores* had been so long contracted, and the *adductores* so overstretched and lengthened, that they had lost their use; I could however observe, that, by pressing with my hand upon the globe of the eye with a little force, I could bring it a good deal more into the socket, but upon taking away my hand, it would immediately return to it's former place. This made me think, that a constant and gradual pressure, by some proper bandage, might be of service to force the globe into it's place, and keep it there till the muscles had recovered their tone: accordingly, I got a steel bandage, with a concave brass plate corresponding to the convexity of the eye; which, by the means of a screw, bore upon the side of the globe next the temple: I applied this bandage, by first gently forcing the globe more into it's place with my hand; then, putting a thick soft compress betwixt the globe and the brass plate, I screwed it down upon the globe in such a manner, that it was impossible for it to start back again as it used to do. I left an assistant with the patient all night, with instructions, if the

bandage

bandage caused great pain to ease the screw; and so, by gradually forcing it more and more, and keeping this bandage constantly applied day and night, in about 20 days the eye was brought entirely into its place, so as to remain there of itself, had all its regular motions every way, and the patient saw with that eye as well as with the other.

This patient, after the cure, was shewn to the Physicians that had been present at the operation, and to others the most eminent of the Faculty. In the morning, when I used to take off the bandage, I could observe that side of the globe which the plate bore upon considerably flattened, and yet not attended with any pain, or bad consequence. In about a month the wound was quite healed up. A spongy carnosity had grown all along the inside of the lower eyelid, which, being long over stretched by the tumour, was so relaxed, that, after the operation, it turned inside out, and occasioned that disorder which is called *ectropion*: the upper eyelid having been very much extended for so many years by the globe, upon the eye returning to its place, was so relaxed, that its cartilage, on the contrary, turned inwards; whereby the *cilia* or hairs upon its borders rubbed against the globe of the eye and occasioned the disease commonly called *trichiasis*. For the cure of the *ectropion*, I passed a crooked needle through the middle of the carnosity, and raising it by the thread, I cut it off with the scissors; I afterwards touched the inside of the eyelid with the lunar caustic, in order to destroy what remained of the carnosity; and, giving the eschar time to throw off, I repeated the same twice or thrice, by which the eyelid, in about a fortnight, recovered its healthful and proper situation. I did not think proper to torment the poor patient with the operation of the *trichiasis*; which, though very easy to the operator, is not so to the patient; and I found, by the application of proper topicks, the eyelid recovered strength daily; and I judged by the continuance of the same method it would soon be well.

It will not be easy to account how sight should remain after the optic nerve was so stretched; which it must be in a very considerable manner in this case; and how it came to contract or recover itself so soon, after being so great a while extended. It is true, that while the optic nerve was in its state of extension, the sight was impaired; but, after 7 years extension, how it came to recover itself in a month's time, without any alteration in the sight, but for the better, I leave to the speculation of the curious. It is likewise pretty extraordinary how the muscles, after having been so many years in disuse, should recover their natural action in so short a time.

III. A poor widow, aged 26 years, of a pale complexion, was for several years now and then subject to the colic. Dec. 26. 1744, she received a wound in the *cornea* of her right eye, by the spear of a common fork, which also divided the *uvea*. Part of the aqueous humour was discharged, the eye lost its transparency, had a violent pain

Cure of a wound in the Cornea, and a laceration of the Uvea in the Eye of a woman; in a

letter from
Tho. Aery,
M. D. 10 C.
Mortimer,
M. D. Sec.
R. S. N^o.
488. p. 411.
June 1748.
dated White-
haven, June
14. 1748.
Read June
30. 1748.

in it, and she could only distinguish objects when she looked down. I ordered her a *collyrium* prepared of the *Bals. Tolut. Camphor. solut. in Sp. Vin. Aq. Plantag. cum pauxillo Tinct. Mart. Mynsicht*. A few drops of this blood-warm was to be used frequently; to bleed her largely in the arm, and her diet was to consist of water-gruel, *Aq. Hord.* and fresh broth. Next day she had no pain in the eye, but complained she saw motes floating before it: I ordered her a purge of *Infus. Sennæ*, and an astringent fomentation to her temples and eyelids. The day following the eye was inflamed, and the lids tumefied, and she had a pain in her head. The *collyrium* was changed to rose-water and vinegar, $\overline{au.} \frac{3}{4}$ ss. roche-alum gr. v. 3. drops twice a day. The 29th the inflammation increasing, the *Infus. Sennæ* and bleeding were repeated, and the parts were fomented only with spirit of wine. The 31st the inflammation continued to decrease, till after a fright. Jan. 5. the inflammation increasing, the sides of the wound became a little protuberant, The *Senna* was repeated, and a blister laid behind the right ear, and an emollient *collyrium* was used: next day the swelling of the eyelids was gone: the 11th she had a shew of the *menses*, and the wound appeared healed: from the 15th to the 24th the inflammation continued to abate; only one day it increased by fretting and weeping much; but by bleeding she grew better, and so she continued to the 30th; unless one day, upon catching cold, her eye became exceedingly inflamed, which was relieved by bleeding. Feb. 4. she had a little pain in her eye, and the *tunica adnata* looked a little red. Soon after dropping in of 2 drops of cold water, the eyelids swelled, and a violent inflammation of the eye ensued, with a speck appearing; but these symptoms went off by repeated applications of leeches and a mercurial purge. The 19th a sternutatory of hellebore and *euphorbium* was ordered. In a few days after the inflammation left her eye; when she complained she saw double; which complaints also soon left her.

The eye is myopical, and she sees the right side of objects a little darkened; yet she can read pretty small characters. The *uvæa* is not united where it was divided; but still retains it's natural power of contraction; the transparency of the humours and convexity of the *cornea* are the same as before; there is no scar upon the *cornea*; the shape of the *pupil* is much altered, as may be seen by the figure.

Fig. 62.

Upon catching cold she is subject to a slight pain in her eye. At present there remains no other alteration than what I have just mentioned, and what necessarily follows from the contraction of the *pupil*, the not admitting a sufficient quantity of rays to pass to the *retina*, upon which account she is short-sighted. Her seeing objects darkened on one side, may proceed from the artificial part of the *pupil* being situated nigher to the great *canthus* of the eye than usual in nature; by which the rays which fall on the side of the *cornea* next to the little *canthus* of the eye, being partly intercepted, must occasion a defect in the picture; from which defect a darkness will be seen on one side of the object. To the weakness

weakness of the vessels of the eye we may attribute the pain of the eye upon catching cold : it oft-times happens to those who have had a severe *ophthalmia*, that, during life, the small vessels are too weak ; and hence, from slight causes being distended, they will be painful, and frequently red.

This case is known to several in this place, particularly to Mr *Blencowe*, an ingenious Surgeon and Apothecary.

Some remarks occur from this case ; viz.

1. Her cure would have been performed sooner, if her circumstances had allowed of her observing an exact regimen.
2. When her eye had little or no appearance of inflammation, I tried cold water, but with rather bad success. All cold applications to inflamed eyes, astringents or repellents, require the utmost caution in applying them ; for if they produce not a good, they will produce a bad effect. In slight cases they oft have very happy effects, but where the obstructed matter is so fixt that it will not suffer itself to be easily repelled back, the vessels being straitened, the fluids coagulated, the disease will be increased ; which happened in this case from the application of cold water.
3. The good effects of evacuations are very evident in abating the inflammation. Wounds in the *cornea*, attended with a wound of the *uvea*, and a troublesome ophthalmia, heal without any scar.
5. An artificial *pupil*, made by flitting the *uvea* in a different method from that invented by Mr *Chefelden*.
6. The necessity of changing the topics, according to the state of the distemper ; which has been remarked before by several celebrated authors.

IV. Jan. 17. 1747. Henry Halsey, of South Mims, Labourer, thrust a long lath with great violence into the great *canthus* of the left eye of Edward Roberts of the same place Labourer, which broke off quite short ; so that a piece 2 inches and near $\frac{1}{2}$ long, $\frac{1}{2}$ an inch wide, and above $\frac{1}{4}$ of an inch thick, remained in his head, and was so deeply buried there, that it could scarce be seen, or laid hold of. He rode with the piece of lath in him from Kick's End, where the thing was done, to Barnet, which is above a mile, to the house of Mr Justinian Morse, a Surgeon there, who extracted it with difficulty ; it sticking so hard, that others had been baffled in attempting to extract it. Roberts continued dangerously ill a long time ; but at last, by the blessing of God, and the care of Mr Morse, recovered entirely, and has the sight of the eye, and the use of the muscles. But some time after he seemed well, he told me, that, upon leaning down forward, he felt great pains in his head. The piece is supposed to have passed behind the right eye.

Concerning a large piece of a lath being thrust into a man's eye, who recovered of it ; in a letter from Rich. Hassel, Esq; F. R. S. to Peter Daval, Esq; Sec. R. S. N^o. 489. p. 520. Oct. & Nov. 1748. Read Nov. 10. 1748. Fig. 63.

V. As several of the Members of this Society were somewhat divided in their opinions concerning what was reported of Margaret Cutting, when

A physiological account of the case of

Margaret
Cutting, who
speaks distinct-
ly, tho' she has
lost the apex
and body of
her tongue; by
James Par-
sons, M. D.
F. R. S. No.
484. p. 621.
Oct &c.
1747. Read
Dec. 17.
1747.

Of her present
condition.

when they were first informed of her by Mr *Baker**; it will be necessary (in order to render her case the better understood) to lay before you the following short particulars, which are the result of an examination made a few days since by Dr *Milward* and myself; and which, in general, differs not from the opinion which that learned Gentleman and I mentioned to this *Society*, upon the occasion, which the science of Anatomy necessarily suggested to us at that time. But *James Theobald*, Esq; having encouraged her to come to *London*, and having brought her to this meeting of the *Society*, has now given us all an opportunity of coming at the truth of her case; wherefore I shall now present you with, 1. An account of her present condition; and then, some considerations on the natural state and uses of the tongue; which will shew you how far she makes the *lips* and *teeth* supply the want of her tongue in speaking; and also be a direction to every Gentleman present to judge of the case before him.

The *apex* and *body* of the tongue (being the only parts that naturally fill the cavity of the mouth) are entirely wanting in this woman, as closely to the region of the *os hyoides*, which is the root of the tongue, as can well be conceived; and which is now situated too low in the throat to be perceived, even when she opens her mouth at the widest. But let any one lay the tops of the finger and thumb to the sides of her throat, and let her at the same time pronounce the letter *k* he will feel the remaining root of the tongue rise towards the roof of her mouth, in order to perform it; however, she cannot keep it there any longer than the moment of thrusting it up, for want of the ligament (which was destroyed with the tongue) that is destined, together with the following muscles, to keep the whole tongue forwards in it's due situation. The *genioglossi* are a pair of muscles which arise from the fore part of the inside of the lower jaw, and are inserted into the body of the tongue by three different directions; the anterior part is carried forward towards the *apex*: the posterior runs obliquely backwards towards the root, sending a narrow slip on each side to the *cornua* of the *os hyoides*; and the middle part ends about the middle of the tongue. Now there are certain *inequalities* appearing on, and closely adhering to the floor of the cavity of the mouth, one of which being the most considerable, and having a resemblance in it's substance to that of the surface of the tongue, has been, if I am rightly informed, inadvertently mistaken for a tongue, by a Gentleman professing Surgery in the country; and which he thought, for want of a careful examination, performed the offices proper to the *apex*; but a little care and circumspection would have informed him, that those appearances are only fragments of the *genioglossi* mentioned before, and that upon the separation of the sound parts from those mortified, such fragments, as had escaped, were retracted, and cicatrized down into their present state; nor is it difficult to conceive how the root of

* See Vol. IX. Part III. Chap. III. §. vii.

the tongue must of necessity sink lower down into the throat; by the loss of these muscles and the proper ligament; which, as I have observed before, naturally kept it higher than it could remain ever since their destruction. If the mortification had reached the *os hyoides*, it must have reached, and destroyed the muscles of the *larynx*, and then the voice would have been destroyed; and also those of the *pharynx*, and then deglutition could never have been performed; the dreadful consequences of which need not be enumerated here; but she swallows well, and her voice is perfect, and therefore it is not very extraordinary she should command her voice by the proper muscles which remain untouched. The nasal opening is quite exposed, because the *uvula* which covered it was also destroyed; for one pair of it's muscles (the *glossopharyngealini*) arise from the tongue; by which no doubt the distemper was communicated to this part also. She has her taste perfectly, which is hereafter accounted for.

The tongue is a fleshy substance, chiefly made up of muscles; and consists of a *basis* or root, a *body*, and an *apex*; the basis is the thickest and most substantial part, contains the *os hyoides*, and is naturally situated very low in the throat: from which the body rises upwards and forwards, and is terminated by the anterior part or *apex*; proceeding under the *uvula* and roof, and lying upon the floor (if I may so call it) of the mouth. As to it's uses, it is said to be the instrument of *speaking* and *tasting*; as to the latter, experience shews us that the very *apex* of the tongue is less capable of discerning tastes than the next part to it, and this than the parts yet farther back, all along the *body* to the root; so that altho' the taste of any thing is first perceived by the *apex*, yet the gust increases, the more the morsel approaches to deglutition, until it is quite protruded into the *gula*; because as the tongue grows more thick backwards it contains more of the nervous *papillæ* than the smaller part, and also because there is a capacity of tasting in the membranes of the back part of the roof to the root; as if nature intended to increase the gust, that deglutition may be the better and more eagerly performed for the service of the animal: hence altho' the *apex* and *body* of the tongue be gone, yet there is not a depravation of taste, which is the case of the person now under your consideration. As to *speech*, which is only *sound* or *voice* articulated into expression, the tongue is not the *sole* organ for such articulation; the *lips*, *teeth*, and *roof* of the mouth are instruments also for the same purpose; the two latter for the necessary resistance to the *apex* of the tongue, and the lips for the absolute articulation and pronunciation of many letters; however the following short examination of the letters of the alphabet, as expressed by these organs, will demonstrate it. The tongue expresses some letters with it's *apex*, and some with it's root. Those absolutely proper to the *apex* are only five, *d, l, n, r, t*. And those to which it only assists are the following letters, as *c, g, s, x, z*; all which can be performed by the teeth alone, and which this person does very well. Now the lip-letters, and those expressed

Some considerations on the natural state and uses of the tongue.

expressed by the root of the tongue, she also performs as well as any person ; the former are *b, f, m, p* ; and the latter are *k, q, x* ; and as to the vowels, and the aspiration *h*, since they are chiefly founded by the exhalation of the voice, commanded partly by the lips in widening or straitening the capacity of the mouth, these she can also express ; so that there is no letter she cannot pronounce but the five *apex* letters ; and those she manages so well by bringing the under lip to her upper teeth, in the course of her conversation, that any one can instantly apprehend every word she says ; and she further plainly proves the lips are a better *succedaneum* to the *apex*, than that could be to the lips if they were wanting. Indeed it is natural enough for those who make the tongue the absolute and sole instrument of speech, to imagine it as absurd to say a woman spoke without a tongue, as that she saw without an eye ; but when we consider the provisional assisting organs ordained by the wise AUTHOR of PROVIDENCE, serving to this necessary and expressive accomplishment, I hope it will not seem so extremely marvellous, that she speaks without the *body* and *apex* of her tongue, as to create any further doubt of the matter.

The case of Henry Ax-ford, who recovered the use of his tongue, after having been four years dumb, by means of a frightful Dream ; communicated by the Rev. Mr Archdeacon Squire, F. R. S. N^o. 486. p. 148. Feb. and Mar. 1748. Read Feb. 4. 1747-8.

VI. *Henry Axford*, Son of *Henry Axford*, of the *Devizes* in *Wiltshire*, an Attorney, when a child was subject to convulsion fits, which followed him pretty frequently till he was about 25 years of age. After this, his health became extremely good. At about 28 years old, going with some ladies to see *Longleat* in *Wiltshire*, the seat of Lord Viscount *Weymouth*, he perceived a hoarseness coming upon him, which was soon after attended with all the symptoms of a common cold, till, in about 6 days after his first seizure he became quite speechless, not only losing the articulate use of his tongue, but being scarcely able to make the least noise with it. His cold quickly went off in the usual manner, and he grew perfectly well, as well in health as ever he had been in his life ; but he still continued absolutely speechless. He had advice from all the neighbouring Physicians, but to no purpose ; for nothing they did for him could restore him to the former use of his tongue. He continued in this dumb way about 4 years ; till one day in *July 1741*, being at *Stoke* in the above-mentioned county, he got very much in liquor, so much, that, upon his return home at night to the *Devizes*, he fell from his horse 3 or 4 times, and was at last taken up by a neighbour, and put to bed in a house upon the road. He soon fell asleep ; when, as he tells the story himself, dreaming that he was fallen into a furnace of boiling wort, it put him into so great an agony of fright, that, struggling with all his might to call out for help, he actually did call out aloud, and recovered the use of his tongue from that moment as effectually as ever he had it in his life, without the least hoarseness remaining or alteration in the old sound of his voice, as near as can be discerned. He was not used to drink hard ; he is still alive, continues in good health, and has the use

Fig. 61.



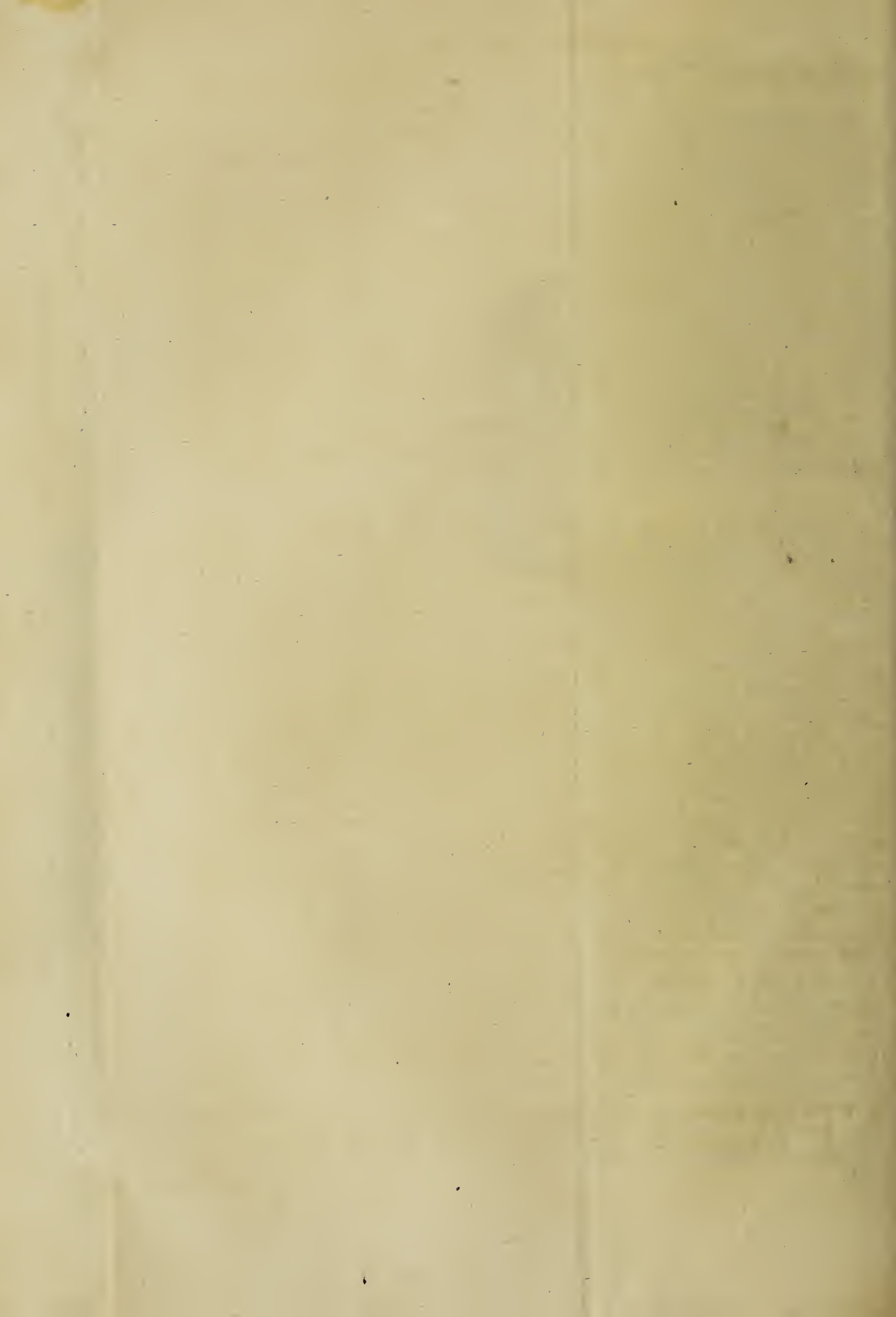
Fig. 62.



Fig. 63.



The piece of Lath.



use of his tongue as perfectly as ever he had in the former part of his life.

VII. In a M.S. account of the life of the Rev. Mr *Henry Wharton*, Chaplain to Archbishop *Sancroft*, written by himself, I have met with the following passage:

“ From my very birth I had two tongues, each of the same form and bigness. The women, who assisted at the labour insisted upon having the lower one cut off; but my mother would not consent: and fortune seconded her piety: for the lower tongue withered by degrees, and was contracted to a substance not bigger than a pea, which yet continues. The upper tongue however grew to it's just bigness, though marked by several long and deep furrows, which grew together with the tongue in a parallel position, and do not seem to be ever likely to meet.”

An account of a very learned Divine, who was born with two tongues; communicated to the R. S. by C. Mortimer, M. D. & Sec. R. S. Ibid. p. 232. Read March 10. 1747.

Nat. Nov. ix. 1664. Ob. 1694-5. Mart. 5. Æt. 31.

It appears by this journal of himself that he was always infirm and sickly.

VIII. This substance, seemingly a concretion of stone or chalk*, was voided in *July 1748*, from under the root of her tongue, just on the left side of the middle string among the blood-vessels. It was lodged in a cell formed by itself, the traces being left behind exactly tallying. It was voided without pain, or effusion of blood. The patient began to feel in the part affected some uneasiness about 18 months before the discharge. The pain extended itself sometimes along the jaw almost to the ear; the glands being at times swelled, and a salt rheum flowing into the mouth. The swelling of the part gradually increased to about the size of a large nutmeg; and, being felt by the finger, was hard. About a fortnight before the discharge, some white specks appeared; upon which it was supposed, that matter was gathering; and being still hard, a common poultice of white bread and milk was applied, and then it presently dislodged itself, without any application, and left the patient ever since free from complaint. I was obliged to the Rev. Mr *Chauncy*, the husband of the patient, for this account.

An account of the case of a Clergyman's lady, at Cottered near Baldock in Hertfordshire, who had a stone under her tongue; by W. Freeman Esq; F. R. S. N^o. 491. p. 5. Jan. &c. 1749. Read Jan. 19. 1748.

CHAP. IV.

The NECK and THORAX.

I. **T**HE *morbus strangulatorius*, with great propriety and justice thus denominated, has within a few years reigned in several parts

An account of the Morbus Strangulatorius, in a letter

* The stone is in the Museum of the Royal Society.

from J. Starr,
M. D. to C.
Mortimer,
M. D. Sec.
R. S. &c. No.
495. p. 435.
May, &c.
1750. dated
Liskard,
Jan. 10. 1749.
Read May 24.
1750.

parts of *Cornwall* with great severity. Many parishes have felt it's cruelty, and whole families of children, whence it's contagious nature is but too evident, have, by it's successive attacks, been swept off. Few, very few, have escaped. It is enough to say, that the disorder does not appear with the same train of symptoms in every subject. On the contrary, a vast difference is observable; but then, whatever, or how various soever, the symptoms may be, there is a certain degree of malignity, or (which is what I mean) there are signs of a putrid disposition of the juices, in all. Some, I am informed, have had corrosive pustules in the groin, and about the *anus*, eating quick and deep, and threatening mortification, even in the beginning. Others after a few days illness have had numbers of the worst and deepest *petechiæ* break out in various parts of their body. Such I have not seen. Many on the first attack have complained of swellings of the glands, as tonsils, parotids, submaxillary and sublingual glands, but frequently of no great importance. A few, from an internal tumor, have had a large external oedematous swelling of the subcutaneous and cellular tunic, from the chin down to the thyroid gland, and up the side of the face. One such I was concerned with, the tumor broke in the *fauces*; but, instead of a laudable pus, some ounces of a coffee-coloured exceedingly fetid matter were spit off. The man recovered. As respiration only suffered here by pressure, I should rather choose to call this a malignant *angina*, than the true *morbus strangulatorius*.

Not a few early in the disorder have had gangrenous sloughs formed in their mouths, and perhaps so early in some, that the disorder was scarce complained of, till the slough was formed, so quick has it been in it's progress. Others again, without any of the preceding symptoms, have only complained of a slight pain in swallowing, succeeded with a hot flesh, feverish pulse (never quick and weak, but as to the stroke quick, and sufficiently full and strong), a short, low, heaving, hoarse cough (the patient generally so hoarse as to be difficultly understood after a day or two's illness), which, sooner or later, for I never could observe any certain period, was productive of a difficult, noisy, and strangulating respiration.

These last, especially the former of them, I esteem as the pathognomonic symptoms of the real *morbus strangulatorius*: the above-mentioned are rather *symptomata causæ, quam morbi*. I have not mentioned a *fætor oris*, which, when it happens, is usually an early symptom, because, though some have had it, others have had it not. This respiration, however agonizing it appears, has, especially in the beginning, it's remissions, and exacerbations. It's cause cannot of course be permanent. I take it to be owing to a lodgment of some matter in or about the *glottis*, and *larynx* through which the inspired air is obliged to pass: while this matter is capable of being expectorated, and happens to be coughed off, the breathing for a time becomes free, and the patient is delivered from the utmost seeming distress; but, on it's recollection,

lection, which, if the progress of the disorder cannot be stopt, never fails to happen, this symptom again occurs, and the patient either dies suddenly, or, being worn out, or quite dispirited, sinks away gradually, or, falling into convulsions, in these expires.

I was called to a girl of 5 years old. Her tongue was quite clean; she could move it every way as in health. Nothing morbid was seen in her mouth, or indeed *fauces*: she had a trifling pain in swallowing (it was felt on depressing the *epiglottis* for the passing the bolus), not sufficient to prevent her from eating bread and butter, biscuit, figs. It was on the 4th day of her disorder, she had the strangulating respiration, with a cough exceeding hoarse. After the use of a stimulating gargle, &c. her cough became stronger, and she threw off a large quantity of white rotten flesh, or membranes, mixt with a slimy adhesive matter; her respiration became so easy, that she seemed to ail nothing. In 3 hours it grew again difficult, and gradually increased till it arrived at its former violence. Those about her fancied there was somewhat in the passage which ought to come off: the child gargled, and provoked her cough as far as she was able, but in vain. Her agonies increasing, she said, as well as she was able, I shall be choaked, and in a few minutes died. This case shocked me, being satisfied, that somewhat very extraordinary and uncommon could only occasion so sudden, and to appearance, violent a death.

I have frequently examined the matter those patients have at times spit. Though there was some difference in various subjects, yet I never once saw a well-digested or concocted phlegm, or *mucus*, on the contrary, the greatest part was of a gelly-like nature, glary, and somewhat transparent, mixt with a white opaque thready matter, sometimes more, sometimes less, resembling a rotten membranous body or slough. Such a slough I have seen generated on the skin of one of these patients in the neck and arm, where blisters had been before applied. The blisters had been dressed with colewort-leaves, and ran but little; but, contiguous to them, small red pustules, not exceeding fiery, arose, which, sweating plentifully in a few hours, became quite white: these, hourly enlarging their bases, united, and covered a large surface, fresh pustules arising in the adjacent parts. This white surface had the aspect of an oversoaked membrane, which was become absolutely rotten. The part blistered, if not quite, was in effect dry, and the flux from the slough was incredibly great. If I mistake not, cloths 10 times double, the child's shift, a double bed-gown, were wet quite through, and a large spot was seen in the bed of some hands breadth; and this in a very few hours. I scratched the slough with my nail; it separated with ease, and without being felt by the child. What my nail took off afforded the same appearance with the matter of the spittle before-mentioned. Hence, I thought, I saw sufficient reason to convince me that the disorder in the *larynx* and *aspera arteria* was similar to this, generated in the same manner, and arising from the same internal cause: and sup-

posing this conjecture true, the production of every symptom seems easy to be accounted for.

In Dec. 1748. while the *morbus strangulatorius* was among us at *Liskard*, a child here and there had red pustules, not unlike the above, which broke out in the nape of the neck, and threw off a surprising quantity of thin transparent ichor, vastly glutinous when dry. These were easily cured in the beginning, if managed aright; but, being drawn with colewort-leaves, or pultised according to the direction of our old female practitioners (too often the case) the above-mentioned slough was soon generated. I was desired to look on a poor person's child in this unhappy situation, who, with little intermission for I think near two days, had bled profusely at the nose; her pulse was almost gone; the bleeding was with difficulty stopt; but, being quite exhausted, in about 6 hours she sunk in a faint fit. The slough had spread from shoulder to shoulder, extended full $\frac{1}{3}$ down her back, and seemed very thick. All treated in the above manner died. Scarifying afforded no relief. Now, tho' this was not properly the *morbus strangulatorius*, yet I apprehend it was analogous to it, and produced from the same cause; and it is likely, had the anatomical knife been employed, what was seen on the back of one, might have been discovered in the *aspera arteria* of the other. There is a circumstance which adds to the probability of this opinion, viz. in one or more instances, these different disorders appeared in different subjects, in the same family, at the same time.

What I have hitherto said, does not, I own, demonstrate the case to be as represented, but the following history throws the strongest light on this dark, mysterious affair, renders the disorder, by it's consequences affrightful, even shocking to the imagination, accounts for it's too common fatality, and must convince of the great difficulty of the cure, if in itself possible, unless attempted with judgment in the very beginning.

Dec. 11, 1749, I was called to the son of Mr *Kitto*, a Farmer in the parish of *St Eve*, a lad aged 10 $\frac{1}{2}$ years. This was the 7th day of his illness. His first complaints were, a pain in swallowing, not great; a cough, hoarse, vexatious, like an incipient catarrh, a pain on coughing shot into his ears. This was still felt at times; a thin ichor ran from his mouth in great plenty, supposed to be a quart, or 3 pints daily. His pain in swallowing was now so trifling, that I saw him drink a considerable draught without removing the vessel. He was now so hoarse that he could scarce be heard. His cough was rough, low, short, and ineffectual; breathed with much straitness and noise, especially in inspiration; the wheezing or rattling might be heard at a great distance, was always worse during a coughing fit, or for a short time after. When he spit by the cough, it was glary, but glutinous; a whitish rotten sort of stuff would sometimes accompany it; it's quantity never great. Examining his mouth, he could move his tongue every way without the least pain; forward it was clean, but behind a little furred. De-

pressing

pressing it with a *spatula*, a white body was seen on the *velum pendulum palatinum* and tonsils. I desired Mr *Scotchburn*, a Surgeon present, to examine with his *forceps*, if this body adhered firmly to the *velum*, or was loose; on trial he found it strongly adhered. The lad complained of no pain on his taking hold of it. The circumambient parts of a somewhat deeper red than natural; his breath stinking, and highly offensive. He was but little thirsty; pulse quick, but sufficiently strong; slept but little; what sleep he had was disturbed; he breathed much better up than in bed; here he was always in danger of suffocation, and feared it. After pronouncing a prognostic disagreeable to myself, and all concerned, I ordered the slough, as I then thought it, to be well rubbed once in 3 hours with a mixture acuated with *Spir. Sal. marin.* by means of a silver probe armed with cotton, after which an astringent, detergent, antiseptic gargle was to be frequently used, and a cordial mixture to be taken at proper intervals. After rubbing with the probe, &c. twice, and gargling often, in a violent fit of coughing with a deal of slimy filthy stuff from the pipe of the lungs, the membrane (*Fig. 64.*) separated from the *velum palatinum*. It was really the external and mucous coat of the part, was not rotten like a slough, but retained, tho' dead, it's membranous structure, was strong, would bear handling, and stretching without breaking. It was at first thick (as near as I could guess from a bit remaining on the right side of the *uvula* parted from the hollow (*a*) in the figure, of about $\frac{1}{3}$ of a barley-corn in length) having it's fibres and cavities soaked with a very viscid and slimy matter, which, by washing in water, leaked off, when the membrane became evidently thinner. The lad immediately, as I was told, breathed better, without that noise and wheezing heard before, and was less hoarse; not, I am satisfied, from the separation of the membrane, but from that load of filth discharged at the same point of time from the distressed respiratory passages. But, as usual, this relief did not prove lasting. In an hour and half the noisy respiration began anew, his hoarseness increased, and his cough, tho' short and low, was busy and vexatious; now he appeared as if quite strangled, and in the agonies of death; now he would again revive; for a few days he was interchangeably in these different states; at length his father perceiving somewhat in his mouth, which he thought thick phlegm, thrust in his finger and thumb, and, taking hold of it, drew it out. It was a hollow bag, as he thought, filled with rot and corruption, for a considerable quantity run out of it. It was, when full, he said, as big as his thumb, and of many inches in length. The agonies of the child, during these moments, were not to be expressed; his face was livid or black; but, being freed from this burthen, he soon revived, smiled, and said, now I am easy. Being put to bed, he soon slept, and continued to have short naps for 2 hours.

I got to the house, being sent for in the beginning of the lad's extremity, a few minutes after the affair was thus concluded. The account

Fig 65.

greatly surprized me; but I was more surprized, when, on sight, I found the supposed bag was the mucous coat of part of the *larynx*, the whole *aspera arteria*, with the grand division of the bronchial ramifications. I spread it on paper, for the conveniency of carriage, being some miles from home, and thence took it's likeness with great exactness, as here sent you, *Fig. 65*. There was somewhat bloody visible about it's middle. It was more rotten and tender than the former, also somewhat thicker, excepting where it belonged to the branches of the *bronchia*. What sweated from it was as sticking as bird-lime. It was probable this morbid affection ran thro' the whole *bronchia*; for the ends plainly discovered a laceration; consequently much more remained to be separated and discharged. He now complained of soreness in the pipe, and pointed to the first and second *costa*, as the place of it's termination. His inspiration was now free, soft, but short: his pulse was become a little more frequent and weaker. Examining his mouth, no ulcer or wound was discernible in that part of the *velum*, &c. to which *Fig. 64*. adhered. 'Twas smooth, clean, and looked only like a new skin not quite hardened. While I was in the house, he spit off another membrane of an irregular figure, thinner than either of the former, but more than sufficient to cover a crown-piece. It came from the *fauces*. After this I was informed he brought off with difficulty another tubular membrane of some length; and whenever he had strength to expectorate, little bits of the same were observed mixed with a very slimy *mucus*.

He lived 21 hours after the second coat was drawn from him, and died in the end somewhat suddenly, tho' in his perfect senses. I must add, that I never saw one in this disorder attacked with a *delirium*.

A gibbous
sternum; by
Jo. Jac. Hu-
ber, in a let-
ter to Dr Mor-
timer. N^o.
492. p. 95.
Read April 6.
1749.

II. I have observed an extraordinary conformation of the breast in the bodies of several children. But none ever exceeded that of a child of 8 weeks, in which the *os pectoris* was so pressed by the sides that it appeared as prominent as a saddle. The anterior parts of the upper ribs, excepting 2 or 3, appeared to be pressed inwards, within the breast on each side, but more on the left side than on the right, where they are very weak and soft, and joined with the cartilages of the *sternum*: and indeed the cartilages externally seemed hollow: so that some of the ribs almost looked as if they were broken, one of them however seemed to be pressed inwards at small distances more than the others. At last, on opening the body, I found several of the ribs gibbous toward the cavity of the breast; the cartilages of which were in a manner knotty and so did not a little contribute to an unusual contraction of the cavity of the breast. Hence it will be no difficult matter to apprehend the *phthisis* of those children, who have lost their lives by this miserable deformity. This is abundantly confirmed by the infarction and adhesion of the lungs on all sides to the *pleura*.

On a diligent enquiry into the cause of these disorders, by various observations and dissections, I concluded, that the whole blame was to be

Fig. 64.



Fig. 65.



be ascribed to those who had the care of young children. They are not sufficiently acquainted with the plastic structure of those tender little bodies: nor do they sufficiently consider what methods are to be used in quieting those which are cross, or diverting those which are lively. The greatest injury of all is, when they place their left hand under the buttocks of the child, and bend the body of it forwards, placing their right hand against it's breast, with their fingers extended, and so toss it up and down, with the whole weight resting on their hand. Hence it often happens, that the force of their fingers is impressed on the tender and yielding substance of the breast.

Before I happened upon this cause of the above deformity, I did not attend to some distinct pits in the ribs. But upon due consideration, I found these pits or depressions to be owing to the marks of the five fingers impressed on the breast of the child. For on applying my right hand to the breast of the child, I found that my 4 fingers exactly corresponded to the depressions on the left side of the breast, and my thumb to the larger one on the right. This detestable manner of treating young children will, I hope, be sufficiently apparent from what has been said: and I heartily wish that it may now be left off. It may prevent many disfigurations, consumptions, and other diseases. Last winter I made a similar observation in a girl of two years old. I have only one thing to observe, that this deformity is by no means to be confounded with the rickets.

III. A controversy has lately risen, concerning the manner of respiration, the history of which it is not my intention to repeat: and therefore I shall reduce the experiments to two heads, which seem to decide the question.

The first doubt was, whether the internal intercostal muscles elevate the ribs, as *Mayow* discovered; or depressed them, according to the opinion of *Galen*, *Bayle*, &c.

The other doubt was, whether there is an elastic air between the *pleura* and the lungs, or whether the lungs are continuous to the *pleura*.

I have dissected about 60 living animals, besides making other experiments, which I should be glad the R. S. would repeat; and if they find them to be the same, it may put an end to the controversy.

As to the first question. Lay the pectoral muscles of a dog open by raising the skin, especially in the upper part, where the thing is more evident. Then remove both those muscles, and destroy as much of the exterior intercostal muscles, as is sufficient to observe the condition and action of the internal ones. It will be proper to force the animal to inspire with great vehemence, which may be done by perforating one part of the breast, so that the air may render one lung useless. Hence the animal makes use of the remaining lung with great force. But it is not necessary to perforate the *pleura*, and the irritated dog, even without that force, respire strongly on touching the wound with alcohol. It will

Experiments relating to Respiration; by Albert Haller, Archiater & Conf. Aul. Reg. Prof. Med. Gotting, and F. R. S. N^o. 494. p. 325. Jan. &c. 1750. Read Feb. 1. 17⁴⁸.

will appear, in inspiration, that the intervals of the ribs approach as near to each other as possible, that the internal muscles labour vehemently, swell and harden; and that all the ribs ascend, except the first, which is but a little moved, that they are all turned round an imaginary point, which is in the cartilaginous appendage not far from the *sternum*, that the part of the rib joined to the *sternum* may descend, and the part the most remote from it may ascend, and be turned outwards. In expiration, all the ribs descend, the first again being excepted; the intervals in vehement expiration become longer, and are stretched, and the internal muscles are at rest.

As to the second. Take any animal either alive or dead, sink it under water, and carefully perforate the *pleura* under water, so as not to hurt the lungs. If there is any elastic air between the *pleura* and the lungs, it will shew itself by raising bubbles in the water, if none appear, I need not say it is manifest that there is no air. This experiment I have many times made, without so much as once seeing any bubbles; whence I think I may conclude, without being too precipitate, that there is no elastic air between the *pleura* and the lungs.

The case of a
lad, who was
shot through
the lungs;
drawn up by
Mr Nich.
Peters, jun.
Surgeon; in
a letter from
Will. Hallet,
M. D. to Hen.
Pemberton,
M. D. F.R.S.
& Chem. Prof.
Gresh. N^o.
474. p. 151.
June, &c.
1744. Read
Nov. 8. 1744.

IV. Dec. 28. 1737. James Channon, aged about 14, was accidentally shot in his back by another lad, at the distance of 2 yards from him; so that the whole load of shot, not having space to scatter, entered like a ball, by the edge of the left *scapula*, which it splintered; and, slanting upward, passed between the 2 superior ribs, and fractured the clavicle; the resistance of which bone hindered their passage thro the skin; for some of them lay immediately on the fractured part, covered only by the *cutis*; which, with a touch of the incision-knife, I took out, in number about 12: they were the small mustard-feed shot. After reducing the fracture, I drew off 3x of blood (he having lost but very little by the wound); and treated the wound in his back as usual in gun-shot wounds; and the fever which attended it, as a common symptomatic fever. In 8 or 9 days time a plentiful suppuration came on, and his fever abated. Towards the middle of Jan. the discharge of fetid pus was so great, not only through the wound, but also by expectoration, that I thought he could not long survive it: at each time of dressing (which was morning and evening, till the quantity lessened) full 3vi of pus were discharged: the like quantity he would generally cough up between the dressings. When the dressings were removed, I frequently made him force a cough, and try if he could not throw out any pus by his mouth; but, instead of passing that way, it flew out through the wound, like water from a pump: if I blocked up the wound with tow, he could then freely discharge it by the mouth. When the matter had done flowing, the air which was forced thro' the wound by coughing, would blow out a candle, which I often experienced. The matter was so prodigiously fetid, that, for some time after he was dressed, the stink in the chamber was scarce tolerable; and it was near the middle

of *March* before the discharge began to abate. In this space of time he coughed up 25 shot; had frequently hectic heats, and night-sweats; a quick feverish pulse returned constantly towards evening, with great thirst; he had lost his appetite, and was greatly emaciated; his chief food was milk, and physick the bark. In order to give a free passage to the matter thro' the wound, and prevent the stink from killing him (as the poor boy expressed it), by coming through the mouth, I had for some time kept a *cannula* in the wound; but, in less than a fortnight, I found myself obliged to leave it quite out; for tho' it answered the end of giving the matter a free passage that way, and prevented it's coming by the mouth, yet the quantity thro' the wound increased daily, and his hectic heats became more violent. Seeing no prospect of any end to be put to the discharge of matter, it keeping up to it's usual quantity for a month or 6 weeks longer, and the poor boy reduced to a mere skeleton, I was determined, if possible to heal the wound, and commit the event to nature; there not being one favourable symptom to give the least hope of his recovery.

About the middle of *June* the wound was quite cicatriz'd, 2 or 3 exfoliations being first cast off from the *scapula*. His cough still continued with a discharge of the same fetid *pus*, but in 3 weeks it began to abate; and, towards the latter end of *July*, he had gained flesh, and his cough had left him; he walked abroad, and was, to appearance, quite recovered. But this fair prospect did not last long; for, towards the latter end of *Aug.* I was called to him in the night, and found him supported in the bed, with a half-pint basin in his hand, almost full of the same sort of stinking putrid matter, which he used to cough up: it had been emptied but $\frac{1}{4}$ of an hour before, so that, in less than $\frac{1}{2}$ an hour, he had expectorated a full pint. This cough continued upon him 16 hours longer; when, the load of matter being pumped up, he grew much better. Two or 3 days before this severe attack, he had complained of being faint, feverish, and strait at the breast, for which he was bled, &c. In this fit of coughing, he brought up with the *pus* 14 shot. He had 3 of these violent returns before the summer was quite over, which reduced him nearly to his former weak state, but discharged no shot.

In *Nov.* following, I laid on a caustic to the *cicatrix* of the wound in his back; and kept it open with a large bean, to try if a discharge, by way of issue, might divert the matter from coming by the mouth: he had no such violent seizures afterward, but still a hectic cough upon him, which expectorated a small quantity of the same fetid *pus*: the discharge from the issue was pretty considerable, and he weathered out the winter tolerable well.

In *March* 1739, he grew feverish, and complained of a great load and pain just above the diaphragm, on the left side: I applied a warm plaster, and drew off $\frac{3}{4}$ of blood, which I found was pretty much inflamed. A few days afterwards an abscess formed between the ribs, where he had before complained of the pain; which I opened, and discharged.

charged about $\frac{3}{4}$ iv of the same fetid matter, and 18 shot. Here was a true *empyema*, and I had now great hopes of a cure, nature having pointed out such a depending part, for a discharge of what matter might be lodged in the *thorax*. I then healed up the issue in his back, and kept this new wound open with a *cannula*; but, within 10 days, the matter had ceased flowing, his feverish symptoms again increased, and his cough returned with a discharge of the same putrid matter. I threw aside the *cannula*, and healed the wound between the ribs, it answering no end to keep it open longer. The remaining part of the year 1739, he had several returns of his cough, with pretty large expectorations, but they were not of long continuance; frequent bleedings, a milk-diet, and vulnerary medicines, were made use of.

In the years 1740, 41, 42, towards spring and autumn, he generally was seized with a difficulty of breathing, and pain of the side: bleeding would relieve him for the present, but it seldom ended without a discharge of the same sort of *pus* by coughing, and with it, sometimes, one or two shot. At the latter end of the summer 1741, he had an abscess formed in the left side, between the true and spurious ribs: I opened it, and with the matter discharged 9 shot. Between these grand fits of coughing (if I may so call them), which happened 3 or 4 times in the year, he would gain strength, grow fat, and work at his trade of glove-making.

Towards the latter end of *March* 1743, his cough returned again with the same usual violence, and the discharge in one night was a full pint of fetid bloody *pus*; half that quantity was expectorated next day. He had the same feverish symptoms a few days before this cough, as usual, but rather more severe. It continued upon him 8 days before it began to abate. No shot were discharged at this time, as was expected; but he coughed up a broad scale of a bone, ragged at the edges, and of an irregular shape, which, I imagine, was a part of the *scapula*. A few weeks after this he was taken into your hospital.

The case continued; by Dr Hallet.

He had the benefit of the *Devon* and *Exeter* hospital, under my care for 15 months; during which time he was hectic, had sometimes pleuritic pains, for which he was often bled, and took soft pectorals. He frequently spit *pus* in great quantities. I confined him to a milk-diet; ordered him balsamics, particularly *Bals. Locatelli* in an electuary. He is now healthy, strong, and fat; and frequently walks from *Topsham* to *Exeter*, which is near 4 miles, and returns the same day.

Observations on a case published in the last volume of the Medical Essays, &c. of recovering a man dead

V. There are some facts, which, in themselves, are of so great importance to mankind, or may lead to such useful discoveries, that it would seem to be the duty of every one, under whose notice they fall, to render them as extensively public as it is possible. The case which gives rise to the following remarks, I apprehend, is of this nature: it is an account of “*A man, dead in appearance, recovered by distending the lungs with air; by Mr Will. Tossack, Surgeon in Alloa;*” printed in

in P. 2. p. 605. Vol. V. of the *Medical Essays*, published by a society of Gentlemen at *Edinburgh*; an abstract of which will be sufficient in this place: those who desire an ampler account may consult the article itself.

A person suffocated by the *nauseous steam* arising from coals set on fire in the pit, fell down as dead; he lay in the pit *between $\frac{1}{2}$ an hour and $\frac{3}{4}$* ; and was then dragged up; *his eyes staring and open, his mouth gaping wide, his skin cold; not the least pulse in either heart or arteries, and not the least breathing to be observed.* In these circumstances, the Surgeon, who relates the affair, *applied his mouth close to the patient's, and, by blowing strongly, holding the nostrils at the same time, raised his chest fully by his breath.* The Surgeon immediately felt 6 or 7 very quick beats of the heart; the thorax continued to play, and the pulse was soon after felt in the arteries. He then opened a vein in his arm; which, after giving a small jet, sent out the blood in drops only for $\frac{1}{4}$ of an hour, and then he bled freely. In the mean time he caused him to be pulled, pushed, and rubbed, as much as he could. In one hour the Patient began to come to himself; within 4 hours he walked home; and in as many days returned to his work. There were many hundred people, some of them of distinction, present at the time. This is the substance of the account; from whence it naturally appears how much ought to be attributed to the sagacity of the Surgeon in the recovery of this person. Anatomists, it is true, have long known, that an artificial inflation of the lungs of a dead or dying animal will put the heart in motion, and continue it so for some time; yet this is the first instance I remember to have met with, wherein the experiment was applied to the happy purpose of rescuing life from such imminent danger.

Bleeding has hitherto been almost the only refuge upon these occasions: if this did not succeed, the patient was given up. By bleeding, it was proposed to give vent to the stagnating blood in the vein, in order to make way for that in the arteries *à tergo*, that the resistance of the heart being thus diminished, this muscle might again be put in motion. But, in too many instances, we every day are informed, that this operation will not succeed, tho' the aperture is made with never so much skill: nor is it likely, that it should, when the blood has lost considerably of it's fluidity, the motion of the heart, and the contractile force of the solids, are at an end. Chaffing, rubbing, pulling, the application of stimulants, are too often as ineffectual as bleeding. The method of distending the lungs of persons, dead in appearance, having been tried with such success in one instance, gives just reason to expect, that it may be useful to others. It may be a proper inquiry, In what cases, and under what circumstances, there may be a prospect of applying it with success?

It will at once be granted, that when the juices are corrupted, where they are rendered unfit for circulation by diseases, where they are exhausted, or where the tone and texture of the solids is injured or de-

in appearance, by distending the lungs with air. Printed at Edinburgh, 1744; by John Fothergill, Licent. Coll. Med. Lond. N^o. 475. p. 275. Jan. &c. 1745. Read Feb. 21. 1744-5. Now printed with additions.

stroyed, it would be extreme folly to think of any expedient to recover life. But where the solids are whole, and their tone unimpaired by diseases, the juices not vitiated by any other cause than a short stagnation; where there is the least remains of animal heat, it would seem wrong not to attempt so easy an experiment. This description takes in a few diseases, but a greater number of accidents. Amongst the first are many of those which are called sudden deaths from some invisible cause; apoplexies, fits of various kinds, as hysterics, *syncope's*, and many other disorders, wherein, without any obvious præ-indisposition, persons in a moment sink down and expire. In many of these cases it might be of use to apply this method; yet without neglecting any of those other helps, which are usually called in upon these melancholy occasions. It is not easy to enumerate all the various casualties; in which this method might be tried not without a prospect of success; some of them are the following: suffocations from the sulphureous damps of mines, coal-pits, &c. the condensed air of long-unopened wells, or other subterraneous caverns; the noxious vapours arising from fermenting liquors received from a narrow vent; the steam of burning charcoal; sulphureous mineral acids; arsenical *effluvia*, &c. Perhaps those, who, to appearance, are struck dead by lightning, or any violent agitation of the passions, as joy, fear, surprize, &c. might frequently be recovered by this simple process of strongly blowing into the lungs, and by that means once more communicating motion to the vital organs. Malefactors executed at the gallows would afford opportunities of discovering how far this method might be successful in relieving such as may have unhappily become their own executioners, by hanging themselves. It might at least be tried, if, after the criminals have hung the usual time, inflating the lungs, in the manner proposed, would not, sometimes, bring them to life. The only ill consequence that could accrue from a discovery of this kind would be easily obviated by prolonging the present allotted time of suspension. But this method would seem to promise very much in assisting those who have been suffocated in the water, under the above mentioned circumstances; at least it appears necessary to recommend a trial of it, after the body has been discharged of the water admitted into it, by placing it in a proper position, the head downwards, prone, and, if it can be, across a barrel, hoghead, or some such-like convex support, with the utmost expedition.

It does not seem absurd, to compare the animal machine to a clock; let the wheels whereof be in never so good order, the mechanism complete in every part, and wound up to the full pitch, yet, without some impulse communicated to the *pendulum*, the whole continues motionless. Thus, in the accidents described, the solids are supposed to be whole and elastic, the juices in sufficient quantities, their qualities no otherwise vitiated than by a short stagnation, from the quiescence of that *moving something* which enables matter in animated bodies to overcome the resistance of the *medium* it acts in. Inflating the lungs, and, by this

Fig. 66.



Fig. 67.

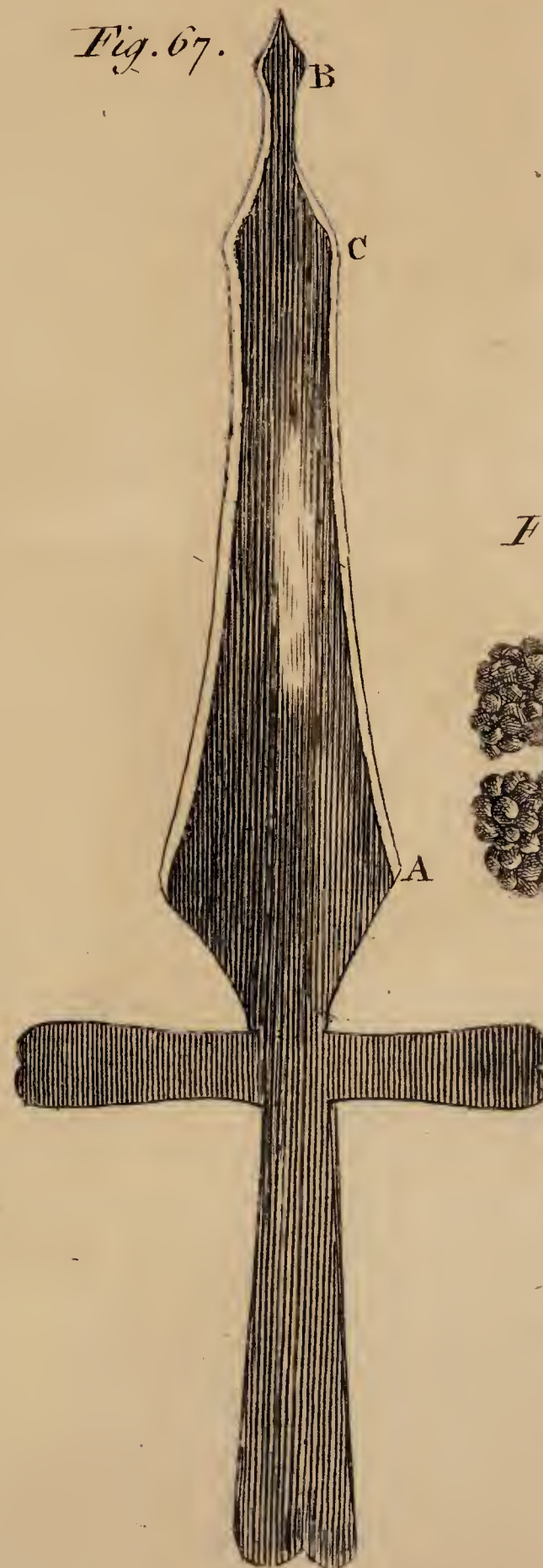


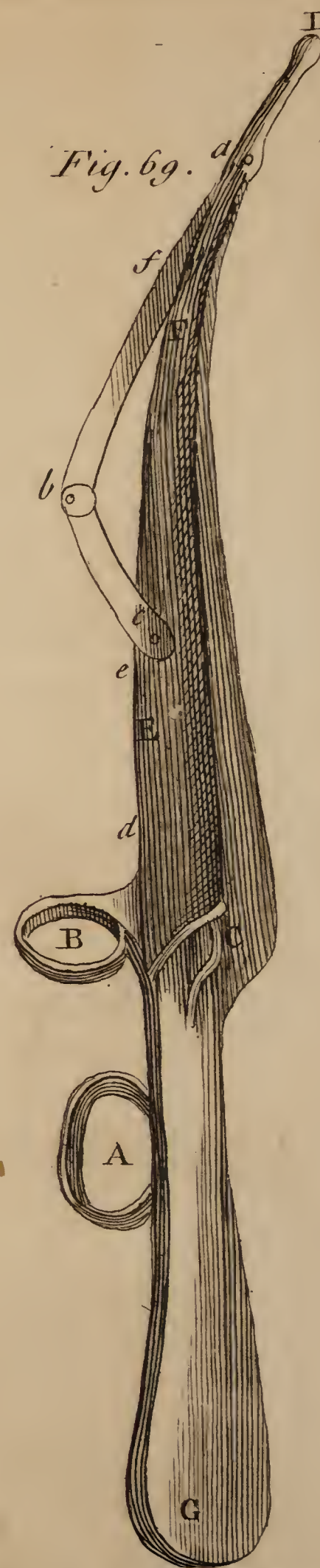
Fig. 68.



Fig. 70.



Fig. 69.





this means, communicating motion to the heart, like giving the first vibration to a *pendulum*, may possibly, in many cases, enable this *something* to resume the government of the fabric, and actuate it's organs afresh, till another unavoidable necessity puts a stop to it intirely.

It has been suggested to me by some of my acquaintance, that a pair of bellows might possibly be applied with more advantage in these cases, than the blast of a man's mouth; but, if any person can be got to try the charitable experiment by blowing, it would seem preferable to the other: *1st*. As the bellows may not be at hand: *2dly*, As the lungs of one man may bear, without injury, as great a force as those of another man can exert; which by the bellows cannot always be determined: *3dly*, The warmth and moisture of the breath would be more likely to promote the circulation, than the chilling air forced out of a pair of bellows.

To conclude, as I apprehend, the method above described may conduce to the saving a great many lives, as it is practicable by every one who happens to be present at the accident, without loss of time, without expence, with little trouble, and less skill, and as it is, perhaps, the only expedient of which it can be justly said, that it may possibly do *great good*, but cannot do harm, I thought it of so much consequence to the public, as to deserve to be recommended in this manner to your notice: for though it is already published in a work which is generally read by the Faculty; yet, perhaps, it may be over-looked by some, forgot by others, and perhaps, after all the care that can be taken, it may never come to the knowledge of a tenth of those who ought not to be ignorant of it.

C H A P. V.

The A B D O M E N.

I. **T**HIS uncommon case or disorder of the liver, I lately observed at the opening the body of Mrs *A. B.* deceased, aged near 40, whom I had attended some weeks before. The complaint was a constant acute pain on the region of the liver, with a swelling, or more than ordinary fullness on that side; by pressing of which was perceived a fluctuation of some fluid lying deeper than just under the first teguments. This was confirmed by Mr *Sherwood*, the Surgeon who assisted and examined the same. The body was opened by his son, when the liver was found of a prodigious size (there was a small adhesion to the *peritonæum* without inflammation): it spread over the stomach quite to the spleen on the left side, and contracted much the cavity of the *thorax*,

An extraordinary cystis in the liver, full of water; by Charles Jernegan, M. D. Lic. Coll. Med. Lond. No. 475. p. 305. Jan. &c. 1745. Read March 14. 1744-5.

by pressing and thrusting up the *diaphragma*. On opening the great lobe of the liver, there issued out above 4 quarts of a limpid water, from a cavity formed by the proper containing coat of the liver; though the water itself had been contained in a single conglobated gland, and there formed a *cystis*, which had burst, and was found loose at the bottom of this large cavity. This skin or *cystis* was not so thin but still capable of further expansion.

The liver still did it's function of separating the gall: the gall-bladder and it's ducts were in a good state: the *lobulus Spigelii* was much enlarged, and crumbled easily like a mass of congealed blood.

The patient had no particular thirst; nor was there any alteration in the urine, as to quantity more or less. But she had this symptom, common in the *hydrops pectoris*, of not bearing any other posture but that of leaning forwards on her breast.

The left kidney, being longer than usual, was examined and found to have two ureters; and each had it's separate *pelvis*.

Of the dissection of a human body, in which the gall-bladder was wanting; in a letter from Jo Jac Huber, M D. to Dr Mortimer, R. S. Sec. N^o. 492. p 92. Apr. &c. 1749. Read Apr. 6. 1749.

II. In the body of a woman 60 years of age I found the liver very large, and of an unusual thickness: but the right lobe had not any concave part. There was not the least trace of any excavation for the reception of the gall-bladder: but that whole lobe, both on that side which reaches to the intestines, and on that which touches the diaphragm, was smooth, even, very tumid, convex on both sides, and in a manner round. In this lobe, as well as in every other part of the liver the gall-bladder was sought for in vain: nor did any of the neighbouring parts discover any thing that could be supposed to have answered the use of that part; nay they were all formed after the usual manner. But this want of a gall-bladder seemed to be in some measure supplied by the unusual largeness of the hepatic duct, which would admit of one's finger. This, tho' seated in it's usual place, had some singularity in it's structure, having it's coat almost equal in thickness to those of the arteries: however it was not white, but yellowish, downy on the inside, and marked with several minute spots, resembling simple *folliculi*. From the liver quite to the *duodenum* this duct kept the same diameter, and so was almost confounded with the *vena portæ*: but when it came to enter the coats of the *duodenum*, it lost that unusual diameter; and making use of the *papillæ* in the cavity of the *duodenum*, exonerated itself that way, I believe, during the whole course of the woman's life. There were many *pori biliarii*, or branches of that duct, flowing together from each lobe, very large like arteries, and easy to be traced to the utmost circumference of the liver, all of them yellow without, and within full of bile. All the ducts contained a bile sufficiently bitter, and not small in quantity, and especially that great one formed on the outside of the liver. It is my opinion, that this largeness of the duct was instead of the gall-bladder, and that the extraordinary distention of it served to detain the excreted bile, till it grew more sharp, and at last

last to evacuate it, when it's quantity and acrimony were increased, as the true time of digestion came on. This largeness of the ducts was assisted in performing the office of a gall-bladder by the simple *folliculi*, which being dispersed all over the coat of the great duct, naturally poured out a mild liquor, by which the whole duct was defended from the acrimony of the remaining bile, by which means the superfluous quantity could be retained till the proper time of a greater evacuation.

Give me leave to add, that 15 years ago, I saw in a dissection at *Strasburgh*, the *ductus cysticus* and *cholodochus* so far distended as easily to admit of ones finger. But this great amplification to no small number of stones generated in the gall-bladder. Some were still remaining in the duct, of the size of a filberd, but soft, and in some measure suiting themselves to the shape of the duct. But the *ductus hepaticus* was of the usual form.

III. He was in the vigorous time of life, being no more than 36 years of age; and, to all appearance, of a strong well-set habit. His way of living was quite regular; but his practice of midwifry, which was pretty large, often forced him in severe weather from a warm bed into bad roads, and sometimes into raw uncomfortable houses.

He had for several years complained of uneasiness at his stomach; but it was not considerable, till about *January 1746-7*. From that time, he almost constantly threw up his food within an hour or two after taking it, and he felt violent pain about the *scrobiculus cordis*. Divers Physicians were advised with, but medicines availed him nothing; nor had he any ease, except from opiates, or spiritous liquors; and this was of short continuance.

It being, in the *Sept.* following, recommended to him to go to *Bath*, he for some weeks drank the waters, and afterwards bathed. The first had no remarkable effect; but he found himself worse after bathing. Upon his return home, new Physicians were consulted, and new methods were tried, but to no purpose; and, to make life tolerable, he was forced to be very free in the use of spiritous liquors and opiates.

In *Feb. 1747-8*. he voided, by 2 or 3 stools, about 3ij of matter. Some weeks before his death the pains went off, and his vomiting was at times stayed; but whenever that happened, whatever he took ran directly through him. And indeed he was now-and-then, during the whole illness, subject to bilious dejections.

On this remission of the symptoms, his friends flattered themselves yet that all might do well; but his wasting, which had long began, continued; and his legs, especially one of them, became oedematous. After growing gradually weaker, till nature was quite spent, he expired, with the utmost serenity of mind, in the evening of *Aug. 7*.

His body being, pursuant to his request, opened in the presence of *Dr Scarling*, and 3 or 4 surgeons, the coats of the stomach were found changed into an uniform, white, inelastic, almost cartilaginous substance.

The case of Mr Smith, Surgeon, at Sudbury in Suffolk; the coats of whose stomach were changed into an almost cartilaginous substance; by Mr J. Sayer; communicated by the Rev. Mr Murdock to C Mortimer, M. D. Sec. R. S. N^o. 491. p. 39. Jan. &c. 174 . dated Clare, Oct. 11. 1747. Read Feb. 9. 1748-9.

stance, which was $\frac{4}{10}$ of an inch in thickness. Besides this strange alteration in it's coats, the stomach was so contracted, as to be incapable of holding more than ℥v or vi ; and it's inner surface was besmeared with a various coloured matter. The rest of the *viscera* seemed to be quite unaffected, and every thing was in it's natural situation, except the *omentum*, which, besides being, as it is in all tabid bodies, much wasted, was necessarily drawn upwards by the contraction of the stomach.

Remarks.

It is highly probable, that this Gentleman's disorder, whether constitutional or acquired, was at first an obstruction in those glands, which separate the humour that serves to defend the villous coat from the acrimony of what is taken into the stomach, and to prevent it's being stimulated by the aliment in digestion; for want of which it was so subject to irritation, that scarce any thing would stay upon it. The matter voided by stool was undoubtedly formed in the stomach, because he never complained of considerable pain in any other part; besides, had it been from an abscess in the intestines, or any other of the *viscera*, the seat of it would in all likelihood have been apparent. The looseness, which in the latter part of his illness, always attended him when the vomiting ceased, plainly shews, that the stomach had at that time acquired a great if not it's greatest degree of contraction; for which reason, as it could contain but little, any quantity of food must, if not thrown up, go immediately downwards. The going off of the pain some weeks before his death, was owing to the sensibility of the coats of the stomach being in a great measure, or quite destroyed. The bilious dejections, that frequently attended him, may be ascribed to want of digestion; which, as little or no chyle was sent into the *duodenum*, rendered the bile useless. The consequence of this was a non-secretion of that humour, an accumulation of it in the liver, or gall-bladder; it's being reconveyed into the blood; or it's going off by stool. If the first or third had been the case, it would have shewn itself in a jaundice; if the second, there would have been an abscess in the liver or gall-bladder; so that of course it must run off by stool. Spirituous liquors might help to bring on this contraction, inelasticity, and insensibility of the stomach: but it seems pretty clear that they were not the sole cause; else immoderate drinkers of them would generally be affected in the same manner.

The operation
of lithotomy
on women;
by M. le Cat,
M. D. &
F. R. S.
Translated
from the
French by
T. S. N^o.

IV. The lateral way of cutting for the stone, which I have used on men since 1732, naturally led me, in 1735, to cut the widow *Neel*, a Farmer near *Yvetot* in the *Pais de Caux* in the same manner, as has been seen in the observation. In this operation the common grooved staff served me as a director; and having made the incision, on the left side of the *urethra*, with the strait grooved knife, which I used that year in cutting men, I withdrew the grooved staff, and introduced the groove of the knife. Immediately after this operation, I shortened the work,
by

by reducing the 3 instruments to 2. For that purpose I contrived to add to the common gorget *AA*, Fig. 66, 67. a grooved staff *BB*. After placing and tying the patient in the same manner as for cutting men, I at once introduce into the *urethra* the end *B* of the gorget, which constitutes the grooved staff, I turn the groove towards the patient's left buttock: on this groove I push the knife (*urethro-cistitome*) Fig. 68. which is not grooved, nor so broad as that which I made use of in 1735 on the widow *Neel*. Having laid open the neck of the bladder, I lay aside the knife, and thrust the gorget farther into the bladder; for example, as far as *C*. Then I pass the fore-finger on the gorget into the bladder, to dilate the neck; which done, I introduce the gorget as far forward as is necessary, and on it the *forceps*. The rest of the operation is performed as on men.

The first of the sex, whom I cut in this last method, as appears by my journal, is *Magdalen le Marchand* of the *Pais de Caux*, aged 22, cut in *May* 1738. I extracted a large stone from her, and she was cured in 10 days. Since that time I have constantly practised this method, which has succeeded perfectly well. When the stones were small, the patients were cured in a few days: but here is one, whose speedy cure has somewhat surprizing in it; inasmuch as I really believe it the only one which has happened so.

Mary le Comte of *Diepdal* near *Rouen*, aged 12, cut *May* 24, 1740, had a stone of a midling size. In 3 hours she retained her urine, so as not to discharge it but voluntarily. I thought it was the pretty common effect of the inflammatory swelling, which frequently happens after the operation; and that the suppuration would soon relax these parts, and open the wound; but I was mistaken. There was not the least suppuration. She performed all the functions of this organ, as usual; and being tired of the bed, to which she was confined against her will, she got up the third day, in good health, without any accident supervening.

At the same time that I was labouring to improve the manner of cutting women, and shorten the operation, I contrived another gorget, (Fig. 69.) which, besides the grooved staff of the former, contained within it the cutting instrument, which was to make the incision or enlargement; that is to say, the three instruments in one: and this instrument was attended with this advantage, that it could serve for men as well as women. One hand is sufficient to perform the operation with this instrument; but as the other hand became in a manner useless; and moreover, as it is probable that a person can better depend on an incision made directly with the hand, than on those which are made by machines or springs. I have not hitherto used this instrument; tho' possibly the habit I had contracted of using the others has some share herein. However that be, I have annexed the figure and description of this gorget, that those who like it, may make use of it.

Explanation
of the figure
of the gorget,
which M. le
Cat calls gor-
geret urethro-
cistitome.

Fig. 69.

A, A ring, for passing the middle finger of the right hand, which grasps the handle *G*. *B*, Another ring which is slid by the fore-finger towards *d*, to push the blade *ab*, out of the groove *EF*. The figure represents the instrument in the state in which it is at the very instant when the incision is made into the neck of the bladder. The same fore-finger draws the piece *B* back towards *A*, when the operator intends to make the blades return into the groove *F*; where they lie hid, while he introduces the instrument from *D*, as far as *Ff* into the *urethra*. The groove *FE* is closed or covered from *d* to *e*, in order to secure the pieces *a*, *b*, *c*, *d*, *e*, in situation. The pins, which bind the hinges *a*, *b*, *c*, must not be in the center of the pieces, but as they are expressed in the figure; where *b* is pretty near the outer edge, and the other two *a*, *e*, on the opposite side; to the end that, when the ring *B* is thrust forward, the hinge *b* may bend, and issue out of the groove *F*, by making the angle *abc*.

An extraordi-
nary calculus;
by John Hux-
ham, M. D.
F. R. S. N^o.
474. p. 208.
June, &c.
1744. Read Dec. 13. 1744 Fig 70.

V. I send you an exact draught of a stone lately taken out of the bladder of a boy about 12 years old; which I think of a pretty uncommon figure; tho' not indeed so very remarkable as that mentioned *Philos. Transf.* N^o 450. The boy died 2 or 3 days after the operation: so that the parents keep the stone as a relique of their unfortunate child, and will not part with it.

Remarks on
the operation
of cutting for
the stone; by
Claud. Nic
Le Cat, M. D.
F. R. S. Surg.
to the Hôtel
Dieu at Rou-
en, and R.
Demonst.
Anat. and
Surgery.
Translated
from the
French by
T. S. M. D.
F. R. S. N^o.
476. p. 391.
Apr &c. 1745.
Read at seve-
ral meetings;
finished May
16. 1745.
ARTICLE I.

VI. All the methods of cutting for the stone may be commodiously divided into the *high apparatus*, wherein the incision is made above the *os pubis*; and into the *low apparatus*, wherein the incision is below the *os pubis* and *scrotum*. In the first, the stone is extracted through the upper opening of the *pelvis*: in the second, the stone is taken out through the lower opening of the same *pelvis*. The *low apparatus* may be likewise divided into *direct* and *lateral*. The *direct* is the *greater apparatus* [or cutting on the staff]: the *lateral* is of 4 sorts.

The *lateral apparatus* of the first sort is that which is done without the staff [or *catheter*], and wherein the operator has no other guide but the stone itself, which is pushed forward, as much as possible, towards the *perinæum*. This is called the *lesser apparatus* [or cutting on the gripe], which *Celsus* has described. It is the oldest of all the ways of cutting, and may be looked on as the source of all the other sorts of the lateral operation.

The second sort of lateral *apparatus* is that wherein the operator makes use of a grooved staff, on which he cuts the [inner] end of the *urethra* lengthways, and makes laterally on the inside of the prostate, and on the neck of the bladder, an incision about two lines deep, or a sort of laying open, which only makes way for the dilatation or laceration. This is the method of cutting which I use, after having had it from Mr *Morand*, who learned it of Mr *Cbeselden*. And it is to this I have endeavoured

endeavoured to give the improvements which are already known in the world, and which I intend speedily to publish. In my opinion, one may rank in the same class the method whereby *M. de la Peyronie* lays open laterally the neck of the bladder, with instruments differing but little from those of the greater *apparatus*. [For] this method, as it has been communicated to me by that great Surgeon, scarcely differs from ours but in the instruments.

The third sort of lateral operation is that, wherein, under the direction of the same grooved staff, he cuts quite thro' the neck of the bladder, the prostate, and the [inner] end of the *urethra*. This is, properly speaking, Frier *Jaques's* method rectified; that which Mr *Chefelden* practised last, that of Mr *Sharp* his disciple, and, probably, that of Mr *Rau*; if, after all, it is not of the second sort.

The fourth sort of lateral operation is that, in which, without touching the *urethra*, or neck of the bladder, the incision is made into it's body, on one side of the neck. This is ascribed to Mr *Rau*; but I am of opinion, that the first persons who tried it on the living body were Dr *Bamber* and Mr *Chefelden*, who soon after abandoned it; after them, Mr *Foubert*, who endeavoured to improve it; and, in fine, myself, who am in hopes, that I have given it those degrees of perfection, which were essentially wanting in the methods of those who went before me.

Frier *Jaques's* manner of cutting, quite imperfect as it was, is the source, or, at least, the occasional cause, of all the new methods of the lateral operation. Immediately after Frier *Jaques*, Mr *Rau*, who was qualified to correct the defects in the method of the former, invented a particular one, of which he never shewed more than the outward part, which he could not conceal, and made a mystery of the essential part of the operation. The only particulars that have been known with certainty, are;

ART. II.
Remarks on
the fourth sort
of lateral
operation,
commonly
ascribed to
Mr Rau.

1. That he made use of a grooved *catheter* passed into the bladder without being injected; and that he himself held this *catheter* in his left hand during the operation.

2. That he made the outward incision between the left *erector* [*penis*] and the *accelerator* [*urinæ*], and carried it down to near the buttock on one side of the *anus*; which he did by several strokes of the knife.

3. That he made the inward incision with the same instrument, which was like the common incision-knife.

But, in Mr *Rau's* operation, we know not what parts he cut in his inward incision: however, from the foregoing circumstances, I believe I can demonstrate, that this Surgeon never performed the fourth sort of lateral operation, of which he is said to be the author; and that his manner was, at most, to cut through the *urethra*, the prostate, and the neck of the bladder, as is done in the second and third sorts of this operation. [For]

1. Mr *Rau* made use of a grooved staff. Now, in order to cut into the body of the bladder, the grooved staff is quite useless; and even the

common staff is generally of little or no use, because the end of the staff, that answers to the body of the bladder, is plunged very deep towards the *pelvis*, and also very apt to slip; for which reason Lithotomists, such as the celebrated Mr *Chefelden*, who resolved to try this manner of cutting, have been obliged to inject [the cavity of] the bladder, that it's body might be the less subject to slip [from the knife]; but made no use of the groove of the staff, as deeming it useless. *Incisionem in sulco catheteris fieri non posse, sive necesse non esse, ut sectio in sulco fiat.* Douglas in *Heister* on the lateral operation. But Mr *Rau* made use of the groove; therefore he did not make his opening into the bladder through it's body.

2. This Lithotomist did not inject the bladder; and yet his operation was quick and safe: wherefore, it cannot be that he cut into the body of the bladder. For, even with the new staff of my invention, which makes a considerable elbow forward, the incision into the body of the bladder is tedious and difficult. And I can assure you, from experience, that this same incision with the common staff is so difficult, that it comes near to an impossibility; and that it is absolutely impossible to be always sure of making this incision in one certain place, and without fatal mistakes, in this method, even supposing the bladder injected. Therefore Mr *Rau*, who did not inject it, would have, *à fortiori*, performed an impossible operation, and with success too: therefore this Surgeon did not cut into the body of the bladder.

3. Mr *Rau* held the staff with his left hand, and did the operation with his right. Those who do the lateral operation of the first 3 sorts do not certainly find both their hands too much for cutting into the *urethra* and the prostate, without injuring the neighbouring parts: and yet it is pretended, that Mr *Rau* could open the body of the bladder very exactly (an operation which I have proved above to be impossible in his manner); it is pretended, I say, that he did this operation with one hand, a thing which is more than possible; since, in order to do it with the new staff, which projects forward, besides the two hands of the operator, which are absolutely necessary, we are obliged to make an assistant put his finger, or an instrument instead of it, into the *rectum*, to keep the gut clear of the place of the incision.

4. Mr *Chefelden*, in the first trials he made of this pretended method of Mr *Rau*, opened the body of the bladder, and found himself under a necessity of relinquishing this method, because putrid ulcers were formed, in the course of the cure, in the cellular membrane that surrounds the bladder and *rectum*. Now Dr *Heister*, a disciple of Mr *Rau*, says, This accident never happened to Mr *Rau*; therefore he did not open the bladder in it's body: for there is no reason why he should avoid this accident rather than Mr *Chefelden*.

5. Mr *Rau* made all his incisions, the inward as well as the outward, with the same instrument, whose make was much like the common cutting knife, according to Dr *Heister*. This last circumstance makes me think

think, not only that Mr *Rau* did not cut into the body of the bladder ; because, with so broad an instrument, and the little precaution he used, as we have already seen, he would never have succeeded ; but likewise this broad instrument gives me a violent suspicion, that this Lithotomist did not so much as cut into the neck of the bladder ; and that he only laid it open, as is the practice in that masterly operation with the greater *apparatus* executed in Mr *de la Peyronie*'s manner : for it is well known what precaution must be used in the several lateral operations, to remove the *rectum* from the prostate and the end of the *urethra*, in order to cut these parts without touching the gut. Wherefore, in all the several ways of the lateral operation, not only the fore-finger of the left hand of the operator is necessary, but also, it is requisite that the finger of an assistant, or an instrument introduced into the *anus*, should contribute to keep off this same gut. Whether Mr *Rau* opened the body of the bladder or not, his disciples believed he did : and as they were witnesses to the great success of their master, they have not failed to conceive and give the public an high opinion of a method of cutting which opened the body of the bladder with all the safety that could be expected from a true method. The trials made in *England* having failed of success, it was pretty natural in *France* to think, that the persons, who made these first trials, had not light on the true manner of making this opening, so greatly cried up, and so much desired. M. *Foubert*, Surgeon of *Paris*, flattered with these hopes, added to the common instruments for cutting, the grooved trochart of the *paracenthesis*, which he proposed to thrust in between the tuberosity of the *os ischium* and the *anus*, directly into the body of the bladder ; and on which he was afterwards to introduce a sharp instrument of his invention, to make a proper wound for extracting the stone. He made trial of these instruments on a dead body ; and, in fine, he cut a certain number of patients for several years successively in that manner. People are divided on the success which attended these trials : much has been written against it ; and I have heard a great deal said in it's favour by good judges. One advantage, which flattered me in this method, was that of it's not being liable to occasion incontinencies of urine, nor even *fistula*'s, as I was assured ; inconveniences from which the lateral operation is not exempt in the case of large stones, because this operation attacks the bladder in it's neck. But one defect of M. *Foubert*'s method, which his very partizans cannot help taking for an essential inconvenience, is, that the operator thrusts the trochart in toward the bladder without any thing to guide him, and, as it were, by guess. 'Tis needless to enlarge on the fatal consequences of this defect ; they appear at first sight ; and are sufficient to make one refuse giving even the name of a method to so uncertain a way of cutting. Nevertheless, if this way of cutting had otherwise great advantages, and that one could clear it of that blemish which overcasts it, by giving it this guide which it wanted, and rendering it's process steady and certain, it must be allowed, that it would

prove an excellent method, a lateral operation of the fourth sort, worthy of being put upon a *par* with, or perhaps of having the preference of, the other three.

Such are the reflections I was led to make on M. *Foubert's* operation, by the good things I have heard of it, and the ill consequences which I knew attended it. I endeavoured to find an instrument that could fix the incision into the body of the bladder to the place intended: and here is [the description of] the staff which I invented for this purpose: such as it is when I pass it into the bladder.

Fig. 71.

Fig. 71. *A*, is it's crooked end, which is split lengthways into two pieces; the concave piece of which, *A*, is fixed, and of the same piece with the rest of the staff; and the convex piece, *C*, is moveable, having it's fixed point joined by a hinge to the end *a* of the piece *A*, and it's moveable part jointed at *b*, with a piece which makes the end of a strong stilet [or wire] that runs through the centre of the piece *B*, where it is riveted at *e*. This piece *B*, the wire, and the piece *C*, are held in the situation which the operator puts them in, by the screw *E*, the end of which bears against the piece *B*. This is made of 2 solid plates of silver soldered together; in the middle of which a groove has been made to lodge the wire. The handle, *D*, is square, especially on the inside, in order to serve as a sheath for the piece *B*, and give it a firmness, which it communicates to the wire, and to the moveable piece *C*. The body, *G*, of the *catheter* is almost intirely solid, leaving in it's centre but just room enough for the passage of the wire. Without this solidity and this narrowness of the passage of the wire, the *catheter* would not be firm; and the wire itself, liable to waver, would not run true, but would impart it's weakness, or want of justness, to the moveable piece *C*, which is the guide of this operation. This body, *G*, is soldered to the piece *D* at *H*, making it enter square into the said piece *D*, till it meets the slider *B*, which I suppose intirely within the piece *D*. The rings are very large and strong, for the conveniency of using it. This *catheter* is made of silver, from the rings inclusive to *F*: all the rest, together with the wire, ought to be of the hardest gold; because it is on this end of the *catheter* that the greatest stress is laid; and silver has not firmness enough to resist the efforts that these pieces must sustain. And particular care must be taken, that all the angles and prominences be rendered very smooth. I enter into these details of the make of the instrument, because I have learned, to my cost, that the workmen do not think of them.

Fig. 72.

Fig. 72. shews the whole mechanism of this *catheter*, by representing it open, and such as it is in the bladder while the incision is making. The piece *B*, of *Fig. 71.* is here sunk in it's sheath *D*; whereby the small style or wire is thrust towards the crooked end of the *catheter*, and, at the same time, pushes the end *b* of the small moveable piece *C* towards this same part. The *catheter* being thus open in the bladder, when the operator draws the instrument towards him, it is stopped by the neck
of

of this organ, at the place marked *dd*; and then the angle *b* projects about a finger's breadth from the orifice of the bladder. Yet there are some subjects, in whom this orifice, being very wide or relaxed, gives greater way to the effort made by the widening, *dd*, of the anterior angle of the *catheter*; whereby it happens, that, instead of stopping this angle at *dd*, it lets it pass thro' to *ff*, which brings the incision so much nearer the neck of the bladder: nay, I have seen in some dead bodies, in which the relaxation is still greater, that the prostate was somewhat concerned in the incision; which is no great misfortune. But even this may be easily avoided, by taking care, in the first incisions, to disengage the part that answers to the projecting angle from every thing that may hide from us the prostate and bladder; and then the foregoing case becoming visible, it is easy to guard against it, by causing the projecting angle of the *catheter* to be pushed, or by pushing it one's self farther into the bladder. This projecting part of the *catheter* is not seen; but it is very perceivable to the touch, through the integuments; and still more so, after they are cut through. I must not omit observing here, that, notwithstanding all the care I have taken to instruct the instrument-maker in the construction of this *catheter*, and especially of the moveable piece *bC*, in order to make it solid: yet it has often proved too weak to bear the effort of thrusting the part forward, which we are obliged to do on one side; so that it bent, and remained in the middle, while the rest of the *catheter* was to the left side. In case of this accident, it came into my thought to turn the *catheter* upside-down; so that the angle of the moveable piece might answer to the upper part of the neck of the bladder, and stop there, while the concave and immoveable part of the *catheter* answered to the incision, and that the very end of the *catheter* projected at the place where I was to open the body of the bladder. And, upon several trials, I found that this place was the very same which had before been pointed out by the angle of the moveable piece; therefore, when I have one of these *catheters*, on which I cannot depend, I make use of it in this last manner; and it intirely answers my expectation; because the fixed piece of these *catheters* is always very solid, and that the angle of the moveable piece does it's duty as well on the upper as on the under side of the neck of the bladder. It has even seemed to me, that the end of the instrument makes the greater protrusion forward. In fine, this *catheter*, being almost strait, easily assumes in the bladder every situation which one finds necessary to give it.

Fig. 73. represents the incision-knife, which I use. It is the same *Fig. 73.* that I call *urethrotome* in my common lateral operation; excepting that here I give a greater length to the back. *A* is the handle; *BC* the blade; of which *B* is the great edge, *C* the back, *FE* is the little edge. In the middle of this blade is a chanel, that ends with the point of the instrument at *E*. The little edge *FE* must not go beyond the point *F*, if the operator would spare the neck of the bladder, when he plunges the instru-

instrument into this organ; for, if it be made longer, it must carry the incision as far as into the prostate.

The manner
of performing
the operation.

Having placed the patient as usual, I pass an hollow *catheter* into the bladder, through which I half inject the bladder; because I have found, that, as a moderate injection renders the operation more speedy and safe, so a complete injection forces back toward the *rectum* the place appointed for the incision, and makes the operation laborious and dangerous. When I have made the injection, I draw out the hollow *catheter*, and pass in my new staff by half a turn; which I make very short, upon account of it's little convexity. I push it to the bottom of the bladder; and when I am quite sure it is there, I give to the moveable piece *b C*, the situation requisite to make the protrusion answer the places where I intend to open the body of the bladder. The places which I have chosen in the trials I have made of this method are two; the first is between the two *vesiculæ seminales*, close to the left, under the orifice of the left *ureter*; the second is above the orifice of the *ureter*, and over the left *vesicula seminalis*. For during the operation pursuant to the first intention, when my staff is in the bladder, I keep it's rings exactly in a horizontal position, so that the moveable piece *b C* may bear directly on the line, which may be imagined to pass between the orifices of the *ureters*. In this situation, I loosen the screw which stoppt the sliding piece *B* of *Fig. 71*. I thrust in this piece, which opens the staff, and makes the elbow or angle *b*, *Fig. 72*. I then fasten the screw to fix the staff thus open: I afterwards draw the rings towards me, as if to draw back the staff, always in a horizontal position, till the angle *b d a* stops me. Then, being sure that the elbow *b* of my staff has passed directly between the orifices of the *ureters*, and that it is a good finger's breadth from the neck of the bladder, I raise up the handle of my staff a little, carrying it softly toward the right thigh, and I give to the rings an oblique direction approaching to a diagonal, in order to push the elbow *b* toward the space between the *anus* and left *os ischium*. An assistant holds the staff in this attitude; another passes the fore-finger of his right hand, or an instrument made for this purpose, *Fig. 74*. * into the *anus*, and pushes down the *rectum* on the right side. With my left thumb I secure the integuments, resting it on the middle of the *perinæum*; and with the right hand I make, with the incision-knife, *Fig. 73*. a long and deep incision between the *os ischium* and the *anus*, beginning on one side of the place, where ends the incision with the greater *apparatus*. This first incision generally lays open no more than the common integuments: then, with the fore-finger of my left hand, I feel for the elbow of my

* Tho' this instrument serves me here for removing the *rectum* from the parts destined for the incision, it was not for this use that I invented it, but to make a *speculum ani & matrixis*, being joined to another intirely like it, with it's angle and groove placed on the angle and groove, *AB*, of this. We shall have occasion, in another place, to give a more ample description of this instrument, and it's advantages over the other sorts of *speculum*.

staff; and on this elbow I cut upward and downward; first, the *septum*, formed by the *elevatores ani*; secondly, the ligamentous *lamina* that supports these muscular expansions, and which, without an accurate knowledge of those parts, may be easily mistaken for the bladder. These obstacles being well removed, the elbow of my staff becomes more and more disengaged, and the end of the prostate and beginning of the bladder are laid bare. Then, being certain of the place I am at, I feel again for the elbow of my staff: I set it right, if the assistant has let it slip from it's due position, and on it I plunge the knife into the bladder, so as that it's point runs on the outside, and the whole length of this elbow, and the great edge *B*, *Fig. 73.* faces the posterior part of this organ. By this incision I cut into the bladder an inch long or more, if I think proper, a finger's breadth from it's orifice, under and close to the left *vesicula seminalis*, and the orifice of the left *ureter*. I had caused a groove to be made on the projecting piece *C*, *Fig. 72.* to direct my knife, but I found it of no use. On the groove of my knife, now in the bladder, I slide the gorget; and then the assistant, who held the staff, loosens the screw, draws the ring of the piece *B*, whereby the staff is brought back to it's former shape, as in *Fig. 71.* and then he draws it out of the bladder. The rest of the operation is performed in the usual manner. In order to open the bladder above the left *vesicula seminalis* with the same staff, as soon as it is in the bladder, one must turn the moveable piece *b C* *Fig. 72.* towards the left side of the bladder, by giving to the rings of the staff such an oblique direction, that they make an angle of about 45° with the horizontal line. In this state I open the moveable piece, and oblige it to make an elbow exactly in the place desired. Then I do my operation, as above described, which is the result of a great number of experiments made on dead bodies; in some of which I had injected the hypogastric artery. These numerous trials have constantly convinced me, that my staff is an instrument with which one is as sure as possible, always to open the body of the bladder in the place resolved on by the operator. For if it happens, that a bladder either too large or too small, or some other motive, obliges the Surgeon to make his incision farther from, or nearer to, the neck of this organ, he will fulfil that intention, by more or less pushing the piece with the slider *B*, and thereby causing the moveable piece *b C* to make a greater or lesser angle.

From trials on dead bodies I passed to operations on living subjects; and, having had some reasons to think, that the incision into the body of the bladder between and beyond the *ureters*, was preferable to that which is practised above the left *vesicula [seminalis]*. In autumn 1741, I cut 3 patients in this way; *viz.*

John Peter Desmarest, whose stone could not be extracted whole. He lost much blood during the operation, and after it; and died the 17th day.

Peter

Peter le Grand ; who died in 3 months after cutting, of a sinuous *fistula* in the *pelvis*.

Giles Laurence ; who had likewise an hæmorrhage. Of 3 or 4 soft stones which he had, the greatest part remained, and he died the 4th day.

As I was accustomed to good success in our lateral operation, I was concerned for the accidents that happened in these first trials more sensibly than another would probably have been. I publickly opened the 3 above-mentioned subjects. The incisions of the bladder were found to be most exactly done in the place above specified, without hurting any of the neighbouring parts : and it plainly appeared, that the death of these subjects was occasioned by the following inconveniences attending this method.

1. The hæmorrhage, which is almost inevitable from the depth of the incision.

2. The stripping of the gut of the cellular membrane that surrounds it ; which, together with the depth of this denudation occasions putrid ulcers by the irruption of the urine into the rest of the cellular membrane, behind the *septum levator-ani*, and thence into the whole circumference of the bladder. Mr *Chefelden* complained of this accident, when he tried his method.

3. The stones more difficult to be found, either with the fingers, or the instruments ; especially when they are lodged in the right side, and anterior part, of the bladder.

4. Even when the operator has laid hold of the stone, he finds more difficulty in extracting it than in any other sort of the lateral operation. I was formerly of the contrary opinion ; but experience has since convinced me, and I have plainly seen the grounds of this 4th inconvenience ; which are these : it is not the exterior integuments that ever hinder the passage of the stone ; for they yield too easily [to give any obstacle]. The *septum levator-ani* is not more difficult to dilate from the moment it has begun to be divided : wherefore the real obstacles to the extraction of the stone are, either the bones of the *pelvis*, the bladder, or the prostate. The bones of the *pelvis* give an equal obstacle to all the sorts of lateral operation : and even, generally speaking, in all the methods of the *low apparatus*, it is the same road, the same outlet, the same obstacle. The bladder presents as great an obstacle in the 4th sort of the lateral operation as in the 2d, or in ours. In the first, the wound of the bladder is made an inch long ; in the 2d, the bladder is laid bare for some lines, and then dilated the rest of the way. In both these the opening is the same, when the *forceps* is introduced. In extracting the stone, you must in both still dilate or tear as much as the size of the stone requires ; and, consequently, in this respect, the difficulty is the same. The prostate and neck of the bladder are the remaining obstacles to the extraction of the stone. These parts are divided in our method, and they are left whole in the lateral operation of the 4th sort. Now it is

is plain, and I have experienced it in the 3 operations I did, that the neck of the bladder and the prostate advance, whilst the stone is extracting, under the *pubis*, and against the interosseous ligament of the *os pubis*, and there form a considerable obstacle; and indeed so considerable, that, in the case of *J. P. Desmarest* it could not be forced by the greatest efforts: a circumstance which has never happened to me in the lateral operation these 10 years past that I have used it.

5. Tho' I have had the good fortune, in the foregoing operations, not to hurt the *vesiculæ seminales*, nor the *ureter*, much less the *rectum*; and that, with the precautions which I have laid down in the account of my manner of doing the operation, one always avoids these accidents; yet it must be allowed, that the above-mentioned organs are extremely near the incision; and that in so terrible and bloody an operation as that of cutting for the stone, one is not always in a condition to make so strict an examination as is requisite for shunning these dangers. For which reason I reckon them as one of the inconveniences of this method, especially of that which opens between the two *vesiculæ [seminales]*: and I have found it so on some dead bodies, which I cut by an affected negligence, without making use of that scrupulous exactitude of which I have just now made mention. In my opinion, the 2d method I have spoken of, wherein the bladder is opened above the left *vesicula seminalis*, is less exposed to the hazards above mentioned, especially with our staff. But it is subject to the other inconveniences, and also to this additional one of opening the bladder in a place thick set with [blood] vessels, and in particular with a very considerable *plexus*; as is well known to those who have dissected these parts. In fine, I do not pretend in this place to examine the advantages and inconveniences of all the different methods: this detail I have reserved for a more complete work than this paper: at present, I only seek to lessen the inconveniences of the general way of cutting by opening the body of the bladder; or, rather, I endeavour to improve it; and I am humbly of opinion, that the instrument which I have the honour to lay before the *Society*, may contribute to this end.

Whatever improvements have been made in the different methods of cutting for the stone by the *low apparatus*, there still remains in them several inconveniences, to which the high operation is not subject. These advantages of the high operation above all the other ways, have been learnedly treated of by celebrated authors *French and English*, and have not been contradicted by any one; so that it would seem as if this operation had been abandoned, in order to run after the *lateral* operation, pretty much as people quit an old fashion for a new one. This being a shameful circumstance in an art of such importance as surgery is, and with respect to so serious an operation as that of cutting, it was at length said, in justification of this change, that the *high* operation is not so general a method as the *low*; that it is not practicable on subjects of too full a habit of body, on indurated bladders, which are said to be

REMARKS on
the operation
of cutting for
the stone.

ART. III.

On the method
of cutting by
the high operation.

very common in persons afflicted with the stone. One might say, in answer to these objections against the *high* operation, that persons troubled with the stone are very seldom plethoric; for I do not remember to have met with one instance of it in above 15 years that I am conversant with this distemper: that most part of the indurated bladders become so, by the stones being lodged many years therein, and that such stones are of extraordinary size and weight; and that, in this case, the bulk of the stone alone pushes the bladder forward enough to be able to cut by the *high* operation; and the rather, because these subjects are so much emaciated, that, generally speaking, one may feel the stone above the *pubis*, through the very integuments. This I have experienced on one *Anthony Germain*, of 44 years of age, a native of *Calais*, but residing at *Diepe*; who coming to our hospital in order to be cut, and being dead of an accidental fever, even before the [usual] preparation, I cut him, by the *high* operation, on the very stone, and without injecting the bladder. This stone however was not excessively large: it is represented of half it's natural dimensions, together with the bladder, in

Fig. 75, 76. *Fig. 75.* and *76.* and it weighed but 8 ounces. But, granting that the *high* operation is not a general method, is there any one method universally proper in all cases? and even the *lateral* operation, which I look upon as the most perfect of all the sorts of the *low apparatus*, does it lay claim to this universality? it must be allowed, that it does not, whatever attachment one has to it. True it is, that very large stones are extracted by this method; but it is equally true, that the lacerations attending the extraction of these large stones are generally mortal, and always followed by *fistulas*. It is a decided point in practice, that large openings, whether made by incision or laceration, in the lower part of the body of the bladder, are almost all mortal: wherefore our posterity may spare the public from such murdering experiments. Thus the case of large stones is one of those, wherein the *lateral* operation becomes too fatal to venture putting it in practice; and besides, we are not destitute of examples to prove, that some stones are too large to be extracted by this method, even with all these risques it is exposed to. On the other hand, experience has long since determined, that the largest stones, even those on which the *low apparatus* has failed, are extracted by the *high* operation with ease, and constant success. This then is one case, wherein the *high* operation, if it be not an universal method, is at least the only one. I think this circumstance might have deserved more of the attention of Lithotomists; and that, while they cultivate new ways of cutting by the *low apparatus* (which are indeed useful in a great number of cases), they should not absolutely neglect the *high apparatus*, which [in it's turn] is necessary in several cases, wherein the former are either insufficient, or very dangerous. For, even supposing the number of these cases to be but small, the *high* operation is not the less a necessary supplement to the other methods for all cases that offer. Thus much I have learned by experience, that is, by the ill success of the different

different methods of cutting by the *low apparatus* in the case of large stones: and therefore I am resolved to follow the *high* operation in the said case. In adopting this way of cutting, as practised by Mr *Douglas*, *Chefelden*, and *Morand*, I thought I might, under the patronage of these great men, be able to make some improvements on it. An inconvenience, which always happened in doing this operation, is, that as soon as the knife has opened the bladder, the urine or liquor injected, which kept up it's side close to the integuments, comes off; the bladder sinks, and often slips from the instrument before the incision is made large enough; and then it is very difficult to find the bladder, and finish the operation, which by this means becomes tedious and painful. This accident has happened several times. In order to guard against it, I have thought of two things: first, instead of cutting the bladder downward, which contributes to the sinking-in of it's coats, I plunge the knife into the bladder behind the *os pubis*, and I cut it upward toward the belly; by which means the edge raises and supports the coats of the bottom of the bladder. When I see that my incision is large enough, I turn my instrument quick, so that the back may be where the edge was: and this back has a very smooth shoulder, as appears in *Fig. 77*. This Fig. 77. shoulder continues to keep the upper side of the bladder close up to the integuments. Then along the blade of the knife I introduce into the bladder the part *a* of the instrument represented in *Fig. 78*. and placing Fig. 78. this very easy smooth crotchet in the room of the back of the knife, I give it to an assistant to hold and keep up the bladder close to the integuments. This done, I put the fingers of my left hand very securely into the bladder, and examine, if it be sufficiently opened. I lengthen the incision, if requisite; and, if the stone presents itself to my fingers, I draw it out, if it can be done without difficulty; if not, I use the instruments represented in *Fig. 79, 80, and 81*. in this manner: by means Fig. 79, 80, of the first *suspensor*, placed at the upper angle of the wound, I slide 81. into the bladder the end *B* of one of the instruments of *Fig. 79. and 80*. I apply this second instrument, which I call *dilatato-suspensor*, under one of the lips of the wound: I raise it up, carry it close to the integuments, and give it to an assistant to hold. I do the same thing to the other lip of the wound with the other instrument exactly like the foregoing. Thus the wound of the bladder is kept close to that of the integuments in all it's parts; whereby the urine cannot ouze out towards the cellular membranes (a very common accident in the old way;) and it must all come through the exterior opening. In the case of little bladders, and small stones, the two last instruments are sufficient for this purpose; and then I draw out the first. The bladder being in this condition, in order to extract the stone I introduce either my fingers, or the *forceps*, or the scoop, *Fig. 81*. which I have found, by experience, to be much more commodious and sure than the *forceps*. To be able to use this instrument with the utmost advantage, one must practise with it a little on dead bodies: and I am bold to say, it will be

found much superior to the *forceps*; and that with it one will extract stones with ease and safety, which the *forceps* would either miss, or not draw without great pain. The essential reason of this superiority is, that the scoop takes up less room, and that it pushes the stone from behind, so that it can never slip back. This instrument, as well as the *forceps*, is passed into the bladder between the two instruments that support and line the lips of the wound. The passage for the *forceps* is, as it were, marked out on the back *B* of these instruments, *Fig. 79*, and *80*. which I have designedly made lightly concave, in order to direct the *forceps* or scoop, and prevent their going wrong. They are in the same manner drawn out, with the stone, between these two concavities; and it is easy to conceive what advantages must attend this contrivance. The whole stress of the operation falls on these concavities: all the contusions, all the rubbing, which these efforts might have caused to the lips of the wound, and to the bladder, bear upon these instruments: the shrinking of the lips of the bladder behind the integuments, which is another common consequence of these efforts; the tearing of the cellular membranes, which follows this shrinking, and makes way for lodging the urine, and forming purulent and mortal *sinus's*: all these accidents, I say, which are common in the usual *high* operation, and which have contributed toward disgusting some operators, are avoided by this aforesaid contrivance. The instruments that extract the stone touch the bladder no otherwise than to lay hold of the extraneous body: and, as soon as they have laid hold of it, they have nothing to do but with the *dilatato-suspensors*, which are so broad, smooth, and well polished, that they preserve the bladder from any hurt, dilate the lips of the wound as gently as the operator thinks proper, and prepare a slippery issue for the stone, which must render the operation equally expeditious and safe.

All that I have here said, has been practised upon one *John Goubert*, a lad of 17, of the parish of *Plane* in *Normandy*. In searching this lad, I judged the stone was considerable; and, from all the reasons above-recited, I concluded, that he ought to be cut by the *high* operation; which I performed on the 23d of *May* 1742. Instead of the table that commonly serves to cut on, I had prepared one of those little beds which are made by turners. I placed my patient so as to have his head turned towards the window, and his feet at the opposite part; the hips raised, the breast low, the head raised on a pillow. Two assistants, standing at his shoulders, took hold of his hands, and, unknown to him, threw a ligature on each wrist, which they fastened to the bedstead: two others did the same to his knees; for his legs were left hanging beyond the bed, and held by assistants. Having placed myself on the right-hand of the patient, I did the operation in the manner above described, having begun by injecting the bladder. The subject was not such as one would chuse; he had a strong fever, and was blooded twice the day of the operation. The second day he complained of great pain about the

the hips ; which, as he would not lie on his belly, I attributed to the urine got into the cellular membranes, and beginning to hurt the parts adjacent to the bladder. As the fever, and the other accidents which the patient complained of, were attended with a cold over all his body, and a paleness of his face, I bled him no more ; but put him into a warm bath at eleven in the morning. He had a clyster given him at 4 in the afternoon, and at night he was bathed again. He was put in on his back with the wound bare, that the water of the bath might enter in ; and, when he was put to bed, he was desired to lie on his belly. The third day in the morning, as the symptoms did not cease, he took a clyster, and was bathed at 11, and again at night. The 4th day he was bathed once more. He slept therein $\frac{1}{2}$ an hour, and the symptoms abated. This method of bathing might seem strange to practitioners ; but I have used it for many years with success in cases like this. The first year that I tried it, I gave an account of this practice to the *R. Acad. of Sciences*. The 5th day my patient was upon the mending hand ; and he lay regularly on his belly. The 7th day the accidents quite disappeared. The 15th day he was purged. The wound was almost closed ; he made water pretty easily thro' the *urethra* : but, what was singular, in order to make water thro' the yard, he was obliged to put himself on his belly ; and when he lay on his back, the urine came out through the wound. To prevent which, I put a common *catheter* into the *urethra*, which he could not bear : I changed it for one made like an S ; but that also he could not bear. A fever seized him, which made a greater quantity of urine pass thro' the wound ; so that I was compelled to let him lie on his belly, to suppress this efflux, which, one would be apt to think, ought to be facilitated by this situation. The reason of this uncommon appearance seems to be, that the opening of the bladder, and that of the integuments, no longer answering to one another, the posture on the belly applied the wound of the bladder, now shrunk, and become lower, against the *pubis* ; and thereby stopped up the passage of the urine [that way]. I allowed him to put himself in whatever posture agreed best with him : and, notwithstanding several indigestions, of which his greediness of eating was the occasion, he was perfectly cured.

Fig. 75. the bladder of *Antony Germain* opened throughout, and it's *Explanation* neck slit by two incisions. *AA*, the *fundus* of the bladder. *BB*, the neck of the bladder. *cc*, the orifices of the ureters dilated to the breadth of an inch. *dd*, a transverse fold, which imprinted on the stone the circular furrow which appears thereon. *of Fig. 75. and 76.*

Fig. 76. the stone of *Antony Germain*, of half it's natural size, viewed by it's posterior surface, which lay on that of the bladder represented by Fig. 75. *E*, the posterior end of the stone. *F*, the anterior end answering to the neck of the bladder. *gg*, the circular furrow remarked above.

N. B. The *catheter*, Fig. 71. and 72. is figured, and a short account given of it, in the *Med. Essays*, Vol. V. p. 466. C. M.

A proposal to bring small passable stones soon and with ease out of the bladder: by the Rev. Stephen Hales, D. D. N^o. 477. p. 502. Aug &c. 1745. Read Octob. 31. 1746. here printed with alterations.

VII. Being present, *February 4. 1744-5.* when the late E. of *Orford* (after having taken for two months Dr *Jurin's Lixivium*) voided at once 11 pretty large nearly cubical fragments of larger stones, which were involved in coagulated blood and urine; and, a few hours after, 15 more at once, in the same manner; in all 32 that day; some of which were as large as were possibly passable; it hence immediately occurred to my thoughts, that all passable stones which have lately fallen from the kidneys into the bladder, or which have broken off from larger ones, might readily and easily be brought out thence, by conveying into the empty bladder, by a *catheter*, some very mucilaginous substance, such as syrup of marsh-mallows, or a solution of gum-arabic, or barley-water. Such substances would bring the stones away soon, and with great ease to the patient; and thereby not only prevent much teasing pain, by fruitlessly endeavouring to bring them away with the weak force of thin urine, but also effectually to secure the patient from the danger of their growing too big to come away, by long continuing in the bladder. And what strongly evinces the reasonableness of this proposal, is, that, on opening the bladder of his Lordship, there were no stones found remaining, except two small grains, which were involved in the folds in the neck of the bladder. If, on trial, any stones shall be found too big to pass off, the patient is but where he was before; and if any shall be of such a size as to enter the *urethra* but part of the way, they may be pushed back, or cut out, according as their situation shall happen to be.

And further to evince the reasonableness of this proposal, I made the following experiments; *viz.* In order to shew the comparative force, with which fluids of different degrees of density and tenacity will impel stones, I took a glass tube, which was an inch in diameter within, and $14\frac{1}{2}$ inches deep; and, having filled it full of urine, I put into it a nearly cubical piece of a large stone, taken out of a human bladder, which weighed gr. $7\frac{1}{2}$; and, standing by a clock whose *pendulum* beat seconds, I found, by repeated trials, that the stone was $1''\frac{1}{4}$ in descending through the $14\frac{1}{2}$ inches depth of urine. The experiment being tried with the same stone in oil of olives, it was $5''\frac{3}{4}$ in descending: so that the resistance of the oil to the falling stone was 4.6, that is, more than 4 times greater than the resistance of the urine; and, consequently, the impelling force of oil to propel a stone in passing through a narrow tube, would be proportionably so much greater than that of urine, were their velocities equal. When \mathfrak{z} i of gum-arabic was dissolved in \mathfrak{h} ss. of water, the stone descended in $2''$; with \mathfrak{z} ij, in $3''$; with \mathfrak{z} ijj, in $4''$. In a decoction of warm barley-water, which was so thick as to be a tender gelly when cold, the stone was $45''$ in descending, that is, 35 times slower than in urine; and, consequently, the impelling force of urine

is 35 times less than that of this mucilage, in case their velocities were equal. This mucilage was, as I guess, of a due consistence for the purpose; for it was about the thickness of Lord Orford's coagulated blood and urine. Equal quantities of blood and urine will continue a thick *coagulum* for many weeks, without any separation. But as the velocity, with which such mucilaginous substances pass thro' small tubes, is considerably less than the velocity with which urine will pass; supposing the forces with which they are impelled to be equal; it was requisite to determine those different velocities by experiments: and, in order to it, I put half a pint of the same blood-warm decoction of barley into a glass vessel, where it's depth was near 8 inches, and therefore it's mean depth near 4 inches. It run out at the bottom in about 50'' through a glass tube, whose bore was $\frac{1}{7}$ th inch diameter; it's length 2 inches: and, on repeating the same experiment twice, as the decoction grew cooler and cooler, it was about 80'' and then 90'' in running out; whereas a like quantity of urine ran out thro' the same tube in 18''. Now, supposing the velocities, at a *medium*, thro' the *urethra*, to be as 72 to 18, then the velocity of the urine will be $\frac{3}{4}$ greater than that of the mucilage of barley. Taking therefore $\frac{3}{4}$ from 36, the force of the mucilage, the remainder nine is the force with which the mucilage will impel the stone; and, consequently, the impelling force of the mucilage, in the neck of the bladder and in the *urethra*, will be 9 times greater than that of urine; besides the advantage of greater slipperiness which it gives to the *urethra*.

VIII. Mr Simpson, of Little Ormond-Street, aged 75, after having been afflicted with the stone above 5 years, and taking Mrs Stevens's medicines about a year before for 7 months successively, without receiving any benefit, was cut by Mr Cheselden, March 13. 1741-2. at which time he had a fit of the stone upon him, which had continued for ten days; and when, consequently, the bladder and urinary parts were very much inflamed, this, together with his great age, made the success very doubtful. However, at his own earnest request, after consultation, the operation was performed, and a large flattish round stone was extracted, weighing very near $\frac{3}{4}$ iv. The wound bleeding plentifully from the small vessels, only a piece of thin wet sponge was introduced, that it might bleed for a while through; intending (if there should be occasion) to tie any vessel afterward, that should require it. But, contrary to expectation, this proved the means of stopping the effusion of blood; and, from experience, in many cases since, it has been observed, that nothing is so useful as this method (thus accidentally discovered). About six hours after the operation, the patient having lost but little blood, it was thought proper to take $\frac{3}{4}$ xii of blood from his arm. The urine flowed freely through the wound in about two hours after his being cut, and also thro' the *penis*; and continued to do so, though with great pain from the heat and sharpness of it; which excoriated

A remarkable case of a person cut for the stone in the new way, commonly called the lateral; by W. Cheselden, Esq; Surgeon to her late Majesty; communicated to M. Folkes, Esq; Pr. R. S. by Mr Reid, Surgeon at Chelsey, who attended the cure. N° 478. p. 33. Jan. & Feb. 1746. Read Jan. 23. 1745-6.

excoriated the parts about, notwithstanding his being drest every two or three hours, and the parts being anointed with a cooling ointment.

On the 4th day, not having had a stool since the operation, an emollient clyster was ordered; which purged him twice, gave him a great deal of ease, and abated some slight feverish symptoms that were observed. This was repeated once in a day or two for about 3 weeks, he being naturally costive.

About a week after the operation he complained of a pain in his sides, had little convulsive motions in his bowels, with faintings, and the *abdomen* swelled: but, on bleeding him ℥viiij , his pain was removed, and the other symptoms went off on taking the *Confect. Ralegh. in Aq. Pæon. Comp.* every night for a week.

During this time the wound began to digest, a large slough cast off, and in it came away several very small bits of stone, which had crumbled off in extracting the stone with the *forceps*. These, by obstructing the free passage of the urine, had given him sometimes great pain, but which was removed by frequently injecting oil and warm barley-water through the *penis* and wound.

At the beginning we made use of a fomentation made of *Absinth. Roman. & Flor. Chamæmel.* but it proving too sharp for the excoriated parts, it was left off, and the parts bathed with warm milk, and sometimes barley-water. His diet, the first 3 weeks, was nothing but spoon-meats and light pudding, afterward a boiled chicken, &c. and, in a fortnight after the operation, asses-milk night and morning, from which he received great benefit, being very thin, and having a cough, which had afflicted him many years, and being of a hectic disposition.

At the end of three weeks, the wound was above half healed, the urine began to come chiefly thro' the *penis*, daily lessening through the wound; and when that was near cicatrized, it all came the natural way, and had lost it's heat and sharpness; then his mouth blistered within, from the saltness of the *saliva*. For which, during the whole course of the cure, he drank plentifully of cooling emulsions with gum-arabick, &c. In 5 weeks he was perfectly cured, and continues to this day without any return of his distemper.

The effects of the Lixivium Saponis, taken inwardly by a man aged 75 who had the stone, and in whose bladder, after his decease, were found 214 stones: from W. Cheselden, Esq; Ibid. p. 36. Read Jan. 13. 1745-6.

IX. May 21. 1739. *Laurence Welch*, aged 75, born in *Denmark*, but at that time a pensioner in his Majesty's royal hospital at *Chelsey*, after having had symptoms of the stone for 8 years, but never known to void any in all that time, began to take ten drops of *Lixiv. Sapon.* in half a pint of water, 3 times a day; and on the 23d in the evening voided several small scales of stone, with little or no pain. On the 24th the dose was increased to 15 drops; but, making him very sick, was reduced on the 25th to ten. He complained of great pain in his back this day, and at night passed a whole stone, the size of an orange-kernel, and several little bits. On the 26th he complained of great foreness in the *urethra* when he made water; for which a little oil was injected, and

and he took ℥viij of a solution of gum-arabic inwardly, omitting this day the *lixivium*.

Being very easy on the 27th, he took the *lixivium* as before: his urine was thick and plentiful, but had no gravel or sand in it. On the 28th he took the *lixivium*, and about 4 in the afternoon voided several bits of small stone, of different make and texture, some as large as tares; which making the *urethra* very sore, on the 29th he omitted the *lixivium*, and took a solution of the gum, and had the oil injected as before. Being searched this afternoon, we perceived a great quantity of stone in his bladder. On the 30th he took the *lixivium* again, was very sick, but easy. The 31st, taking the *lixivium* after great pain in his back, belly, and *penis*, he passed a large piece of stone, with several smaller bits. June 1. he took nothing, was very easy, and made a great quantity of thick turbid urine. He took the *lixivium* again the next day, was very easy, and continued so all the 3d; but on the 4th was in great pain all day and night. On the 5th, the medicine having sat well on his stomach these 3 days, I ventured to give him 15 drops of the *lixivium*; and, after having great pain in his loins and *penis*, at night he voided several large bits. Early in the morning, after very great pain, on the 6th, he voided 3 large bits of stone, and in the afternoon several more pieces came away; one of which was the largest yet voided. He took the *Solut. Gum. Arab.* this day, and no *lixivium*; and at night, being faint and fatigued with pain and want of rest, he had an opiate and cordial julap. On the 7th he likewise took no *lixivium*, having great pain and sickness at his stomach. His urine to-day appeared a little bloody, and had a tough *mucus* in it, which sunk to the bottom of the pot, and was very fetid. In the afternoon a large piece of stone stopt at the extremity of the *penis*; which, after endeavouring for two hours to press out with his fingers in vain, was extracted, by enlarging the orifice. This stone weighed but gr. ixss, tho' near as large as a die; and two which he voided before, near as big, and of a triangular form, weighed, one, gr. viij; the other, gr. vij. This day he took the *Solut. Gum. Arab.* and at night had an opiate and cordial julap, to take a little of now-and-then. On the 8th he took nothing; and tho' he had a great deal of pain, and was very weak, had a better appetite than any day since he began the course. He complained of pain, in the morning on the 9th; so took no medicine; was very easy the rest of the day, and passed some very small scales. On the 10th he took the *lixivium*, and was pretty well. The 11th had great pain, and was very sick; and also the 12th: therefore he omitted the *lixivium* on the 13th; and, being very faint and low, had a cordial julep. On the 14th he took the *lixivium*, and was very easy every day till the 20th, when he complained his water made him smart; and also the 21st. The 22d he was very full of pain, and sick; therefore on the 23d he took only the *Solut. Gum. Arab.* was easy, and had a great deal of *mucus* come away with several small scaly bits of stone in his urine; and also on the

24th and 25th. He complained of great pain in the *glans penis* on the 26th; and his belly swelled, and pitted just above the *os pubis*; and he voided some small bits in a great quantity of *mucus*. On the 27th a few little bits were voided, and he had no pain all day. The 28th he had great pain, and made very little water; but on the 29th made a great deal of water. His belly was swelled, and he complained of great pain in his loins. — He voided some small bits of stone on the 30th, which was the last day he took the *lixivium*. The 1st of July he took the *Solut. Gum. Arab.* and was in great pain. On the 2d, being very faint, and wanting rest, he had a cordial julep and quieting draught; and the 3d and 4th; which day he complained of great pain in his limbs; and on the 5th had several loose stools; which however stopt on the next day. He continued languishing, and in extreme pain all over his body, even his fingers and toes, from this day to the 24th of July, not being able to bear being touched in any part of his body or limbs, some days before his death, which was on that day, being 64 days since the commencement of this course: in which time he took 81 doses of the *lixivium*, being one day with another (till he left off intirely, which was 24 days before he died) about 20 drops a day.

During the course, his water was generally thick and muddy; tho' when in great pain, sometimes clear and high coloured. In about 3 weeks after he began, there was a great deal of that tough fetid *mucus* in his urine, which continued till he left off taking the *lixivium*, and then decreased considerably, till a fortnight before his death, when it intirely disappeared, and his urine had no smell or taste, and came away involuntarily.

On straining and filtrating the urine, I often found little scaly bits, very small, which, when dry rubbed, became an impalpable powder. After he left off the medicine, I could never find this in the urine, tho' I searched very carefully.

His diet the whole time was chiefly spoon-meat; now-and-then a little meat, but very seldom; and he had but little appetite when he began, and was very weak.

On opening the body I found his kidneys very sound, and neither stone nor gravel in them: but in his bladder there were 214 stones; the two biggest about the size of nutmegs, the others gradually less, to the bigness of a pea, and smaller, but each whole, not pieces. They were of a soft chalky substance and colour, and weighed all together 3vj *Averdup*. ʒij gr. iv.; besides which there were several small scales, such as I found in his urine. The other *viscera* had nothing remarkable. The quantity of small chalky and scaly bits and powder that I could save out of the urine, was about as much in 24 hours as would well cover a shilling; and perhaps there might be as much more lost in his urine, when at stool, and on the sides of the pot and urinal.

X. The spring of the year 1743 furnished me with a new opportunity of confirming the goodness of this method; which, by certain circumstances was put to such a trial, that I question whether the stone could have been taken out by any other manner of cutting. For this reason I have thought this observation deserved a place, as a supplement to the account which I have presented to the Society concerning this method.

An observation of an operation made by the high apparatus, in 1743; by Claud. Nic. le Cat, M. D. F. R. S. &c. translated from the French by P. H. Zollman, Esq; F. R. S. N^o. 480. p. 175. May & June 1746. Read May 1. 1746.

Joseph Bunel, of 12 years of age, a native of *la Bucaille*, and living at *Vitry* near *Andely*, was afflicted with the stone for 8 or 9 years past. Friday May 17, 1743, being the day appointed to cut him, I prepared myself to do it by the lateral apparatus, which is the common method. At 5 in the morning the patient was placed in the usual manner for the operation; and I, endeavouring to enter the probe, in order to cut him laterally, found the stone so big, or at least so far advanced in the neck of the bladder, that I had all the trouble in the world to make the instrument enter; nor could I effect it, otherwise than by thrusting it quite on the side. I put my finger into the *anus*, and was convinced of the reality of the circumstances which the probe had made appear: I immediately thought that the situation of this stone in the neck of the bladder would hinder me from bringing the instruments freely to it; that, as this hard body seemed exactly to fill the bladder, it would be impossible to introduce a pair of pincers between it and the insides of the bladder; that, it's bulk being too considerable, would cause a mortal laceration by the low apparatus; and, lastly, that the patient was in such a case of necessity, as required to have recourse to the high apparatus. Not having suspected all these particular circumstances, and not being much inclined to make an operation in town, which I had as yet made but once, I had not brought my instruments with me for cutting by the high apparatus; but, seeing it would be a rashness in this case, to perform the lateral operation, and not to prefer the high apparatus to it, contrary to all my experiments and principles, I put off the operation to the next day, and sent to *Rouen* for my instruments for cutting by the high apparatus. That day at noon I gave the patient a very light soup.

Sat. May 18. I cut him by the high apparatus at 8 in the morning. The Surgeon who was gone to fetch the instruments, brought Mr *Grasse*, a Surgeon from *Dublin*, with him, who was come to *Rouen*, on the part of Mess. *Morand* and *Verdier*, to see my way of cutting; and I had 10 or 12 young Surgeons with me who were come from *Rouen*. The patient was placed upon one of those little beds which the turners make for the country people; the two extremities of the bed were raised, especially that which was to support the thighs, and the middle was hollow; the whole covered with *alaizes* *. The patient's head was to-

* These are cloths folded several times, and laid under the patients, to keep them cleanly.

wards the light ; his hands and feet were tied across the bed, in the places where they came to lie, when he was laid on his back ; his legs were stretched and open ; his arms brought down below his hips ; in a word, he was in a very convenient posture. I was on the right side of the patient. In this manner I thrust an *algalié* (or hollow *catheter*) into the bladder, introducing it on the side, as I had done the day before. I injected a syringe-full of lukewarm water into the bladder ; which occasioned a protuberance not immediately above the *pubis*, but 3 or 4 fingers breadth higher ; which plainly proved, that the stone took up the whole exterior and inferior region of the bladder. I opened the teguments, and thrust my instrument into the bladder close to the *pubis* ; but, finding there only one membrane, and the stone, I was obliged to bring the edge of my knife upwards, and then the instrument really entered, and abundance of water and urine came forth. I turned again my instrument, to support the bladder with the projecting part, which is on the back of it *. I slipped over it the strait *suspensor* (a *catheter* that opens with a bow), and dilated the bladder with the incision knife, towards the *pubis*, and introduced the lateral *suspensors* : I put my finger into the bladder, and, having felt a stone which was above the first, I pulled out this upper stone with the *forceps*, which broke between the lateral *suspensors*. I put my finger in again, and felt another ; which I took hold of, and pulled out, taking it for a third stone, though it was but a fragment of the first, which had escaped the *forceps*. This fragment being taken out, I put my finger in again, and felt distinctly that the bladder was parted into two chambers, like a gourd. In the upper hindmost chamber, which I had opened, the injection was lodged, and the stone which I had taken out. In the nethermost chamber I felt the great stone, which went as far as the neck of the bladder, the top of which was surrounded by the bladder like a neck, the opening of which did not admit more than the tip of one's finger, with which I felt that the partitions of the bladder were closely united, and adhered to the surface of this stone. These melancholy discoveries made me very uneasy ; this particular structure of the side of the bottom of the bladder making me sensible of the same impossibility of introducing my instruments, which I had met with toward the *perinæum*. I tried to thrust my finger by force between the stone and the bladder, by setting the nail strongly against the stone, and loosening the bladder from it, which seemed to adhere to it, dilating at the same time the bladder with all the force my finger was capable of. I at last introduced my finger to a certain length, with which I loosened the adherent parts from the stone all along as far as my finger could reach ; then I tried to pass in my *forceps*, afterwards my different scoops, but all in vain. I was for an instant believing that I could not get it out. This frightful idea made me redouble my endeavours. I began to dilate again with the fore-

* See the figures in Art. VI.

finger of my left hand, and, with the same finger, and the thumb of my right hand in the *anus*, I violently thrust the stone upwards towards the belly, after a long and painful labour, both for the patient and the operator. I introduced upon my finger, which was between the stone and the bladder, the small scoop of my double crochet (see *Fig. 81.*) I advanced it quite beyond the stone, and then I began to pull, having wrapped some linen round the instrument which hurt me ; but the *pubis*, under which the stone was, caused an obstacle, not to be overcome. I therefore ordered an assistant to push through the *anus* the stone towards the belly ; and I, on my side, having discovered the end of the stone under the *pubis*, put in the fore-finger of my right hand, and pressed as hard as possible the stone with it towards the spoon, which, at the same time, I strongly pulled with my left hand. These 3 forces being united, made the stone at last give way, and brought it forth to the joy of the spectators, as well as the patient. The upper side of the stone, that is to say, that which answered to the *pubis*, was pretty even, and seemed to give a passage to the urine ; the nether or hindmost side, which lay towards the prostates and the entrance of the bladder, kept the shape of these parts ; it's substance was crumbling, of a reddish colour, and like the membrane of the bladder, having a fungous poile on it, with which it seemed to have been incorporated, and of *Fig. 82, 83.* which a sort of covering yet remained upon it's surface ; this covering being taken off, and the stone a little dried, this whole appeared to be pierced with porosities, which seemed to have been the receptacles of the foresaid poile. There was no mark on this side of any urine having passed. A pledget charged with digestives, a rag dipped in an embrocation, and an emollient cataplasm, were laid upon the wound ; the patient was laid upon the belly, his breast being supported by one bolster, and his head by another.

Saturday, 1st Day. An hour after he was let blood 3 porringers full. He found himself a little out of order after bleeding, and had several *nausea's*. I had a simple cordial made for him with $\mathfrak{z}\text{v}$ of balm-water, $\mathfrak{z}\text{i}$ of syrup of barberries, and $\mathfrak{z}\text{i}$ of syrup of clove-gilly-flowers ; of which he took several spoonfuls a day, when his spirits seemed to fail him. He sweated much all the afternoon ; which made me defer bathing him till $\frac{1}{2}$ an hour after 7 ; his pulse was short, quick, and strong : the first hour he was in the bath made no alteration ; he was sick at heart, and spit up a white froth ; at last he fell asleep in the bath for at least an hour : his pulse seemed to be slower, and, coming out of the bath, he found himself very well. He was dressed as before at 10, and laid upon the belly. He slept best part of the night, which hindered his being blooded at 3 or 4 o'clock, as I had ordered.

May 19th, Sunday, 2d day.] At 8 in the morning I found him in a strong fever. I had two porringers of blood taken from his arm : The bleeding made his pulse short, low, and quick, and himself sick at heart. His pulse grew strong again, and the fever came upon him. I design-
ed

ed to bathe him yet that morning; which should have been done if he had been blooded earlier; but it was late; and, besides, he had had no stool. I therefore thought it necessary to give him an emollient and anodyne clyster at 10. He was afterwards dressed; and I deferred his bathing till 4 in the afternoon: but the clyster not being come from him, and the boy complaining of a pain in his stomach, I ordered him to be put in the bath from half an hour past 11, till half an hour past 1. He found himself very well in the bath; his clyster there came from him, and he had a good stool. He was put to bed again at a quarter past 1: there he had another stool, and found himself very well; nevertheless he was yet very feverish. He slept from half an hour past 4 till half an hour past 6; after which his fever was almost half gone. He was put into the bath again at half an hour past 7 in the evening. He desired it himself; partly on account of the ease which it had hitherto given him; partly because the situation of lying on his back, and sitting in the bath, was an agreeable change from that of lying on his belly; which he was obliged to do in his bed. His pulse was very well in the bath; his look and eyes were more clear than in the preceding bathings; and indeed he neither had any pain in his belly, nor at his stomach, nor in his breast, nor in his head; the wound only smarted now-and-then; he could not bear being touched about it; however his belly was very flat, and he began to be hungry. He staid an hour and half in the bath, and slept good part of the night and morning.

Monday, May 20, 3d day.] He did not wake till 8 o'clock: he had hardly any fever at all, and he found himself much better than the day before. I ordered another clyster to be given him, on account of the benefit which he had received from it the day before, and because he had not been at stool since. It made him sick at stomach again, and did not come from him. I had him put into the bath at 9. Part of his clyster came from him there an hour and half after. At every bathing I ordered a spoonful of cordial to be given him at going in, and towards the middle of the bath a mess of broth, or 2 half messes. He came out at the end of two hours. After this fourth bathing he found himself almost without any fever; and was so well, that he teized us and the nurses for something to eat. This determined me to let the supuration quietly fix itself; which seemed already to begin at the wound of the teguments. He made water this day once or twice through the *penis*.

Tuesday, May 21, 4th day.] In the morning he was without a fever; and the distemper which he complained of most was hunger. I then set out for *Rouen*. The patient continued to grow better every day. I returned the 10th day, to prescribe him a less strict diet.

The *cicatrix* of the bladder had formed itself on the 20th. That day no urine at all passed any more through the wound of the belly, a large bandage was laid over the wound, and he was sometimes put on his back; especially after he began to make water through the *penis*. The wound

wound of the teguments was intirely cicatrized on the 40th day ; and he was so well, that he came to see me at *Rouen* on the 50th day. He was grown so fat, that I hardly knew him again.

Fig. 82. represents the anterior and upper side of the stone.

Fig. 82.

Fig. 83. the posterior and under side, which rested partly upon the

Fig. 83.

prostatæ. This side of the stone was attached to the bladder. *a* the place where the body of the bladder was contracted in form of the neck of a gourd. *b.* the extremity, which was lodged in the beginning of the *urethra*. *c.* the impresson made by the nail of the forefinger of my left hand, in passing it between the stone and the inside of the bladder, in order to detach them from each other, and to dilate the bladder sufficiently to introduce the crochet.

XI. *John Cod*, of the parish of *Rock*, in the county of *Pembroke*, aged 40, was from his infancy troubled with nephritic complaints, and frequent obstructions of urine. He, at 25 years of age, had a total suppression of urine for some days ; when, upon a stone's rushing out of the bladder into the *urethra*, he had immediate ease from his pain ; and was therefore negligent concerning it's extraction from thence.

Part of a letter from Mr Geo. Howell, Surgeon, at Haverford-west, to Mr W. Watson, F. R. S. concerning the extracting a large stone by an aperture in the urethra. Ibid. p. 215. Read June 5. 1746.

Five years after, he married ; the stone remaining in the *urethra* ; which was then (as he imagines) about the size of a walnut. In the space of 4 years, his wife brought him 2 children. About that time he sent for me, and wanted me to dissolve the stone ; he having just then read some treatises speaking largely of the powers of lithontriptic medicines. I told him, that, admitting the power of the dissolvents of the stone might be very considerable, when the stone was in the bladder, and might be surrounded by the medicated urine, very little was to be expected in the posture the stone then lay ; but, if he was willing to have it cut out, I would undertake the operation. This he would by no means consent to ; and so we parted at that time. I heard no more from him for 6 years.

In *August* last I had a message from his family, desiring my assistance, they believing him dying ; he having had a total suppression of urine for 3 or 4 days. Just as the servant was coming with the message, his urine had forced a passage through the superior part, or *dorsum penis*, and had flowed out plentifully. Upon my coming to him, I proposed cutting out the stone ; his answer was, he would submit to whatever I should think necessary ; he being now convinced, that, if he were not speedily relieved, his death would be inevitable.

The whole quantity of urine, which was contained in the bladder, had made it's way through the new opening in the back of the *penis* ; and therefore, at present, the inflammation of the *penis* being very violent, I deferred the operation, and only applied a fomentation and cataplasm, to take off that inflammation.

I returned in two days ; and, by an incision into the *urethra* near 3 inches long, I extracted the stone, which had been there 15 years. It weighed

Relief in the Stone by Alicant Soap and Lime-Water.

weighed 3ijfs, and measured 6 inches round, in the direction it lay in the *penis*. About 50 small stones lay between this great one and the bulb of the *urethra*. These came forth from this incision. After this, I put in 5 pins, and made the twisted future, and then introduced a leaden *canula*. The urine passed regularly two days; when an inflammation came on, which tore out all the stitches. I tried once more, and took the greatest hold possible; but the *urethra* was so tender, that my endeavours to unite the lips of the wound were fruitless.

The man was glad to compound for life, even with the loss of the *penis*, if that were necessary, which the stone had increased to an enormous bulk. But, after my attending and dressing him about two months, the orifice, through which the stone was extracted, was reduced to half an inch in length from the *frænum* backwards. But altho' I introduced a leaden pipe near 3 inches long into the *urethra*, the urine never came through the natural orifice, but still continues to pass thro' the aperture.

The passage in the back of the *penis*, made by the urine before the operation, healed up kindly. He has since been much rejoiced, that the incision could not be healed, because he has lately voided several large stones that way; which he most probably must have been cut for again, had that been closed.

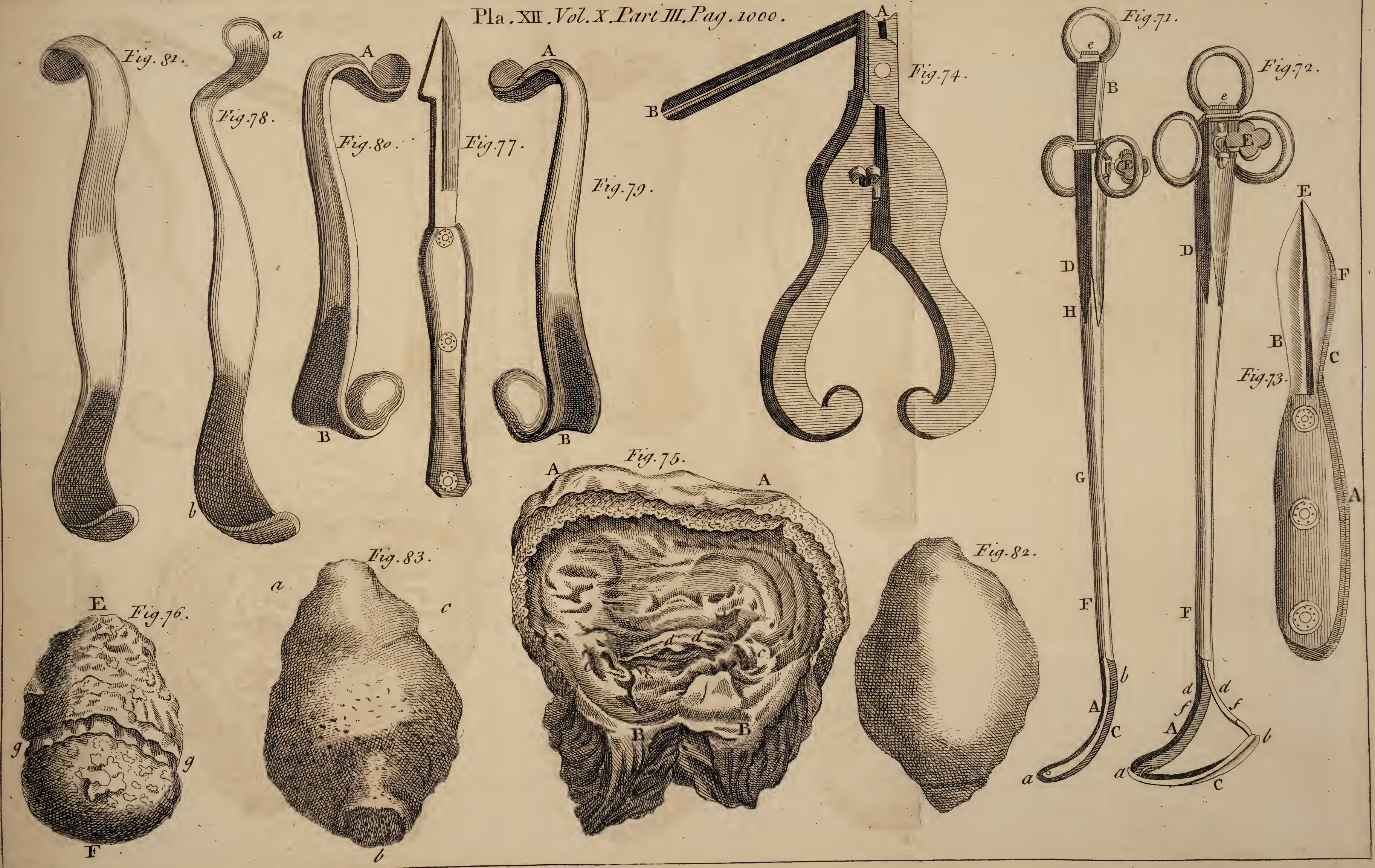
For several years his urine did not drop, but by the introduction of a knitting needle, or such-like instrument, which would move the stone: and this operation generally took up two hours every morning.

You will see easily which way the stone lay in the *penis*, by observing some holes therein, which the patient had made with the knitting needle, hoping to push through it in time. These holes are in the fore-part, or against the natural orifice. The other end you will find quite polished, by the friction of the small stones against it. The superior part is like the *glans penis*: if you place the stone upon a table, and observe it at a small distance, it seems not unlike a *glans* cut off, only larger.

I shall only mention, that, a fortnight after the operation, his wife was delivered of a third child. She has the character of a very virtuous woman; but how the *semen in coitu* could pass by this stone, when the urine could not, but with such difficulty, I do not pretend to determine.

A letter from Mr Rob. Lucas, concerning the relief he found in the stone from the use of Alicant soap and lime-water, to his

XII. I have now the satisfaction to acquaint you, that, by God's blessing upon the means His Providence directed me to the use of, I am so far recovered of my distemper (no other than the stone), that I have not the least doubt of being quite free from it in a little time. I am continually voiding stones all broken, white on the outside, without much pain. I can now walk twice as fast as I could 3 weeks since, without uneasiness; nay I rode the other day at once 24 measured miles, trotting most of it, without pain or change of urine; in one word, I can be as certain



certain of the dissolving power of my medicines as I can be without seeing the dissolution.

Dr *Morgan* advised me to drink a pint of lime-water every day. Col. *Morgan* and his lady coming on a visit, advised me to take 4 pills of *Alicant* soap morning and evening; upon which I resolved to add the soap-pills to the use of the lime-water; only, instead of the quantity proposed, I took between 20 and 30 a day, amounting to near an ounce; which I thought I might safely do, well knowing, that Mrs *Stephens's* prescription amounted to almost 3 ounces of soap, besides other ingredients: afterwards I found in an extract published in the magazine, taken from Dr *Whytt's* treatise about dissolving the stone in human bodies, the prescription of the very medicine I used; only a quart or 3 pints of lime-water instead of a pint, upon which I doubled my quantity.

I have since borrowed the treatise myself, and would earnestly recommend the reading of it to every person troubled with that distemper. The experience the Doctor has had of the real effect of this prescription in this distemper, joined with the many experiments I have found of the dissolving power of lime-water and soap, gave me great satisfaction.

I have used with great success stone-lime newly calcined; but by those experiments it should seem, that the dissolving power of lime-water made of oyster-shells is almost double to that of lime-stone. There are two good qualities attending these remedies; the first is, that they are cheap, easily come at, and prepared by one's self. 2dly, That they may be safely used for a long time, without danger to health, I can vouch by my own experience; for a quart of lime-water, and an ounce of soap, has never given me the least nauseating, lowness of spirits, or abatement of appetite, and I think I was never better in health than I am now.

XIII. Parts of a *calculous concretion* formed in the left kidney of *Mary-Anne Mac-Mahon*, otherwise *England*, taken out after her death, in the 30th year of her age.

Fig. 84. A view of the anterior part of the *calculus* in it's proper situation, wanting, to complete it's form, Fig. 86. and 88. and some other small pieces which were joined or adhered to it at *A*.

Fig. 85. A view of the posterior part, completely the reverse of Fig. 84.

Fig. 86. A view of another portion, which, by the intermediation of some smaller pieces, was joined at *B* to Fig. 84. at *A*.

Fig. 87. The reverse of Fig. 86.

Fig. 88. A portion which seemed broke off Fig. 84. at *A*; for it fitted it exactly at *C*.

Fig. 89, 90, 91. Different fragments, whose places could not be certainly determined.

Fig. 92. A *nucleus* of a dark olive-colour, and oval figure, of the common texture and consistence of ordinary *calculi*, discovered by cutting

Fig 88. transversely at *D*.

brother the
Rev. Mr Ri-
chard Lucas,
F. R. S. N^o.
483. p. 463.
Mar. &c.
1747. Read
March 26.
1747.
The figures of
some very ex-
traordinary
calculous con-
cretions form-
ed in the kid-
ney of a wo-
man; com-
municated by
Mr Charles
Lucas at Dub-
lin. Ibid p
465 Present-
ed March 26.
1747.

Fig. 93. A transverse section of *Fig. 85.* at *E*, very solid, white, and semi-pellucid, except at *F*, where a brown vein, of the colour of the surface of the *nucleus*, *Fig. 92.* at *G*, and very porous, runs thro' it.

Extract of a letter from Benj. Heath, to Peter Daval, Esq; Secr. R. S. inclosing a proposal for

XIV. I take the freedom of conveying to you a memoir written by a very ingenious young Surgeon of my acquaintance proposing an improvement in the lateral method of cutting for the stone. His view in putting me on giving you this trouble is, that it may be laid before the *Society*; and if it be thought to deserve it, it may be communicated to the public in their *Transactions*.

intirely removing the only real defect in the lateral operation for the stone; by Mr. John Mudge, Surgeon at Plymouth. N^o. 491. p. 24. Jan. &c. 1749. Read Feb. 2 1748.

A proposal for intirely removing the only real defect in the lateral operation for the stone.

Though the lateral method of cutting for the stone is now almost universally allowed to have greatly the advantage of any other hitherto discovered, yet it must be confessed, that the difficulty and hazard attending the extraction of large stones this way, has really robbed it of it's title to perfection; for though the incision be made to the wish, quite thro' the prostate, and carried on to the neck of the bladder, if this be the case (for it frequently happens to the contrary) as the bladder itself in general is not, nor in all probability can be, wounded in this way of operating, the real aperture after all for the exit of a large stone is so small, that the parts must suffer most violent lacerations, and a train of consequent evils. The old method is greatly more obnoxious to this misfortune, because the parts are torn to pieces by downright violence, without any previous incision of any consequence to prepare them for the egress of the stone; and this imperfection in the operation is so notoriously apparent, and so destructive in fact, that this method is deservedly in disesteem, and almost universally discarded. I wish it could be said, that the lateral method was intirely free from this imperfection; but I fear an impartial inquiry will make it clear, that $\frac{3}{4}$ of the accidents which have attended this operation, may in truth be attributed to excessive distensions and lacerations of the bladder, those few cases, which have miscarried from (what may have been supposed) a mere symptomatic fever, will, I believe, on a strict disquisition, afford a shrewd suspicion, that this very fever itself arose from some violence offered to the bladder, in the forcible extraction of the stone.

I will not enter into a strict examination of those fatal symptoms which sometimes succeed the operation in grown subjects, in order to prove that they in general proceed from the bladder's being too roughly dealt with, because I take the case to be of itself very evident: unless the habit is remarkably bad, to what else shall we attribute violent pain, and the successive inflammation, tumour, suppression of urine, mortification, &c.? These surely are not the attendants on a simple incision only; for constant experience evinces, that the bladder, tho' an organ of great importance, and essentially necessary to the animal oeconomy, may be wounded

Fig. 84.

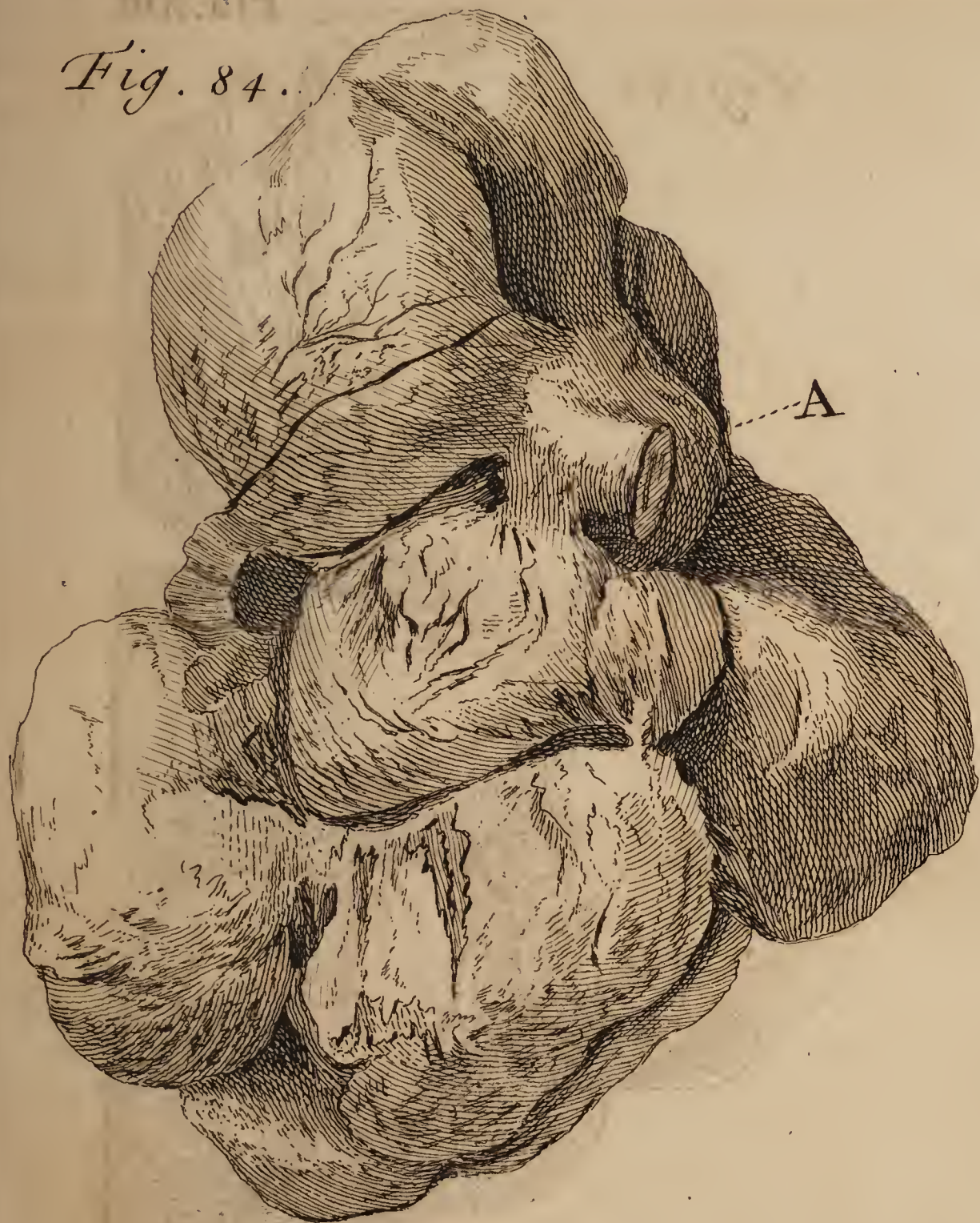


Fig. 85.



Fig. 88.



Fig. 89.

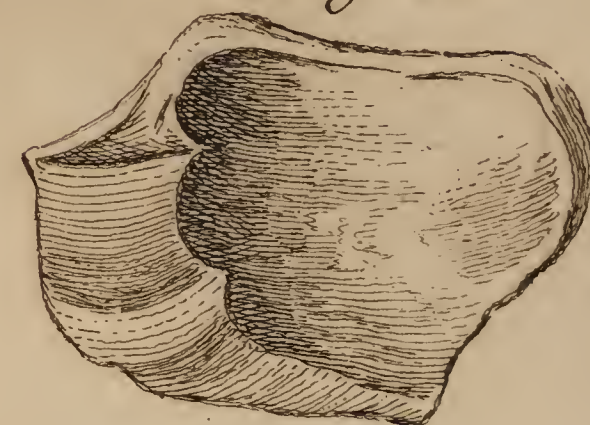


Fig. 86.

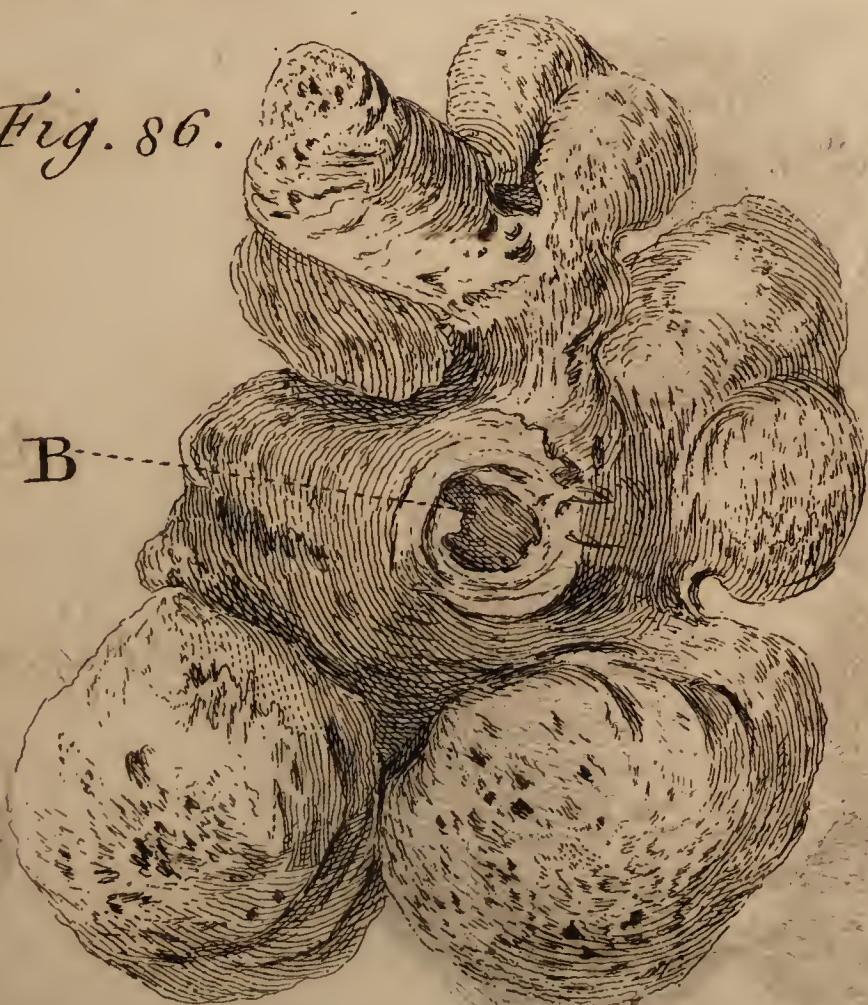


Fig. 87.



Fig. 92.



Fig. 90.



Fig. 93.

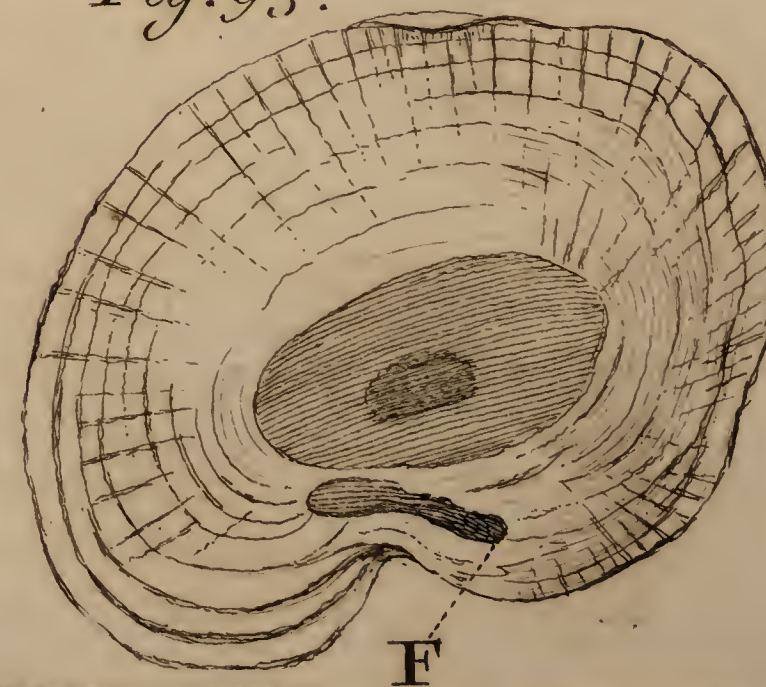


Fig. 91.





wounded with as little danger of any of the above-mentioned evils, as any other membranous part. But I believe we shall not be at a loss for the true cause of all those mischiefs, if the state of the parts in the extraction of a large stone be closely considered.

It may be observed, when a stone is laid hold of by the *forceps*, that both together, stone and *forceps*, from the screw-pin to the former, form a complete wedge; insomuch that a person in a forcible extraction, can scarcely conceive the power applied to the bladder, or the force with which it is distended. If the diameter of the stone be equal to $\frac{1}{3}$ of the length of the chops of the *forceps*, a force of ten pounds applied to them will be to the wound of the bladder, equal to 30; but how shocking must be the case, when (either on account of the magnitude of the stone, or narrowness of the wound) a man uses his utmost force, and many such instances in adult bodies I have seen. The power is then augmented by the action of the lever to 200 or 300; a force no doubt sufficient to reduce the bladder and neighbouring parts to rags. This is scarce to be credited; but there is too much reason to believe, that the want of success in subjects arrived at adult age, where the stones are almost always large, is owing intirely to this very circumstance. When all this violence is insufficient, there is at present no other established method, than either to attempt the making a second incision on the stone, as it is held in the *forceps*, or to withdraw the latter, and to make it on the bladder, in the flaccid state it then lies, without any guide at all. As to the first method, it is evident the *forceps*, stone, and bladder in men are so much in the dark, that the incision must be made with the utmost difficulty; indeed it is hardly possible to cut at all with any certainty. The other way of cutting on the bladder when the *forceps* is withdrawn is much worse; for if it be remembered, that the bladder lieth upon, and is contiguous with the *rectum*, and that they are both in the same flabby state, it will appear impossible to cut the one, without (at least a very great risque of) wounding the other.

This manifest defect in the operation would be intirely removed, if there always was a director for the knife left in the bladder; and this is so easily and completely to be done, that it's great simplicity seems to be the reason it has not been attended to. If one limb of the *forceps*, from the joint to it's extremity, be converted into a staff, by making a deep groove through it's whole length, it will better answer the end desired, than if it were possible to suffer the staff itself to remain in the bladder during the operation; for, when a large stone is laid hold of, and the *forceps* of course much divaricated, insomuch that the extraction of the stone is found to be impracticable with safety, without a second incision on the parts upon the stretch; you then slide the point of a narrow knife on the groove of the *forceps* quite on into the bladder, and it will divide the very part which needs it most; whilst the *rectum*, on account of it's flaccid state, will most certainly escape wounding. If the wound is desired to be but little larger, the *forceps* must be

but gently drawn forward when the incision is made; but if, on the other hand, the stone prove extremely large, the *forceps* should be then drawn forwards with a force sufficient to put the bladder upon the stretch; and by this management the dilatation may be made as great or little as is required.

As the common knife is not so proper for this purpose on account of it's breadth, I have got one made of a more convenient form, with a gorget-handle and button-point, as in the *bubonocèle* knife. This method, as it removes all occasion for violence to the bladder, will reduce the effects of the operation nearly to those of a mere incision only, which might, as now, sometimes prove fistulous, but I believe scarcely ever mortal. I would not be understood to mean that it should be used in common cases, and moderate sized stones; no; Mr *Chefelden* has sufficiently demonstrated to the world, by experiment, the inconvenience of cutting beyond the prostate, when it can be avoided. I only say this, that where a large stone offers, and the circumstances are such, that the operator is under a necessity of tearing the parts to pieces, unless divided by an incision, the latter is more safe and eligible.

The principal advantages of this contrivance are, 1. That the degree of dilatation is in the operator's power, to the greatest exactness: 2. That it will infallibly divide those parts only which are upon the stretch; and for this reason, 3. The *rectum*, if it is empty, and consequently flaccid, must certainly escape wounding. It's great simplicity is another circumstance much in it's favour; for it seems allowed on all hands, the less an operation is incumbered with instruments the better. In common cases, where the assistance of this method is not wanted, the *forceps* is not the least incommoded as to it's general use; but if the stone prove large (which can never be certainly known till it is laid hold of), the remedy is at hand.

P. S. These *forceps* and knife are represented in *Fig. 94, 95, 96.*

Fig. 94.

Fig. 94. The *forceps*, with the knife, *a b*, applied to one of the cheeks of it. *c*, the button. *b f g*, the handle.

Fig. 95.

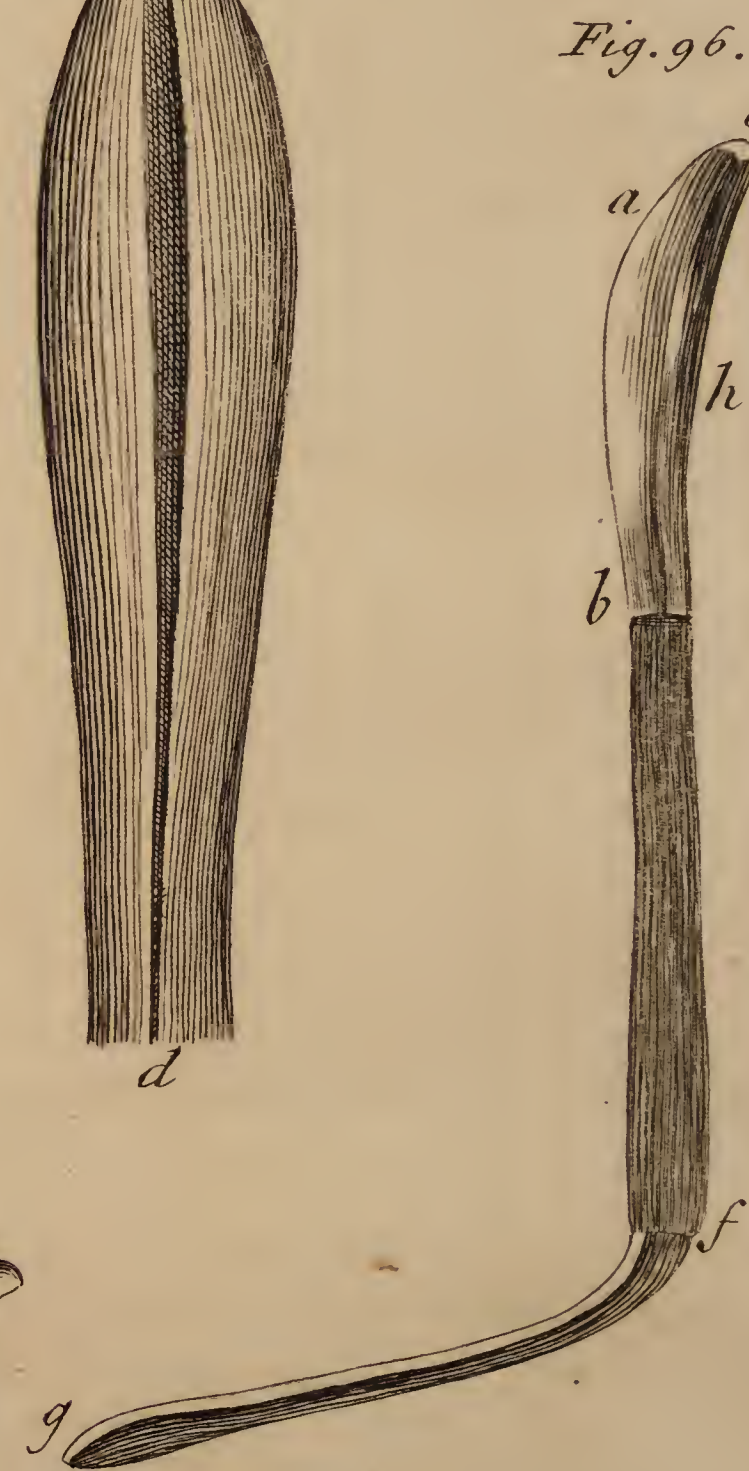
Fig. 95. The cheek of the *forceps* with the groove in it *d e*.

Fig. 96.

Fig. 96. The knife. The blade *a b*; the button *c*, made to fit, and slide along the groove *d e*; *b f g* the handle; *c a b* the sharp edge; *c b f* the back of the knife, which must have the same curve as the back of the cheek of the *forceps* or groove to which it is to be applied, as in *Fig. 94.* C. M.

Concerning a boy, who had a calculus formed between the glans and the præputium; by the Rev. Mr

XV. Upon inquiry, I find that the boy you desire to have an account of, from his infancy laboured under the difficulty of making water; for tho' he was 3 years of age, when put under the care of the person from whom I have this intelligence, yet he could not go alone. He was in the greatest agony upon every motion; but was relieved by putting a great pin, the head foremost, an inch, at least, between the prepuce and the stone; when sometimes a mucous, sometimes a gritty matter,





matter, would first ouze out, and then came the urine with some violence. Rob. Clarke
to Mr Fleming. Ibid.
p. 45. Read
Feb. 23.
1748-9.

During this time, the end of the *penis* was observed to grow in the form of, and as big as, a hen's egg; occasioned, as may be imagined, by the concretion of the stone between the *glans* and the *prepuce*. At last there was a total suppression of urine for 48 hours at least, attended with an appearance of a sore in the side of the *penis*, out of which the stony concretion which I gave you dropped; and from that time, all the while the boy continued under the same person's care, which was about half a year, the urine was discharged at the side, with less and less pain. Last summer he came to see his *quondam* nurse, and told her that he was now perfectly well. His age now is about 23. His name *John Blackhouse*.

XVI. There is preserved in the library of *Trinity-College* in *Cambridge*, a stone taken from a human bladder, which, for its uncommon size, may deserve the notice of this *Society*. It is of an oval shape, flatted on one side and its surface is smooth. The specific gravity plainly shews, that it is of an animal origin; for its weight is to that of water only as 1,75 to 1. An account
of a very
large human
calculus; by
Wm Heber-
den, M. D.
F R. S. and
Fellow of the
Coll. of Phy-
sic. Lond.
Nº. 496. p.
596. Nov.
&c. 1750.
Read Dec. 20.
1750.

In order to get a true and well-attested history of this curious stone, the R. Rev. Dr *Claggett*, late bishop of *Exeter*, was applied to, who was Keeper of the library at the time when it was given, and likewise the Rev. Dr *Baker*, who was then a Fellow of *Trin. Coll.* and entertained the Gentleman that brought it; and lastly, a son-in-law of the very person from whom it was taken. From their accounts it appeared, that this stone was taken from the wife of *Thomas Raisin*, Locksmith in *Bury*, after her death, by Mr *Gutteridge*, a Surgeon, of *Norwich*.

She had felt much less pain than might have been expected from so large a stone; and might probably have lived much longer with it, if she had not thought herself well enough to attempt a journey on horseback; for, while she was riding, she was suddenly seized with violent pains, that obliged her to be taken off the horse immediately: after which she could never make water, unless the stone was first moved, and she continued in great agonies till she died.

This happened in the reign of King *Charles II.* who being then at *Newmarket*, had the stone brought to him; some part of which was chipped off from one of its ends, to shew the King that it consisted of various coats formed one over another, as animal stones usually do.

Mr *Samuel Battely*, who was Member of Parliament for *Bury*, had possession of this stone, either immediately, or very soon after the woman's death, and kept it till it was presented to *Trinity-College*, which was about the middle of Queen *Anne's* reign.

This monstrous stone weighs ℥xxxiiij , ℥iiij , gr. xxxvj , *Troy* weight. There appears to have been at least ℥fs . broken off, on the occasion before related; not to mention what it must have lost by meer wear in 80 years. We

A tumor on the inside of the bladder successfully extirpated.

We are told, that they have in the hospitals of *Paris* human *calculi* weighing 34 *Paris* ounces: but this in *Trinity* library, even at present, weighs 34 *Paris* ounces all but 9 grains; and must have weighed considerably more when it was whole. Yet these are perhaps the heaviest that are any-where recorded; except that very extraordinary one mentioned by Dr *Lister*, in his *Journey to Paris*, p. 232; which he says was taken from a monk *A. D.* 1690. and weighs 51 ounces.

This history may confirm to us the usefulness of endeavouring to relieve the violence of pain in this distemper, by altering the position of the stone in the bladder, either with the help of the *catheter*, or by some proper alteration in the posture of the patient; since, with respect to the pain which it occasions, the situation of the stone appears to be of far greater consequence than it's size.

A shuttle-

spire taken out of the bladder of a boy, in a letter from Mr Wm. Arderon to Mr Baker, F.R.S. N^o. 474. p. 194. June &c. 1744. Read Dec. 6. 1744.

XVII. On the 16th of *September* last, Mr *John Harmer*, a Surgeon in this city, cut one *Peter Riggs*, a boy about 17 years of age, for the stone; at which time there was extracted from him (to the utmost surprize of the spectators) an iron *shuttle spire*, 4 inches long. He had, it is said, some time before, a stoppage of urine; and, by endeavouring with this piece of wire to relieve himself, and thrusting it too far along the urinary passage, he let it drop into the bladder, where it occasioned the same symptoms as a stone would have done. He underwent the operation with great fortitude; and said nothing of this accident until it was all over. He is now perfectly recovered.

The case of a tumor growing on the inside of the bladder, successfully extirpated by Joseph Warner, Surgeon to Guy's Hospital; in a letter to the Pres.

N^o. 495. p. 414. May &c. 1750. Read May 10 1750.

XVIII. An excrescence or tumor arising from the internal coat of the bladder is a disease, though not very common, yet sufficiently known to the curious: but I believe that hitherto, no one has attempted the cure of this disorder by extirpation, nor indeed can it be supposed, that the instances are frequent, where the operation is practicable. But as it is notorious from the history of Physick and Surgery, that several disorders which were formerly unobserved, have been found to occur frequently, after their nature has been once discovered, and exactly described, I flatter myself the publication of this account may possibly throw some light on the present subject.

Mary Bevan, aged 23, on the 24th of *June* 1747, strained herself by endeavouring to lift a great weight; she was immediately seized with violent pain in the small of her back, and a total suppression of urine; which symptoms, notwithstanding the several methods used for her relief, continued till the 29th, when an eminent physician and man-midwife was called to her assistance; who drew off her urine with the catheter. During the suppression, she was seized with an acute fever, and for 18 or 20 hours before her urine was drawn off, she discharged by the mouth a great quantity of saltish water tinged with blood; which, upon lying down, flowed in so great quantities as to threaten suffocation.

In April 1750, she applied to me: upon enquiry I learnt she had never been able, from the moment of the accident, to void a drop of urine without the assistance of the catheter, which had been made use of ever since 2 or 3 times every 24 hours; that she was in continual pain, and had been lately much weakened by having several times lost considerable quantities of blood, occasioned by the force made use of for the introduction of that instrument. Upon examining her with my forefinger, which I introduced with great difficulty through the *meatus urinarius*, I discovered a considerable tumor, which seemed to be of a fleshy substance, and took it's rise from the lower part of the bladder near it's neck; the extent of which I could with difficulty reach. She informed me, she first discovered this swelling about 20 months before. I observed it to protrude a little way out of the *meatus urinarius* upon straining to make water when the bladder was full; but, upon ceasing to strain, it presently returned. It had preserved pretty nearly the same appearance ever since it was first taken notice of; and about 18 months ago, a small incision was made into it, on presumption of it's containing a fluid, but without any effect.

The method I took for the extirpation of the tumor was this: having first prepared her as before the operation for the stone; when her bladder was full, I made her strain as though she was going to make water, upon which I perceived the tumor to protrude a little; this I effectually secured from returning into the bladder by the help of a crooked needle and ligature passed through the tumor in different directions, and endeavoured to draw it out through the *meatus urinarius*, but could not effect it by reason of it's largeness; seeing this, I dilated the *meatus urinarius* on the right side by cutting it upwards about half way towards the neck, when by pulling the tumor forwards, I had sufficient room for tying a ligature round it's basis, which was very broad.

For the 3 first days after the operation, she complained of a good deal of pain in the *abdomen*. On the 6th day, the tumor dropt off. From the first of the operation, she voided her urine without any assistance; and is now perfectly well in every respect.

XIX. *Hannab Hitchcock*, about 60, was from her youth up a very sober industrious woman, of a thin habit of body, and bilious constitution: but in her latter years she became unhealthy, and was frequently subject to fits of the gravel, and brought off some small stones.

For about 3 or 4 years before her death, she was often afflicted with nephritic colics, great pains of the stomach and back, suppression of urine, and sometimes discharged bloody urine. She at length became ascitical and asthmatic; and, for more than 8 or 10 weeks before she died, she had a violent pain in the region of the stomach, and a hard swelling under the *cartilago ensiformis*, with almost perpetual vomitings, colical pains, extreme costiveness, and difficulty of urine. The last fortnight, or more, she vomited every thing, and had a total suppression of urine for 15 days. At last, she died comatose and convulsed.

Upon

One of the
ureters grown
up; by John
Huxham,
M.D. F.R.S.
N^o. 474. p.
208. June &c.
1744. Read
Dec. 13. 1744.

Upon opening the *abdomen*, soon after her death, there neither appeared stomach, liver, or guts, but a large irregular mass, spread from one side of the *abdomen* to the other, and adhering firmly to both sides. This was, in great part, the *omentum*, grown, as it were, cartilaginous, and as tough almost as leather; having up and down in it, several large, very hard, scirrhus nodes, and some tubercles, full of fetid *pus*. The stomach was very much contracted, but its coats were very thick; and, near the *pylorus*, very much inflamed, almost mortified; its glands, in several places, scirrhus, and as large as peas. The passage from the stomach to the guts was shut up, partly by the inflammation and thickness of its coats, and partly by its odd coalition with the liver and *omentum*. The liver was much shrunk and scirrhus, and rolled up into a kind of conical figure; in the *vertex* of which appeared the gall-bladder, of a dark-green colour, and very turgid; and yet the *ductus communis cholecdochus* was near 4 times as large as usual. The *ilium* was thrust down much lower than ordinary; and the convolutions of the gut were, in several places, grown firmly together: the *colon* also, on both sides, was strongly attached to the *peritonæum*. 'Tis almost constantly observed, where the *omentum* is either consumed, or greatly depraved, that the convolutions of the guts grow together, and adhere to the adjoining parts, for want of that oily *mucus*, which, in a natural state, in great plenty transudes from the *omentum*, to lubricate them, and render them fit for the regular performance of the peristaltic motion, &c. In the cavity of the *abdomen* there were near 6 quarts of putrid water, somewhat tinged with blood, in the right cavity of the *thorax* about a pint. The urinary bladder, quite empty, and half rotten, did not contain a drop of urine, but was smeared over with a sort of purulent matter. In the right kidney was found the bigger stone, which took up almost all the *pelvis renalis*. In the right ureter were 2 or 3 small stones, which, with a sort of very tough *mucus*, had shut its passage entirely. Indeed the great stone in the *pelvis* had well nigh quite bunged up the mouth of the ureter. In the left kidney was the smaller stone, which had so entered the ureter with its conical part, as adequately to stop it, like a cork in a bottle. When we had removed this stone (though we found no other in the ureter), we could not force any water thro' it into the bladder, although a strong injecting syringe was used; for the ureter was greatly contracted, and there seemed an absolute coalescence of its sides.

'Tis certain, the canals of a human body, that cease to have any fluid transmitted thro' them, soon coalesce; as is particularly evident in the *canalis arteriosus*, *urachus*, umbilical vessels: nay we see the external coats of the intestines soon grow together, if they are not constantly lubricated with their proper oily *mucus* that prevents it. This will be much sooner effected in the ureters, if they happen to be excoriated by small stones, or gravel: for we find even the fingers, or other excoriated parts, soon grow together, unless prevented by due care. I am
persuaded.

persuaded this is sometimes the case in fatal ischuries ; though more frequently they arise from obstructing gravel, stones, grumous blood, tough mucosity, or the like.

XX. The body of a woman servant, about 30 years of age, who died of a lingering illness, was brought to our theatre Jan. 24, 1743.

On opening the body, I found a great tumour in the *pelvis*, which had given occasion to think she was with child ; but on inquiry, the *uterus* was found in it's place, and two tumours were discovered, with a small space between them, in the upper part of the *pelvis*, adjacent to the *uterus*. The right tube grew to this tumour, so that almost half the membrane composing the tube was lost in the coat of the morbose body. To this tumour, and also to the *uterus* the *omentum* adhered by two *laciniae*, according to the most ancient observation of *Hippocrates*. Several hydatids also adhered to the coat of the morbose bag. There was nothing extraordinary found in the left side.

I cut the tumour, which I easily perceived to be the ovary, and found the covering to be near a line thick. The bag was quite hollow, only it was divided into two cells by a partition. In the smaller *sacculus* was a substance like honey ; but at the same time like fat ; in which was a great quantity of brown, curled hairs, almost 2 inches long, very much resembling human hairs. Having now obtained a true sample of the *melliceres* of the Ancients, I applied the melleous substance to the flame, when it immediately kindled and burned like fat. In the greater *loculus* there was a purulent substance like milk, in which there swam some brown friable substances, like pieces of the lungs.

It does not seem hard to determine, that two *ova*, or two vesicles swelled in this woman, and by degrees occupied the whole capacity of the ovary, destroying the other *vesiculae*, which made little or no resistance. But it seems more difficult to conceive, how so great a quantity of fat is gathered in the ovary, about an ounce perhaps, if I had taken it all out ; for in a sound ovary, there does not appear the least grain of fat. It is manifest therefore, that wheresoever there is a cellulous membrane, fat may sometimes be there deposited, tho' usually there is not any there. Thus I have seen fat in the *scrotum* and *penis* : and I have known it to be found in that tender cellulous fabrick, between the *choroides* and the *sclerotica* of the eye.

It may be inquired, how hairs could grow, without any skin or membrane for them to take root in. I am not ignorant, that other hairs, tho' they perforate the skin, yet have their roots in the fat, as is manifest in the *scrotum* and *pubis*. But it seems a greater difficulty how in a place remote from the *epidermis*, a similar, horny, and pellucid substance should be formed in these morbid hairs ; and whether the common opinion is not hereby weakened, that the crust of hairs is from the cuticle ; and whether it is not demonstrated, that the true bark of a hair is continued from the bark of the bulb. For these hairs do not seem to be

An observation of a steatoma of the ovary, and of hairs found therein ; by Albert Haller, Aulic Counsellor, and chief Phys. to his Majesty, as Elect. of Brunf. Prof Anat. & Bot. at Gottingen, F. R. S. Lond. & Suec. N^o. 472. p. 71. Jan. &c. 1744.

the remains of a destroyed *fœtus*; which has been the case of others, and particularly in that of *Tyson's*: for there teeth were found mixt with the hairs in the ovary. But in our woman nothing remained of the more solid parts of the body, and it is not probable, that so soft a body as hair should alone escape that power, which had destroyed the bones and teeth.

Explanation of the figures. Fig. 97. *A* the *aorta* above the *pelvis*. *B* the *vena cava*. *C* the left *psoas*. *D* the *omentum*. *E* it's adhesion to the *uterus*. *F* another adhesion to the morbid ovary. *G* the *uterus*. *H* the *intestinum rectum*. *I* The left tube. *K* the left ovary. *L* the right tube. *M* it's *lacinae*. *N* the ligament of the right ovary. *O P* the morbid ovary. *O* the greater inferior bag. *P* the smaller bag. *Q Q* hydatids growing to the external surface.

Fig. 98. The morbid bag represented separately. *A* the greater *loculus* open. *B* the morbid thickness of the membrane of the ovary. *C* some remaining part of the purulent matter after the greater part had flowed out. *D* the smaller *loculus*, in which are the melleous fat and hairs. *E* the *septum* separating the *loculi*. *F* the hydatids.

I have deposited the whole tumour with the *uterus*, in the anatomical collection of the university.

An extra-uterine conception; by Starkey Myddelton, M. D. N^o. 475. p. 336. Jan. &c. 1745. Read March 28. 1745.

XXI. Altho' extra-uterine conceptions have frequently appeared from cases of undoubted authority, many of which stand now upon the records of the *R. S.* nevertheless I have thought a doctrine of so extraordinary a nature cannot be too strongly supported, as it is of the highest consequence, as well in establishing the received opinions of conception in general; as in regulating the judgment and practice of those who are more particularly employed in the business of midwifry.

Oct. 28. last, I was sent for to a woman of about 42 years of age. I was told by the patient, that she had been taken with a flooding the day before; which a little surprized her, as having been very irregular in her menstrual discharges for near a year before. At the same time she complained of a great pain in her belly and loins, with a continual forcing both forward and backward; which still continued, though her flooding was then in a manner stopped.

I ordered her a gentle paregoric for that night, and the next day I found her in great pain; at which time she told me, she had some reason to believe she had conceived with child. I then examined her, and found the *os tincæ* intirely close. I was not very curious in my examination at this time; taking it for granted, from what she had told me, that nature would, in a little time, dispose the *uterus* to discharge it's contents, tho' at present there was not the least appearance of it. I ordered her an anodyne clyster, and a paregoric to be taken after it; and the next day I found her pains continued, which now appeared like a *tenesmus*; tho' so violent, as to prevent her rest all that night. I then ordered her a repetition of the clyster and paregoric; and the next day

(finding

Fig. 98.



Fig. 97.

J. Mynde sc.

A Scale of Inches.

(finding her in great pain, and still without any rest, and beginning to be a little feverish) I ordered ʒviij of blood to be taken away, and continued the paregoric, which I likewise ordered her to repeat as she found occasion; from the use of which she sometimes rested tolerably well; but as the power of the opiate went off, her pains always returned.

Several days having now passed without any alteration, I again examined her, and found the *os tincæ* still as close as ever; but, on a stricter inquiry, I felt something, which seemed to me to be the head of a child fluctuating in it's membranes, I told her my thoughts of her case, and that it was not in my power to help her; but that nature must take it's course, or at least point out a method how to act. She seemed very much surprized at my opinion, and asked me, If ever I had met with such a case before? I told her, I had been engaged in the practice of midwifry upwards of 20 years; in all which time I had never met with a case of the like nature: being positive I felt the head of a child, but could not absolutely determine whether it was in the *uterus* or *extra uterum*. I then told her, I would desire the favour of Dr *Bamber* to give her a visit; which I did; and the next day we went together; when, on examination, he confirmed what I had before asserted; but seemed more inclinable to believe the child (the head of which he felt) was *extra uterum*. Indeed he proceeded in his inquiry at that time further than I had done before, having passed his finger into the *Anus*, where, he said, he could distinguish the head more plainly. We then both left her, after having ordered her to repeat the paregoric, when in more pain than ordinary, and once in 2 or 3 days to take a gentle lenitive purge to keep her body soluble; because the continual use of opiates would naturally tie her up.

In this manner she went on for about 3 weeks longer; when I waited on Dr *Nichols*, and desired the same favour of him as I had before asked of Dr *Bamber*, that I might have his opinion also of a case which appeared so very singular. The next day we went together; and when we came, I desired he would examine her, which he did; and after having heard all her complaints, said, He was of opinion, that there were some abscesses forming in (or in contact with) the *uterus*, which, very likely, in a little time, would break and discharge itself; but as, at that time, nothing of a child could be perceived by the touch, so he was obliged to submit that to the credit of my opinion, who had before frequently felt it.

Thus she continued for about a fortnight after this visit; when, calling on her one day, she told me, she was much easier than she had been; and that something came constantly draining away by the *anus*, of a very offensive smell, which, upon examination, appeared to be true pus. I now began to think Dr *Nichols*'s opinion of her case the most eligible, and the rather, as it was not inconsistent with my own sentiments, that there had been a child; which, being now dead, might have given occasion for the forming such an abscess. In this state of

violent pain she continued to the time of her death, which happened Jan. 28. being 13 weeks from the first of her illness; when, by her particular desire, I opened her, in the presence of Dr Bamber, Dr Nichols, Dr Eaton, and Mr Jones Surgeon, &c.

After having divided the integuments of the *abdomen*, every thing, at first view, appeared in a healthy state. On turning aside the intestines, I found the *uterus* sound and perfect, and of a size common to women who have had children; but, in the place of the right *fallopian* tube, there appeared a large tumour, formed by the expansion of the tube extending itself from the *os ilium* towards the extremity of the *sacrum*. Upon opening it, we discovered a mass of fetid *pus*, in which the bones of a *fœtus*, of about 5 or 6 months old, were buried. These bones were, for the most part, wholly divested of their flesh; so that the edges of the thin bones must, of necessity, cut and irritate from every motion of the body. The *pus* had made it's way thro' the *rectum*, in which there was a small passage a little above the *sphincter*.

Upon examining the bones, after having washed them in water, a new matter of surprize appeared: the inferior jaw was consolidated with the *os temporis* and superior *maxilla*; and six of the ribs, with their correspondent *vertebræ*, were united into one bone.

May we not attribute these *anchyloses* to the want of motion in the *fœtus*, which was here prevented, by it's being closely pent up in so unnatural a situation? if this be the case, we see great advantages arising from the frequent motions of the *fœtus* in the *uterus*; and that Providence has not subjected the tender part of our species to such constant disturbances without great necessity.

Fig. 99.

Fig. 99. *A*, the *uterus*. *B*, the *os tincæ*. *C*, the *vagina*. *D*, the left *fallopian* tube. *E*, the beginning of the right *fallopian* tube in it's natural state. *F*, the *sacculus*, formed by the expansion of the right *fallopian* tube, in which the *fœtus* was contained. *G*, the *ligamentum rotundum* on the left side.

An abstract
of the remark-
able case and
cure of a wo-
man, from
whom a fœtus
was extract-
ed, that had
been lodged 13
years in one of
the fallopian
tubes; sent
from Riga by
Dr James
Mounsey,
Phys. to the
army of the

XXII. The particulars of this extraordinary case are set forth at large in * Dr Mounsey's paper, the substance whereof is as follows:

A Soldier's wife, of *Abo* in *Finland*, of a middling stature, and who had been the mother of two children, being pregnant a third time in 1730, was afflicted with violent pains and twistings in the bowels, swooning fits, vomitings, and great disorders in her back and lower belly. These symptoms, and complaints of several other kinds, continued to make her uneasy, till she found her burden increase, which fell from side to side as she changed the posture of her body, and could be pressed by her hand from one place to another, but rather chose to remain on the right side. After quickening her health became better,

* It is likewise printed in the *Swedish* language, in the acts of the *Royal Academy* at *Stockholm*.

she

she grew bigger bellied than ordinary, and was supposed to carry twins. About the time her delivery was expected, she was taken ill with violent pains by fits across her lower belly; but had none in her back, nor any forcings downwards; and next day these pains went off, which made her suppose she had misreckoned. But after this her breasts swelled, and gave milk in plenty; and her menses came on, attended with more violent pains than she had had before; and such large discharges of blood from time to time, that she could neither speak nor move; and even after the floodings were stopped, her blood and strength seemed quite exhausted: she often fainted away, and was sometimes thought to be dead. She continued very sickly for 10 years afterwards; during which time her burden was moveable, and fell from side to side. But in *Sept.* 1741, she felt a pain beneath the navel, with a swelling and redness, which in about 3 weeks appeared like a small boil. This she pierced with an awl, and a yellow-coloured water ran from it without any smell, and continued so to do for near 3 weeks more, when it discharged a purulent stinking matter. In *June* 2 small bones came out, which were given to the Surgeon that visited her; who only applied a piece of plaster, persuading her that a cure was impossible. Other small bones worked themselves out afterwards, till *Oct.* 1742, when the headquarters of the *Russian* army being at *Abo*, this unhappy woman applied to Dr *Mounsey*, who, after a careful examination, undertook to deliver her. And accordingly, desiring the assistance of Mr *Geitle*, Surgeon to the regiment of *Abo*, a grooved probe was thrust into the *fistula*, and an incision made with a bistory; upwards and obliquely, from the *linea alba* into the cavity of the *abdomen*; but she being unruly, and the operation not going on to the Doctor's liking, he proceeded no farther till the next day; only some loose bones were extracted, and the wound dressed with tents and compresses, to hold in the *omentum*, and keep the wound open. At the next operation the incision was carried downwards, and then another incision continued from the first was conducted upwards, and slanting at a small distance from the first; taking care to keep as near as possible the direction where the adhesion of this body to the *peritonæum* appeared to be, and avoiding to make the external wound larger than absolutely needful, lest the *omentum* and guts should fall out; and particularly lest the suppuration should exceed the strength of nature, which was here already nearly exhausted. A large opening was now made, but the *fœtus* being closely enveloped by it's containing sack, the Doctor durst not venture to draw it out by force, for fear some of the naked bones might lacerate the internal parts: wherefore, dilating this sack with the points of a pair of probe-scissars, directed by the fingers of the left hand, he pierced and cut in pieces the skull, which afterwards he extracted piece-meal. The matter that first issued out had a very nauseous smell, and consisted of membranes, fat, and corrupted flesh. On opening the *cranium*, the *cerebrum* appeared of it's natural colour. The operation having been long, and the woman fainting away,

Czarina, with the bones of the said fœtus, as a present to the R. S. and delivered in his name; by H. Baker, F. R. S. No. 486 p. 131. Feb. & Mar. 1748. The original read Feb. 4. 1747-8.

away, the wound was dressed, without attempting to extract more at that time. In the evening she was taken with vomitings; but by proper internal medicines, and flannel stoups wrung out of hot wine, applied over the whole belly, and often renewed, she found ease, and grew better. The loss of blood, during the whole operation, was inconsiderable.

Next day the bones of the trunk, and most of the other large ones, with their ligaments and rotten flesh, were taken out. The matter discharged for several days was of a dark-brown colour, occasioned by blood issuing from the dilated pores of the internal surface of the sack, which rendered the matter at first of a deep-red colour; but that changed daily and gradually, till at last it became white. The Doctor imagines this discharge to resemble the *lochia* after child-birth; for after the *fœtus* was extracted, the woman's breasts swelled, and gave milk in plenty for 2 months, in quantity, colour, and consistence, as if after a delivery at the proper time. The Doctor examined this sack very diligently with his fingers, whilst thin, and not contracted, but formed into wrinkles, through which he felt the *rectum*, the *vesica urinaria*, and, as he thought, the *fundus uteri*. Many small bones lay in the folds; but, as she complained not of pain, they were left till the suppuration began, excepting those the sack in contracting itself threw out. Besides fomentations, balsamics, proper bandages, &c. vulnerary detergent injections were found very useful, thrown in in large quantities, both to wash out the putrid flesh, and bring away the scales of bones which were still concealed in the folds of this sack, which contracted itself daily, grew smooth and white within, and narrower as it approached the *uterus*, which gave reason to believe it one of the *fallopian* tubes. The wound was cured in about six weeks, and the woman delivered from a long state of misery grew fat and lusty, and now enjoys good health.

Comparing these circumstances together, it seems reasonable to believe this fruit never was in the cavity of the womb, but that the impregnated *ovum* was stopt in its passage through one of the *fallopian* tubes, where it grew, and was detained so many years; and that the inflammation, which happened below the navel, was not owing to the rottenness of the *fœtus*, or to its bare bones seeking a discharge, but rather to some accidental friction of the containing sack against the *peritonæum*, thereby producing adhesion, obstruction, inflammation, &c. The *fœtus*, before this accident, must have remained all these years intire, and without perfect corruption: for it took no less time, after its communication with the common air, before it shewed marks of putrefaction, than a fresh subject, kept in the same degree of heat, would have done.

The Doctor's observations of the bones are: that they have a full proportion to those of a child at 9 months, and that the fibres are more compact, and their articulations stronger. The sockets for the teeth are six on each side of the jaws; the *dentes incisores* of the upper jaw
are

are high and large; the *molars* have almost all begun to ossify in their *alveoli*; at least the crown, which is the cortical part, is formed, and they are filled internally with a cretaceous substance. In new-born children those parts are seldom found so far advanced, which gives reason to believe this child did not die within the ordinary time of pregnancy, and that the different accidents, before-mentioned to have happened, were chiefly owing to the preternatural situation of the *fœtus*. Some places in the skull appear to have been carious, and corroded by some sharp humours; and nature, supplying it's ossifying juices, has repaired these places, and rendered them more solid and white than the rest, but very uneven and scabrous, from the different times and directions of the bony sproutings. There are likewise *exostoses* on the ends of the thigh-bone, and some other bones. It is very difficult to determine about what time the growth of these productions began or ended. Supposing it from the time of the disorders that happened in the first months of pregnancy; would not such a disease have caused death to the *fœtus*, before it had come thus to a full growth? if it was the consequence of the violent accidents which happened about the time of the natural birth, the child then must have continued alive some considerable time afterwards, during which these bony excrescences were formed; there being a perfect ossification, as performed by the laws of circulation, and not by any vegetative or petrifying power, as in inanimate bodies. Two or three of the lateral processes of the spine were what first passed thro' the little ulcer; the rest of the bones (except a few that were lost in cleaning) were presented by the Doctor to the Museum of the *Royal Society*. They retain a very strong and singular smell, though they were immediately cleansed from the rotten flesh, and well washed. The woman came by sea to *Stockholm* above a year after this cure, and was presented to the *Academy* in good health; and the Doctor believes she is still alive and well.

XXIII. 1. There are several instances of the bones of *fœtus's*, which have died in their mothers bellies, making their way out by preternatural manners; some by the navel, some by the groin, and some by the anus. Of this last sort I am now going to give another instance, which happened in *New London* in *New England*, in 1737. A negro wench was thought to have conceived with child; and about 3 months after, she had some appearances of a miscarriage, but no *fœtus* was observed to come away. This therefore made the good women now alter their opinion; thinking that she was not with child, but only had not been regular from having taken cold: whereupon remedies, proper in such a case, were given her; but she found no relief from exceeding great pains she complained of in the bottom of her belly, and in the small of her back, more particularly when she went to stool. Her flesh wasting extremely, a skilful woman was sent for, who found milk in her breasts, and other certain tokens of her being with child. She continued wasting

An account of the bones of a fœtus coming away by the anus; communicated by John Still Winthrop, Esq; N^o. 475. p. 304. Jan. &c. 1745. Read Mar. 14. 1744-5.

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ing in a miserable condition, growing less in her belly, and her breasts falling, and was at last given over: but at length, at the end of about 8 months, she brought away much blood by stool, on which her pain in those parts abated; and then she voided with her stools these bones with flesh and rotten skin about them. After this she soon grew well, and recovered intirely. All the parts of the *fœtus* were found in her stools, except the head; which is supposed to have come away by the *vagina*, when she had the symptoms of miscarrying abovementioned; for it was now recollected, that she then said, Something came away with her water as big as a large nut, but it was not then attended to.

— in a letter from Mr James Simon to the Pref. N^o. 477. p. 529. Aug. &c. 1745. Read Nov. 14. 1745.

2. A curious and worthy clergyman, of the county of *Armagh*, sent me, some time ago, a parcel of bones, with the following account of them; *viz.*

“ *Rose*, the wife of *Mortough Mac Cornwall*, of the parish of *Tullylish*, barony of *Clare*, in 1741, about the latter end of *May*, or beginning of *June*, being in the 37th year of her age, and mother of several children, conceived, as usual; but, in 2 or 3 days after, felt an excessive unnatural kind of pain in the *matrix*, which continued, with frequent faintings, a depraved appetite, and an exceeding great weakness, till her child quickned; after which she proceeded reasonably well in her pregnancy to the end of 9 months; and then, her child alive, and every thing right (as the midwife thought,) she fell in labour, which lasted, with proper child-bearing pains, for 24 hours, but could not be delivered; and her labour leaving her, the child was no more observed to stir. In a month after, her labour returned, and, with many regular throws, continued 24 hours more, but to no purpose, save the discharging of some quantities of black corrupted clods of blood; of which kind also she threw up much by vomit: then her labour left her intirely; and soon after she felt the decaying of the flesh of her infant, and the discharge thereof both by the *matrix* and *anus*, with so putrid and deadly a smell as was extremely nauseous both to herself and others about her. Thus she lived for upwards of 12 months, and, at that period her pains increasing to excess, she began the discharging of the bones, which, to the number of 80 and upwards, she voided wholly by siege; 14 the first day, and 2, 3, or 4, at a time afterwards for the space of twelve months or more, with most intolerable pains at the voiding of each bone, especially a broad piece of the skull: so that, from her conception to the day of her death, which was the 4th of *April* last, makes up near 4 years; during most of which time, never was a more calamitous creature: for 3 years, scarce a day without suffering most exquisite torture, being also attended with frequent faintings, a continual want of appetite, and an almost perpetual looseness; inso-

“ much that it is miraculous how she lived, not eating all that long space so much as would have sustained a sucking infant; even the very liquids, at length, not lying a moment in her stomach; by which

“ means

“ means she became quite emaciated, and dismal to look at, not being
 “ able to move from one posture to another, or to be moved, without
 “ fainting at every the least touch or motion. The truth of all which
 “ I attest to you, as I received it partly from the poor woman her-
 “ self, and also from my wife, who visited her frequently during her
 “ illness.”

XXIV. In *April* 1731. Mrs *Ball*, without *Bishopsgate*, perceived (by the usual symptoms) that she was pregnant; and in *Oct.* following, being then in the sixth month of her pregnancy, she had a child died in her lap of convulsions, the surprize of which occasioned a great fluttering within her, attended with a sensible motion of the child, which motion continued, though gradually weaker and weaker, for about 6 or 7 days, after which she did not perceive it to move any more; but from this time she had constant pains attending her, which appeared like labour-pains. Her midwife, for several days, expected a miscarriage; but finding herself disappointed, advised her to apply to Dr *Bamber*.

The Doctor (after a proper examination) finding sufficient indications of a dead child, ordered her some forcing medicines; upon taking which about three times she discharged something, which the women supposed to be part of an after-birth, accompanied with a small quantity of water: in consequence of this discharge her pains ceased, but without any visible diminution of her belly.

After some time she again applied herself to the Doctor, who thought it most adviseable to discontinue her medicines, and leave the affair entirely to nature. In this state she continued for about 20 months, viz. to *July* 1733. which was 2 years and 3 months from her first reckoning; she then again applied herself to Dr *Bamber*, acquainting him, that she was not yet delivered of the child she so long since came to consult him about, and that her pains were lately returned, and daily increased without any intermission, upon the Doctor's examining her, he thought it proper to send her home immediately, directing her to promote her pains, by frequently supping some warm caudle, &c. by the use of which her pains became more regular, and the next day the doctor made her a visit, and was informed she had discharged 2 waters, but nothing more: he then carefully examined her again, and plainly felt a child through the integuments of the *abdomen*, but could not give her any assistance.

It was about this time that Dr *Bamber* first acquainted me with the case, and desired that I would attend her as often as occasion might require, and that I would acquaint him, if any thing like labour, or other remarkable alteration should offer. Accordingly I made her a visit, and after a proper examination, was convinced of the certainty of the Doctor's assertion. Her pains now began to abate, and she grew tolerably easy; but about the latter end of *Jan.* 1733-4. she conceived again with child, and was delivered on *Oct.* 28. following by Dr *Bamber*, who

An account of a child being taken out of the abdomen, after having lain there upwards of 16 years, during which time the woman had 4 children, all born alive; by Starkey Myddleton, M. D. N^o. 484 p. 617. Oct. &c. 1747. Read Dec. 17. 1747.

sent for me to attend him in her labour: the Doctor soon delivered her of a fine boy, and after having brought away the *placenta*, he searched for the other child, which he had before felt through the integuments of the *abdomen*, but found it was lodged in the cavity of the *abdomen*, and beyond the reach of human art to relieve her. This fact every one then present was made sensible of.

Oct. 22. 1735. I was sent for to her in her labour, but before my arrival she was delivered of a boy; but I brought away the *placenta*, which gave me an opportunity of examining for the other child, and found it in the same situation as formerly.

October 9. 1738. I was again sent for to her, when in labour, but she was delivered of a boy before I arrived. Upon examining the womb, and the state of the *abdomen*, the child appeared just as before, without any alteration.

June 17. 1741. I was again sent for in her labour, but found her just delivered of a girl; and upon examining the parts, every thing appeared as before.

Oct. 14. 1747. being greatly emaciated by constant pains, &c. she was admitted a patient in *Guy's Hospital*, where she died Nov. 7. following, after having laboured under the distresses and uneasiness of carrying a dead child within her, in a manner loose, in the cavity of the *abdomen* upwards of 16 years.

The day after her death I opened her, in the presence of Dr *Nesbit*, Dr *Nichols*, and Dr *Lawrence*, when the *uterus*, and the several other contents of the *abdomen* appeared (nearly) in their natural state, but on the right side within the *os ilium* a child presented itself, which was attached to the *ilium* and neighbouring membranes by a portion of the *peritonæum*, in which the *fimbria* and part of the right *fallopian* tube seemed to lose itself.

The child seemed no-ways putrid; but the integuments were become so callous, and changed from their natural state, that the whole seemed to resemble a cartilaginous mass, without form or distinction: the legs indeed were distinguishable, though they were much wasted and distorted. Upon opening the callous integuments of the head and face of the child, the bones appeared perfectly formed, with a few spots of tophous concretions on them.

This account may serve to convince those who are of opinion that boys are conceived on the right side, and the girls on the left; as this woman had three boys, and one girl, after the *fallopian* tube on the right side had lost it's action.

Concerning
the bones of a
fœtus being
discharged
thro' an ulcer
near the na-

XXV. *Jane*, the wife of *James Burman*, Labourer, at *Scawby* near *Brigg* in *Lincolnshire*, was about 29 years of age when she married. About two years after, when she had had a child at full time, she conceived again, and went regularly on for 4 months. She then got a fall, and about 3 weeks after felt a load in her belly, which continued, on the right side, for

for between 2 and 3 years. The woman then grew very big with another child, which pressed so much upon the lump as to give her great uneasiness. However, she went on to her time with her double burden; and 3 $\frac{1}{4}$ years after the accidental fall, she was delivered of a live child at full growth: from which time she grew worse and worse, with violent pain about the navel, and an inflamed tumour appeared near that part. Upon application to a neighbouring Surgeon, fomentations were used, which produced a suppuration at a small breach near the navel. The Surgeon did not know what to make of this swelling, and therefore did not venture to enlarge the orifice; but it continued discharging a fetid purulent matter for 3 or 4 months longer.

vel; in a letter from Mr Francis Drake, Surgeon, F. R. S. to M. Folkes, Esq; P. R. S. &c. N^o. 485. p. 121 Jan 1747-8 Read Jan. 28. 1747.

About a year, or more, after her last delivery, the woman was suddenly seized in the night-time, and a hardish mass of flesh, seemingly about 8 inches long, was discharged thro' the old opening in her belly. The lump was rather thicker than an ordinary man's wrist, and which, being opened, contained all the bones of a *fœtus*, of about 4 months growth. At this time the woman was much emaciated, occasioned by the large discharge of *pus* from the wound; and, what was much more extraordinary, whatever she eat or drank came half-digested thro' the opening. White bread, or better diet, came thro' in that manner; but coarse rye-bread, or such-like, was not digested at all. For which reason, the poor woman must have inevitably perished, had she not been supported by a charitable Gentleman's family in the village, with diet fit for her miserable circumstances. She continued to discharge her excrement in this manner for 6 months, and then that symptom left her; after which the ulcer was kept open other 6 months, when it dried up of itself naturally, with a very firm but small cicatrix.

I had the curiosity to see this woman, and Mr *Charlesworth*, Surgeon and Apothecary at *Brigg*, sent for her. She appeared hale, strong, and in full health. I had the above account of her case from her own mouth, attested by the Surgeon who attended her. I saw the bones of the *fœtus* in Mr *Charlesworth*'s possession, perfectly white, and, I believe, not one wanting. The woman further told me, that, 9 months after the wound was healed, she was delivered of another live child, at full time, but with great difficulty. The whole time that the bones of the *fœtus* may be supposed to have lain in the woman's belly was about 4 $\frac{1}{2}$ years.

XXVI. The following case was lately communicated to me by Mr *Alex. Wills*, an experienced and ingenious Surgeon and Man-midwife, of *King'sbridge*. It seems to have something particular and remarkable. If you think so, on perusal, you may be so good as to lay it before the R. S.

A child born with an extraordinary tumour near the anus, containing some rudiments of an embryo in it; in a letter from John

“ *John Perrine*'s wife, of *Charleton* parish, a brisk active young woman (he very infirm and consumptive) was delivered of a daughter at full time, *July 11. 1746*. The child was perfect, as to all it's

“ limbs,

Huxham, M. D. F. R. S. to C. Mortimer. Sec. R. S. N^o. 487 p. 325. Apr. &c 1748. Read May 19. 1748.

“ limbs, head, body, &c. but from the region over the *os sacrum*,
 “ *glutæi* muscles, and between the thighs, quite home to the *pudendum*,
 “ was growing a very large substance, which the midwife and others
 “ called a wen, in shape very like the ventricle of a sheep, and seemed,
 “ as to it's colour and outward appearance, a continuation of the same
 “ skin with the rest of the body, but very full of blood-vessels. It hung
 “ down behind below the heels, and was bigger than the whole body
 “ of the child itself. It felt very soft, and seemed to have matter fluctuate in it; but in the middle of the whole was evidently felt a hard substance.
 “ The *pudendum* as well as *anus* were in all respects natural, and both
 “ urine and stool were regularly discharged; but the *anus* was placed
 “ much more forward, and immediately under the *pudendum*; so that
 “ the *feces* were discharged in the same direction with that of the urine.
 “ I made a puncture in the depending part of the tumour, and drew
 “ off near 2 quarts of a palish red water, without any smell. The orifice being left open, there was a continual issue of the same kind of
 “ water for several days; but by degrees it became more and more glutinous, and at length whitish like *pus*, and very fetid. As the discharge was great, the child grew weaker and weaker, and at the end
 “ of 15 days died.
 “ The next day I opened the tumour, and found, near the *os coccygis*, an abscess within a cystis, in which were 3iv at least of white
 “ *pus* prodigiously stinking; and, on further examination, found several cartilaginous joints, as it were, somewhat resembling the tail of a
 “ sheep, continued from the point of the *os coccygis*. These were about
 “ 2 inches long, and enveloped with a kind of fleshy substance covered
 “ with a sort of fat: these, when cut thro', appeared exactly like the
 “ inner part of lamb-stones. From those depended a substance like the
 “ head and neck of an embryo, as big as a large egg, which, on opening, contained somewhat resembling brain, and a kind of a *cerebellum*
 “ in the back part: it had a mouth and tongue on one side of the face
 “ (if it might be so called) but no appearance of eyes or nose; however
 “ there was an ear pretty evident. In the large tumour there hung a
 “ kind of loose membrane, which perhaps might be part of a secundine.”

Concerning a polypus at the heart, and a scirrhus tumour of the uterus; in a letter from Peter Templeman, M. D. to Wm. Battie, M. D. F. R.

XXVII. *Ann Hicks* was brought to the workhouse of *St Andrew's Holborn* on *Saturday Nov. 15, 1746*. Her complaints were, a difficulty of breathing, from a cold she had caught about a fortnight before, with a violent pain and palpitation of the heart. The pulse was scarce perceptible. The Surgeon, *Mr Tait*, being present, I ordered him to open a vein; put to keep his finger on the pulse; and if it did not rise on her losing a little blood, immediately to desist. Upon her losing an ounce or two of blood, the pulse grew more languid, and he accordingly desisted. I then ordered a large blister to be applied

applied to her neck, and gave her oily medicines with the volatile salts. Coll. Phys. Lond, and
I did not visit at the workhouse again till the *Wednesday* following, when *F. R. S. N^o.*
I found her much easier in her breath, but the pain and palpitation of *481. p. 285.*
the heart continuing. As the oily medicines had occasioned a violent *Oct. &c 1746.*
purgings, I ordered her the *Elixir asthmaticum* in cinnamon-water. Her *Read Nov. 27.*
pulse was still so little discernible, that though I think it intermitted, *1746.*
yet I cannot be positive.

Upon my telling you on *Thursday* my apprehensions of a *polypus* at her heart, from the pain and palpitation of it, you desired, that, when she died, her body might be opened. To you therefore are we indebted for whatever curious was discovered. She died on *Friday*, and her body was opened on *Saturday*. Upon exposing the body naked on the table, we were surprized with the sight of a very large and hard swelling in the hypogastric region; concluding it at the first view to be a child; and the more so, because the woman had never made any complaint of an uneasiness in those parts. Having opened the body, we found this swelling to be of the *uterus*, which was greatly enlarged, and extremely hard. Besides the whole body of the *uterus* being thus enlarged and hardened, there were two large protuberances distinct from each other, that grew prominent out of the upper surface of the *uterus*, and were each of them of the size of a large egg. There was likewise a third protuberance on the opposite side, but much smaller than the other two; and another that seemed to be but just budding. We cut down directly through one of the large protuberances into the very body of the *uterus*, and found nothing but a solid mass of a cartilaginous substance. The texture indeed of the protuberance was somewhat laxer than of the body of the *uterus*. Having cut down very deep into the *uterus*, and found nothing but this solid mass, we introduced a probe from the *ostium*, to examine if there was any cavity in the *uterus*; and found a small one reaching to the *fundus*, and barely large enough to admit the probe. The *ovaries* and *fallopian* tubes were in their natural state; except a small deviation of the *fallopian* tube on the right side.

I know no writer that has taken notice of a similar appearance in the *uterus*, but *Ruysch*; and his words I beg leave to cite. They are in his *Thesaurus Anat. Decimus*, N^o. CVI. “*Uterus humanus scirrhus factus,*
“*Et in majorem molem extensus, id quod in vetulis aliquoties observavi;*
“*præterea ex superficie superiore alter Et exiguus scirrhus emergit, ut Et*
“*ovaria, ovorum ductus (tubæ fallopianæ dicti) satis bene disposita; Et*
“*hæc omnia in liquore.*” He has not given any plate or description of the dimensions of his enlarged *uterus*; but this, which I have been describing, is, in it’s greatest breadth, $4\frac{3}{4}$ inches; it’s length from the *ostium* 6 inches; thickness $3\frac{1}{2}$ inches; and it’s weight, including the *ovaries*, *fallopian* tubes, &c. two pounds twelve ounces of *avoirdupoise* weight. Whatever *Ruysch* had observed of that kind were in old women. This woman I take to have been between 30 and 40. He does not mention having seen more than one small additional *scirrhus*;
whereas

whereas in ours there were 3 or 4. And, lastly, he does not mention any thing of the hard cartilaginous substance of the *uterus*.

There were several remarkable adhesions in the *abdomen* and *thorax*; as of the *omentum* to the *peritonæum*, of the lungs to the *pleura* and diaphragm, of the *pericardium* to the *pleura*. The liver and spleen appeared in their natural state: the kidneys were enlarged beyond their usual size: the coronary veins of the heart were much distended with blood, and the lungs inflamed to a degree of mortification. Upon examining the cavities of the heart, we found in the right ventricle a polypose concretion of a fleshy fibrous substance that adhered to the ventricle, and, in separating it from thence, was rent into two pieces.

The figures
drawn by
Dr Parsons.
Fig 100.

Fig. 100. represents the back part of the *uterus* with it's scirrhus tumours. *A* is the *uterus*. *B*, the great scirrhus. *C*, one of the great protuberances. *D*, the *cervix uteri*. *E*, part of the *vagina*. *FF*, the *ovaries*. *GG*, the extremities of the *fallopian* tubes. *H*, one of the muscles called *Ligamenta rotunda*. *II*, spermatic vessels. *K*, the edge of the *Alæ vespertilionis* on the left side. *LL*, two small protuberances.

Fig. 101.

Fig. 101. represents the fore part of the same *uterus* laid open, by cutting through the middle of the scirrhus. *A*, the cavity of the *uterus*. *BB*, the divided surfaces of the great scirrhus. *C*, the fore part of one of the great protuberances. *D*, the *cervix uteri* laid open. *E*, the *vagina* laid open. *F*, the divided surface of the protuberance that was cut through.

The extirpation of an excrescence from the womb; in a letter from John Burton, M. D. to C. Mortimer, M. D. & Sec. R. S. N^o. 495. p 520. May &c. 1750. Read July 5. 1750.

XXVIII. The wife of one *Chapman*, a Whitesmith, at *Selby*, 10 miles off, upwards of 7 years ago lay in of her last child, and had a tolerable easy labour: soon after which, she had what she called the *fluor albus*, that continued ever since, and increased upon her; insomuch that she says, she has sometimes had such a discharge, as to wet the place she sat upon thro' all her petticoats, &c. For some months before we were concerned for her, she began to complain of a pain and weight in the *uterus*; which increased as the substance grew in bulk; and at last the excrescence was so large as to appear outwards, and then it grew very fast. The patient consulted her midwife, who thought the womb had come out; but was so prudent as not to do any thing; and desired they would call in better advice. Accordingly they sent for Mr *Fell*, an eminent man-midwife and Surgeon in this city*; who, not having met with a case like that, desired me also to go and see her; which was in *Dec.* (1749). The substance not only filled, but extended, the entrance into the *vagina*. I introduced a finger into the passage, and soon found the excrescence to be less in bulk there than what appeared without the body; and to be in shape as in the drawing. I followed the substance till I reached the *os uteri*, which I found chiefly

Fig. 102.

* York.

filled

Fig. 100.

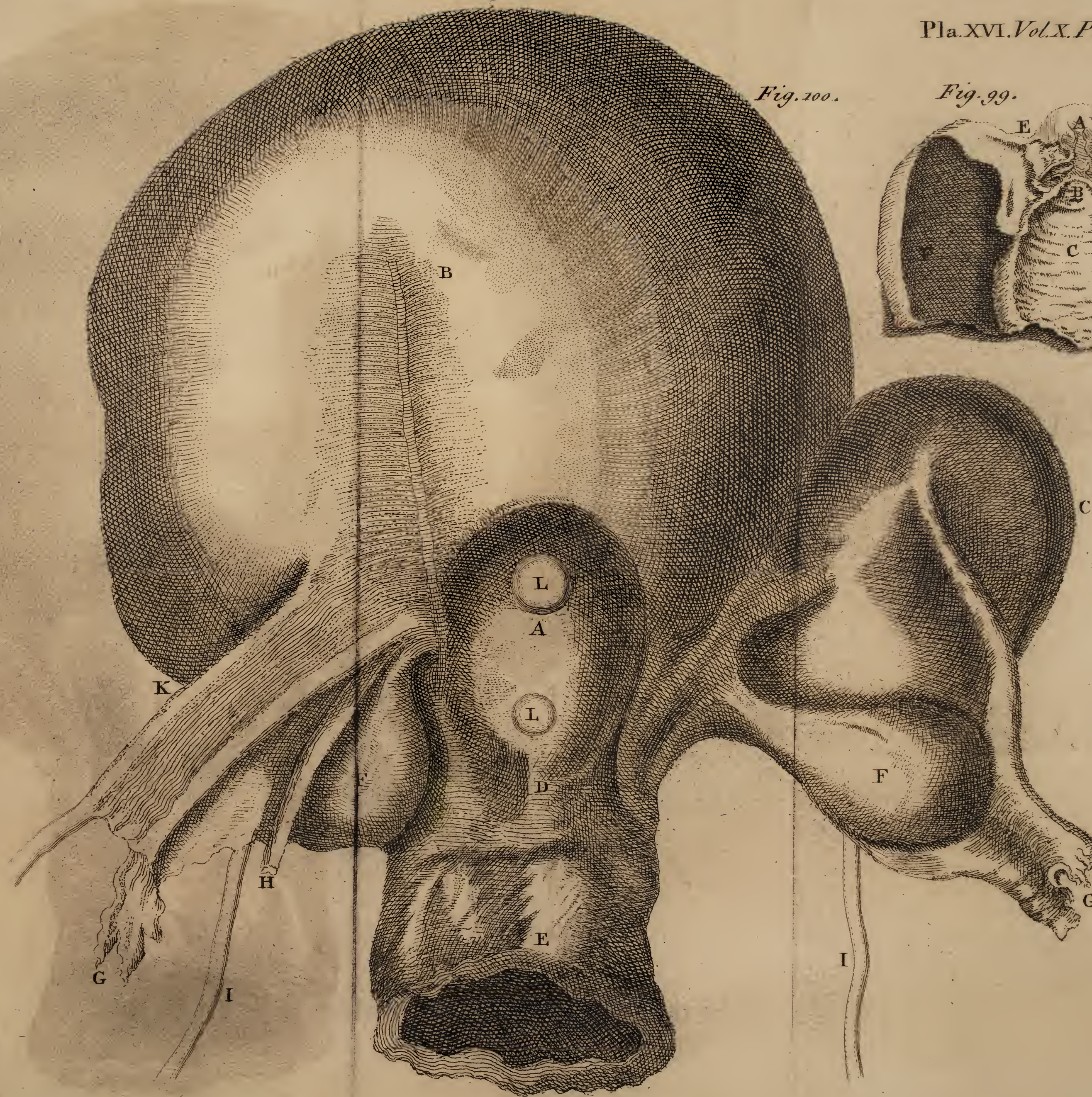
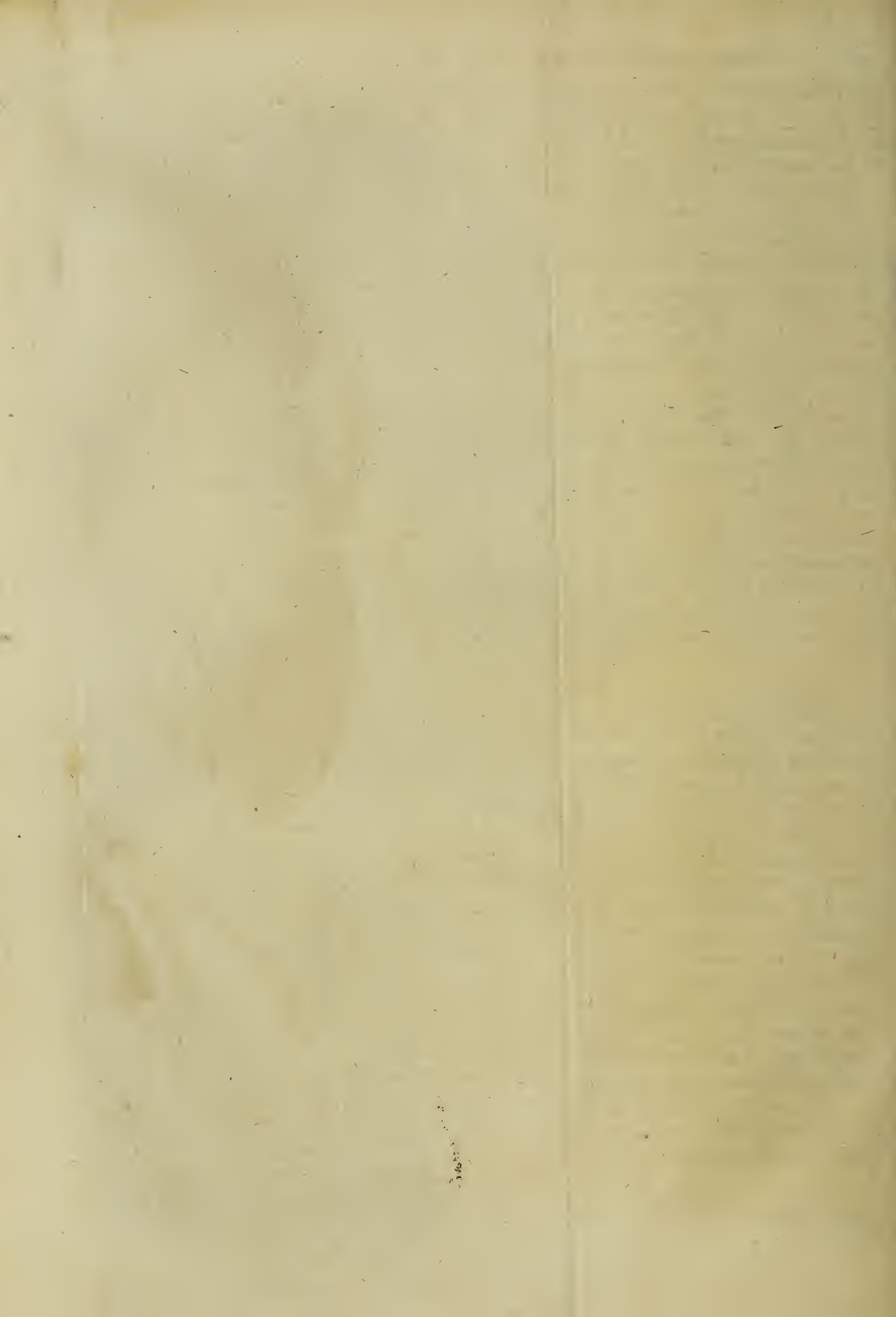


Fig. 99.





filled up, with the neck or smallest part of this substance, leaving only a small part of the *os tincæ* to be perceived on the left side, obliquely towards the back. I tried to penetrate the *os tincæ* with the end of my finger, but could not; however, I so far opened it, as to let out a sort of bloody *ichor*, which was a little offensive in smell.

I asked her, if, when her discharges were so large as to wet her seat, they differed in smell from those which came in less quantities; and if they smelled something like what then came from her when I searched her, which she answered in the affirmative: From whence I concluded she had an ulcer just within the *os uteri*, from the edge of which this *fungus* or excrescence grew. The patient complained of pain in the *uterus* and back, was very faint, and frequently was provoked to vomit, with a feeble pulse, and sometimes sweat.

Upon consultation, we thought proper to tie a ligature as high up within the *vagina* as the Surgeon could reach: which being done, and some internal medicines being ordered, we returned home; and in 2 or 3 days we went again, but found no great alteration as to the uterine complaint; except that the part below the ligature was somewhat more livid, and the fetid smell was much greater, and very like that of a confirmed running cancer: and as the matter was somewhat confined in the passage, I ordered them to wash the *vagina* by an injection made of the common emollient decoction, with *Mel Rosarum* and a little *Tinct. Myrrhæ*. This was frequently done; by which the smell was less offensive; and two days after the excrescence dropped off at the ligature.

The patient has since, in part, recovered her strength; tho' she is not in a good state of health, and her *fluor albus*, as she calls it, is still troublesome to her. This was the last account I have had of her. The excrescence was very solid, of a dark liver colour, and, whilst adhering to the *uterus*, was quite insensible. When cut in two, it resembled the solid substance that is taken out of cancers.

XXIX. A young Lady of 17 years of age, being at a boarding-school about 3 miles from this city, was, on Nov. 28. 1745. taken with profuse sweats, which, after some continuance, and weakening her much, were stopped by means of *saline draughts*, made with *elixir vitrioli*.

On the removal of those sweats an obstruction of the *menfes*, with all it's symptoms, ensued. A shortness of breath, a dry cough, an acute pain in the left *hypochondrium*, rigors, &c. were taken for the signs of a *peripneumonia*; and, the medicines usually prescribed having no effect, a blister was applied on the left *hypochondrium*. The *fætids*, and *musk*, as in a *nervous* case, were also administered in large quantities, but with as little success. It being thought adviseable to bring the young Lady to town, I first saw her on Feb. 12. 1745-6. when I observed a large prominent *tumor* on the left *hypochondrium*, which reached to part of the right, filling up the *epigastrium* and *scrobiculum cordis*, where she complained of a constant acute pain. The *muscles* of the *larynx*, *pharynx*,

An extraor-
dinary im-
posthume in the
stomach; in
a letter from
Daniel Peter
Layard, M.D.
and F. R. S.
to Richard
Mead, M.D.
&c. N°. 495.
p. 406. May
&c. 1750.
Read May 3.
1750.

and

and neck, were much swelled, and the *glands* indurated. The other symptoms were a continual quick pulse, thirst, hoarse cough, difficulty of breathing, *cardialgia*, and obstruction in the *oesophagus*, so that, as soon as any liquid “fell down,” as she expressed it, “to the pit of her stomach,” she instantly threw it up with violent pain, *borborigmi*, *eructations*, and *singultus*. On the 14th, finding the symptoms increase, especially the obstruction in the *oesophagus*, and apprehending that an *abscess* was forming in the *stomach*, I desired Dr *Mead* should be called in, who confirmed me in my opinion. In order to assuage the inflammation, a cooling *mucilaginous mixture*, and a *sperma ceti* one, were prescribed, as also a *laxative glyster*. Next day, being told, that not a drop of the mixtures could be admitted into the *stomach*, Dr *Mead* took his leave, advising the repetition of the glyster every 3 or 4 days, as necessity might require, and that nature should be watched, in case of a favourable turn, which he did not much expect, having observed, that that those *abscesses* more frequently terminate in a *gangrene* than by *suppuration*.

On the 16th the glyster brought away with the *fæces* some pieces of membranes, about a finger's length, and two in breadth; to lubricate the *intestines* I directed $\mathfrak{z}\text{x}$ of plain mutton broth to be injected, which, after the first time, was constantly absorbed and was repeated twice every day, till the 3d of *May*. In this state, with no other sustenance than these *broth-glysters*, the *laxative* one repeated every third day, and a warm *carminative plaster* applied on the *tumor*, did the patient continue till *March* 17. when, observing an intermission in the pulse, and hoping that a decoction of the *Cort. Peruv.* might corroborate the solids, if absorbed, as the *broth glysters* were, I directed $\mathfrak{z}\text{viij}$ of the decoction to be injected, and repeated 4 hours after: by accident, the first was not half thrown up; the second, on the 18th of *March*, at 2 in the morning, had a very extraordinary effect; for being entirely absorbed, about 2 hours after, the young Lady complained of a most acute pain in her *stomach*, which by its violence brought on a profuse sweat, and threw her into a *syncope*, wherein she remained a full quarter of an hour; then, shrieking, made signs to her nurse to bring her the basin; she vomited near two pounds of *grumous blood*, and then some purulent matter; thereupon she discharged by stool above 4 quarts of well digested *pus*, with several pieces of *membranes*, like those before-mentioned. The purulent discharge continued gradually decreasing till *April* 23. *Balsamics*, and small quantities of thin veal and mutton-broths, were daily given. *April* 29. the patient was purged with pulp of *Cassia*. *May* 3. the *mutton-broth glysters* were omitted, the *stomach* now performing its office. The 7th $\mathfrak{z}\text{x}$ of blood were taken from the foot, which brought down the *meneses*. The *tumefaction* and *induration* of the *muscles* and *glands* of the neck were removed by the continual application of the *emplastr. saponac.* And after the use of *stomachics*, and *mineral waters*, the

Fig. 101.

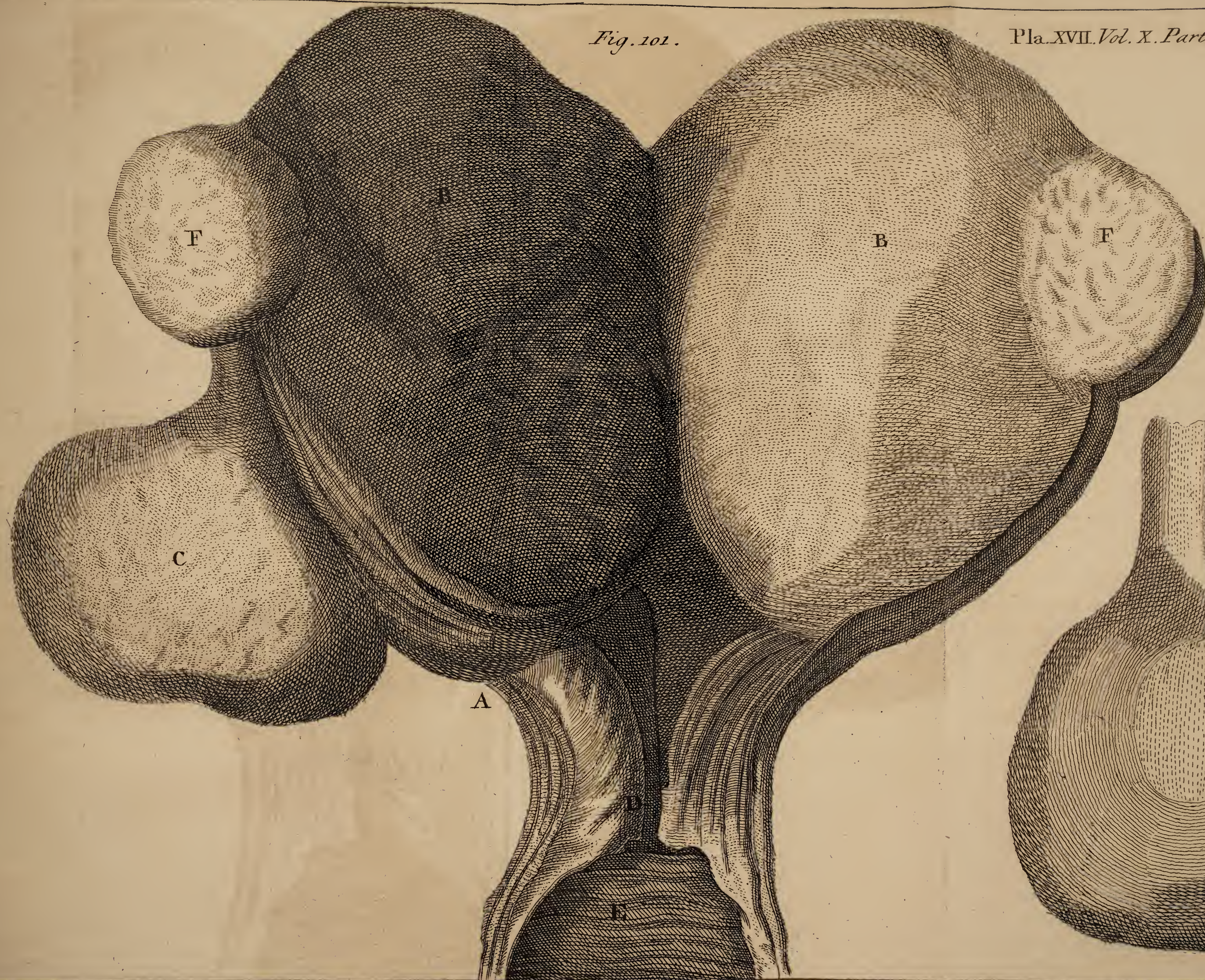
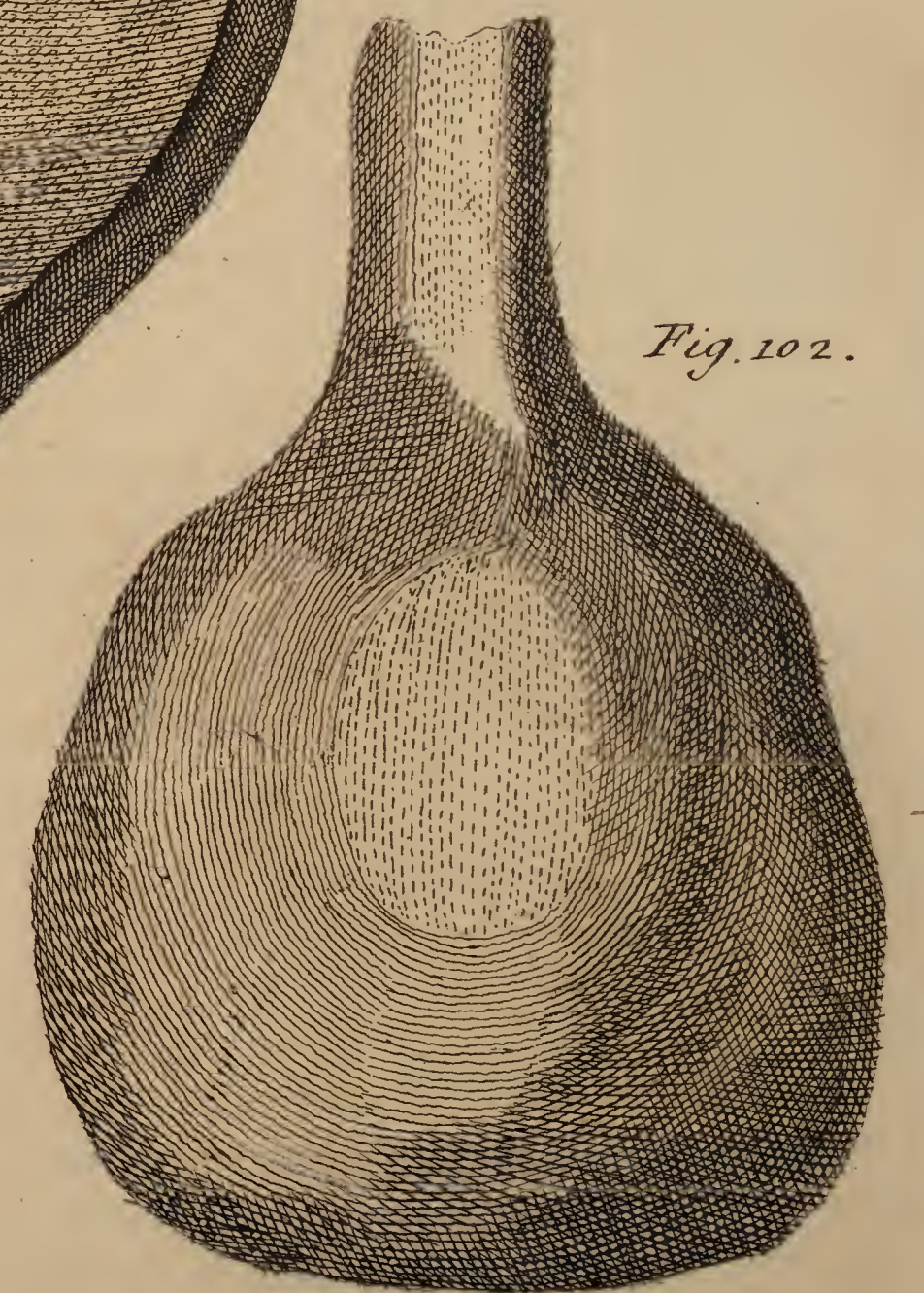


Fig. 102.



the young Lady was perfectly cured on the 17th of June 1746, and has continued well ever since.

It may not be improper to observe, that the *stomach*, on account of the number of blood-vessels it is furnished with, is as liable (*) to *inflammations*, and *abscesses*, as any part of the human body. These are occasioned by a stagnation of the blood, which, if not speedily removed, must greatly endanger the patient's life, by obstructing the necessary vital functions of that *viscus*. The speedy progress of this disease, and the remoteness of the part from proper applications, render it's termination mostly fatal; and, as Dr *Mead* judiciously observed, these *abscesses* more frequently terminate by a *gangrene*, than come to *suppuration*. Those that do suppurate, generally form ulcers, penetrating into the cavity of the *abdomen*, and sometimes also perforate the *integuments*, as the following recorded observations testify. (1) "M. PETIT has found a *carcinomatous* and *fistulous ulcer*, which eat through the *fundus ventriculi*, and the *integuments* of the *umbilical region*.

(2) "M. DUVERNEY also found a hole in a *stomach*, which he could pass his thumb through: this hole was near the *pylorus*, which was extraordinarily dilated.

(3) "Monsieur LITTRE observed an ulcer of 5 lines diameter, about $1\frac{1}{2}$ inch from the *pylorus*; and found 3 pints of *grumous* and *serous* blood in the *stomach* of a young man who had voided large quantities of blood.

(4) "The same gentleman gives an account of a *tumor* on the right side, which being opened between the *last* of the *true ribs*, and the *first* of the *false ones*, there issued out *pus*, *stones*, and the *succus gastricus*, with pieces of digested food.

(5) "And Mr ATKINSON tells, us that he opened a *tumor* on the upper part of the belly, out of which opening part of the *omentum* came, and whatever the patient eat or drank for 8 or 10 days: yet the patient, contrary to his expectations, was cured in six weeks."

But the following observation from FORESTUS comes the nearest to our case "Puella 15 annorum, per biennium ferè cum de dolore ventriculi conquereretur, anno tertio tumorem manifestè viderunt parentes, in eo loco; neque tunc quicquam consilii aut remedii tentatum. Hinc ruptionem animadverterunt, excrevitque puella materiam, biliosam, pituitosam, ac saniosam, per alvum: ex his apostema apparuit." &c. (6).

(*) *Boerhaave* in Aphorism. de ventricul. inflam. p. 228. Articul. 951. 952. 955.

(1) Mem. de l'Academ. des Sciences, Ann. 1716. p. 312.

(2) Hist. de l'Academ. des Sciences, Ann. 1704. p. 27.

(3) Ibid. p. 30.

(4) Ibid. p. 28.

(5) Philos. Transf. No. 371.

(6) *Forestus* Observ. 33. de ventricul. ulcer.

“ LICETUS (7) relates several instances of persons who have lived a long while without food, but then they could admit of liquids; and the young lady which Monsieur LITTRE (8) nourished with *broth-glysters*, in which a yolk or two of eggs, and sometimes a glass of wine, were mixt, could also keep water in her *stomach*, tho’ no other fluid.” An advantage which our patient was deprived of.

From what I have related it appears, that our young Lady had an *abscess* in her stomach, which gradually ripened, and then broke, suppurated, digested, and cicatrized, as all other *abscesses* do; and that during this time, which was near 3 months, she was almost all the while nourished solely by the *mutton-broth glysters*.

Of an iliac passion, occasioned by an appendix in the ilium; by the late Claudius Amyand, Esq; Serjeant-Surgeon to His Majesty, and F. R. S. N^o. 476. p. 369. Apr. &c. 1745. Read May 2. 1745.

XXX. Upon Sept. 15. 1739. a lad about 10 years of age, seemingly in perfect health, after drinking some four small beer, complained of a violent colic; which increasing with great tension of the belly, and continual vomitings of excrements, he died 3 days after of a *miserere*, ending in a mortification of the inflamed guts. He was suspected to have been poisoned; which occasioned Mr *Maccullough’s* being sent for to open him. All the large guts were found empty, up to an *appendix*, or a hernious expansion in the *ilium*, about 3 inches long, and of the same dimensions with the gut itself; which was so contracted and shut by a spasm, that as nothing could pass downwards, so all the *fæces* were retained in the intestinal pipe betwixt this and the stomach, which was greatly distended as far as the *pylorus*; the opening into the stomach there, and that in the guts below, which were contracted and spasmed; being so close, as hardly to admit of a small probe.

The frequent vomitings of excrements, during life, shewed, that the stricture at the *pylorus* had occasionally given way to their coming into the stomach; but, as these were thrown up as soon as let in, this *viscus* was found as empty, as the guts were below the strangulated gut; thro’ which nothing had passed during the course of the disease.

This *appendix* of the *ilium*, more capacious and longer than that usually observed in the *cæcum*, being supported by no mesentery, lay loose and floating. At it’s opening into the gut, it made an acute angle, determining the course of the *fæces* from the upper gut into it, and obstructing their descent into the natural pipe; where the current was made difficult, from a defect in some segment of the fibres inservient to the vermicular and peristaltic action; which, likely, was the occasion of the spasm, the contracted fibres of the gut having no antagonist above, and the compelling force to conquer the resistance being wanting.

In the liver there was a steatomatous tumour stretched out thro’ it’s substance, containing in it’s *cystis* about 3 viij of matter; but so disposed, that the course of the fluids and juices in and out was not impeded.

(7) *Licetus* de his qui diu vivunt sine alimento.

(8) Mem. de l’Academ. des Sciences. Ann. 1716. p. 183.

XXXI. *Ann Stubbensfull* had a very hard labour 17 years before her death, and a little rupture appeared in her navel, and in the next labour it increased; which she endeavoured to cure by a bandage, but in vain; so it continued to increase more and more.

The first time I was called to her, was upon account of a wound at *d.* where it looked as if it would mortify; which I cured, but left a place open as big as half a crown, from which a great quantity of water would sometimes run out; but getting cold, it stopped; and the whole *saccus* was very much inflamed (This wound was formerly made by uneasy trusses). I called on the late Mr *Amyand*; who told me, he had seen in a work-house a rupture of the same kind, but not so big.

She had once an *obstructio alvi* for 15 days; and nothing would do, till I ordered her a glyster of tobacco boiled with urine. The late Dr *James Douglas* saw her; and he and I agreed to open her after her death; to which she consented. At last, she died maniacal. Dr *Douglas* and I would have opened her; but the obstinacy of her children was so great, that they would not agree to it till the 4th day after her death, which happened in *July*, when the putrefaction was so great, that Dr *Douglas*, nor I, nor any body else, could remain by her; and so were obliged, to our great sorrows, to desist.

a. The circumference nearest to the belly 36 inches and an half. *b.* The thickest part, 38 inches. *c.* From *a* to *c*, the length, 34 inches. *d.* The wound. *e.* Several great protuberances.

XXXII. *Mary Nix*, at *Hampton-Poyle*, a small village in *Oxfordshire*, had been remarkable all her life for the preternatural size of her belly. After her death, I had the curiosity, together with some learned Gentlemen of the University, to inspect her body. Her mother was then present, and informed us, that this her daughter was born dropfical; that she herself had been ill of the same disease for some time before, and during, her pregnancy; but, on the birth of this child, she was freed from that disorder. The child, tho' born dropfical, proved otherwise healthy; and, notwithstanding the disease continually increased as she grew up, lived to be near 23 years of age.

She was a tall well-proportioned woman, except with regard to the enormous size of her belly; and, for one of so unwieldy a bulk, healthy, brisk, and active. Her appetite was always good, and she was never more than ordinarily thirsty; had no remarkable difficulty of breathing, not even when she lay supine, nor did her thighs or legs ever swell. Her *menfes*, which appeared at the usual time of life, continued regular, till within 8 months of her death. The only complaint was now-and-then a pain in making water; and the quantity she made was commonly about ℥iv or v . Upon the suppression of her *catamenia*, there succeeded a *dyspnœa*, loss of appetite, emaciation of the superior parts, and a tumefaction of one of her legs with ulcerations. These symptoms gradually increased till her death.

A rupture of the navel, communicated to the R. S. by H. W. Taube, (Dove) Surgeon of the Pearl Man of War. N^o. 473. p. 50. May &c 1744. Read May 24. 1744. Fig. 103, 104.

References to the figures. An uncommon dropfy from the want of a kidney; and a description of a large saccus that contained the water; by Sam. Glafs, Surgeon, at Oxford; sent to Dr Mead. N^o. 482. p. 337. Jan. & Feb. 1747. Read Jan. 8. 1746-7.

Upon taking the dimensions of her body before dissection, we found the circumference of her *abdomen* to be just 6 feet 4 inches, and from the *xiphoid* cartilage to the *os pubis* it measured 4 feet and $\frac{1}{2}$ an inch. The cutaneous vessels, distributed upon the *abdomen*, were remarkably large, and distended with blood, and the spurious ribs were pressed greatly outwards and upwards.

After this general view of the external parts, we began the dissection, by dividing the cartilages of the six superior ribs, and raising the *sternum*. The *thorax* being laid open, we observed that the diaphragm was forcibly protruded into that cavity. The base of the heart lay under the right clavicle, and its *apex* upon the most convex part of the diaphragm; which convexity advanced as high up as the third superior rib. The lungs were surprizingly small, scarce exceeding in magnitude those of a new-born child. The right lobe slightly adhered to the *pleura*, the left was free, and both were in a sound state. Within the *pericardium* was found, as usual, a small quantity of liquor, but none in the cavity of the *thorax*. We next perforated the *abdomen* in the most convenient depending part, and evacuated from thence a surprizing quantity of water, which was lightly tinged of a coffee-colour, limpid as urine, and not in the least fetid. This water was carefully measured, and found to be not above a pint less than 30 gallons wine measure; which must weigh, according to the common calculation, near 240 *l*. We afterwards made an incision into the *abdomen* along the *linea alba*. The integuments upon the epigastric region were very thin; the abdominal muscles much extenuated; and above the *umbilicus* the *tunica cellulosa* contained no fat; but from the navel to the *os pubis*, the *panniculus adiposus* was half an inch thick. Upon dilating the incision, the large membranous bag that contained the water presented itself to view, adhering transversly about 10 inches to the anterior part of the *peritonæum*.

This adhesion being separated, we had a full view of this wonderful reservoir, which was of an enormous size, and had almost occupied the whole cavity of the *abdomen*. In figure, colour, thickness, number, magnitude, and distribution of blood-vessels, it very much resembled the *uterus* of a cow at the end of gestation. The whole inside was scabrous, and looked as if parboiled; and here and there was observed a small quantity of a coffee-coloured sediment. On the left inferior part was discovered the orifice of a duct, which opened obliquely into the cavity of the *saccus*, and would easily admit of a large goose-quill. From this opening the tube advanced about 12 inches between the membranes of the bag obliquely upwards, and towards the right, from whence it was inflected downwards, and passed between the duplicature of the *ligamentum latum uteri*, to be inserted into the bladder of urine. The *saccus* was connected to the *ligamentum suspensorium hepatis*, to a considerable part of the *mesocolon*, to the *peritonæum* on the right side in two or three different places, to the same membrane the whole length of

of the spine, and to the *ligamentum latum uteri* on the right side of the body.

The liver was found, but less than in a natural state; and its convex part adhered closely to the diaphragm. The stomach, spleen, *omentum*, small intestines, and the upper part of the *colon*, were thrust very high up into the left *hypochondrium*. The convolutions of the lower part of the same intestine were intirely obliterated; and that, together with the *rectum*, formed one continued strait tube, from the left *hypochondrium* down to the *anus*. The left kidney, with its emulgent vessels and *ureter*, were in their natural state and situation. The *uterus*, *tuba fallopiana*, and *ovarium*, on the same side, had nothing preternatural; but, on the right side, the *fallopian* tube and ovary were disposed in a very extraordinary manner. The tube, by means of the adhesion of the *ligamentum latum uteri* to the *saccus*, was extended to 3 times its ordinary length. The ovary was likewise, by the same cause, rendered very preternatural, being no less than $5\frac{3}{4}$ inches long, one inch broad, $\frac{2}{10}$ of an inch thick, and $2\frac{1}{2}$ inches distant from the *uterus*. The bladder of urine was very small, but appeared to be sound.

We then made an accurate search for the right kidney; but, to our great surprize, found no such *viscus*, nor any thing analogous to it, unless the *saccus* that contained the water already described, may be esteemed such: and what seemed to favour this opinion, was the disposition of the emulgent vessels on the right side, which were propagated from the *aorta* and *vena cava* to this *saccus*, in the same manner as to the kidney on the opposite side; and, after having ran 12 or 14 inches between the membranes of the bag without any ramifications, were distributed all over it in the manner before-mentioned.

From the foregoing account the following queries are naturally suggested, which I leave to the determination of the learned:

Was not the *saccus* originally a mis-shapen kidney, and the duct a *Query 1.*
ureter?

Was not the water contained in the *saccus* prevented from growing *Query 2.*
putrid, by being continually drained off thro' the duct into the bladder of urine, and by being afresh supplied by the emulgent artery; and more being secreted than was evacuated, the quantity thereby continually increased?

Was not this the reason why the patient had never any anasarcaous *Query 3.*
swellings of her thighs or legs, nor any thirst, or other signs of a confirmed dropsy?

Were not the lungs prevented from growing by the great diminution *Query 4.*
of the cavity of the *thorax*, and the pressure they sustained from the distended *abdomen*? And might not their never having occupied a larger space than they did at birth, be the reason she never laboured under any difficulty of breathing?

Was

Query 5.

Was not the bladder of urine likewise by the superincumbent weight, prevented from dilating itself; and that the reason why the water was often made, and always in so small a quantity?

In order to convey a more clear idea of the several parts already described, to those who may not have an opportunity of inspecting them (they being properly preserved for that purpose), and being sensible of the great difficulty of clearly representing by words such things as are out of the ordinary course of nature, I must beg leave to refer the reader to the figures hereto annexed.

Fig. 105.

Fig. 105. *a a a a*, The great *saccus* that contained the water. *b b*, the greater diameter of the *saccus*. *c c*, the lesser diameter. *d*, a pricked line shews the entrance of the emulgent vessels. *e f*, shews the course of the duct between the membranes of the *saccus*. *f*, the orifice opening obliquely into the cavity. *e g*, the inflected part of the duct, after it's egress from the *saccus*, which passed between the *ligamentum latum uteri* to the bladder of urine. *g*, the duct, divided near the bladder, where it's cavity was so small as only to admit of a common-sized probe.

Fig. 106.

Fig. 106. represents the posterior view of the *uterus*, *fallopian* tubes, and ovary. *a a*, the *uterus*. *A*, the *os tincae*. *b c*, the *fallopian* tube in a natural state. *c*, the *morsus diaboli*. *d*, the left ovary in a natural state. *e e*, the *ligamentum rotundum*. *f g*, the right *fallopian* tube. *g*, the *morsus diaboli*. *h h*, the right ovary. *i i*, the *ligamentum rotundum*. *k k k k*, the *ligamentum latum uteri*. *l l*, it's adhesion to the *saccus*.

An improvement on the practice of tapping; whereby that operation, instead of a relief for symptoms, becomes an absolute cure for an ascites, exemplified in the case of Jane Roman; and recommended to the consideration of the R. S. by Christopher War- rick, of Truro, Surgeon. N^o.

XXXIII. 1. Among all the operations of Surgery, perhaps there is not one, of equal importance, less obliged to improvements, than that of the *Paracentesis*; it being, at this time, notwithstanding the frequent use thereof, in the same state of imperfection as when first introduced among us; a mere palliative remedy, or a relief for symptoms. Mr *Monro* and Mr *Garengeot* have, indeed, thrown some considerable lights thereon; the former, * by substituting a belt, instead of the ordinary long bandage for compressing the *abdomen*, and in ascertaining the proper place of puncture; the latter, in making the evacuation at once, and in rinsing the emptied cavity, to remove the feculent part of the waters; which, subsiding therein, and being apt, by it's acrimony, to make rude impressions on the viscera, he says, † frequently occasion mortifications thereof. These are, I believe, the only improvements therein; at least, that seem to have sufficient solidity in them to recommend themselves to our practice. It's proper object, an *ascites*, however, still baffles our endeavours, and renders the use of it contemptible and precarious; always assuring us of a relapse, by returning as con-

472. p. 12.

* Vide Medical Essays, Vol. I. Art. 18.

† *Traité des Operations de Chirurgie*,Jan. &c. 1744. Chap. 6. Art. 6. de la *Paracentese*.

Read Feb. 16.

1743-4.

stantly

Fig. 104.

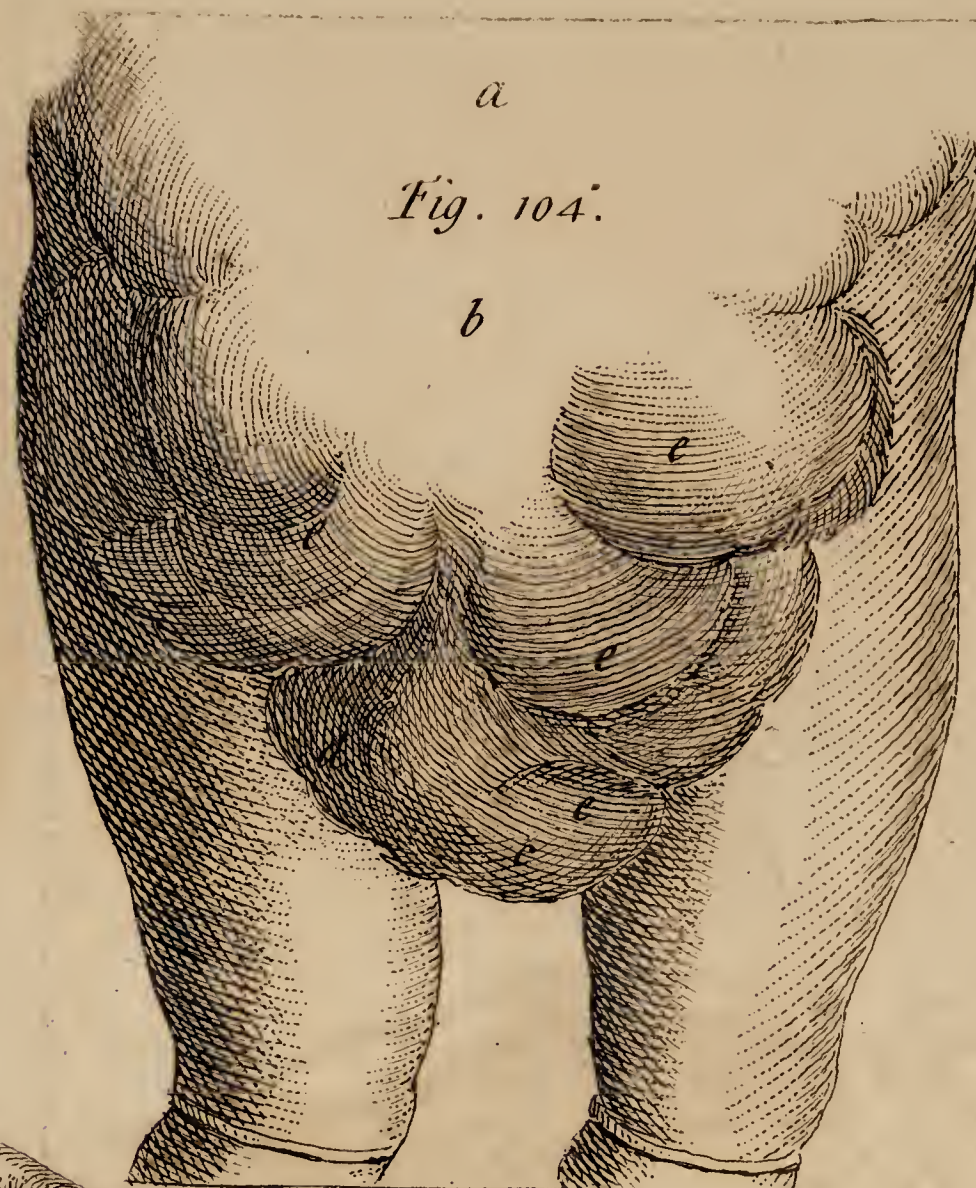


Fig. 103.

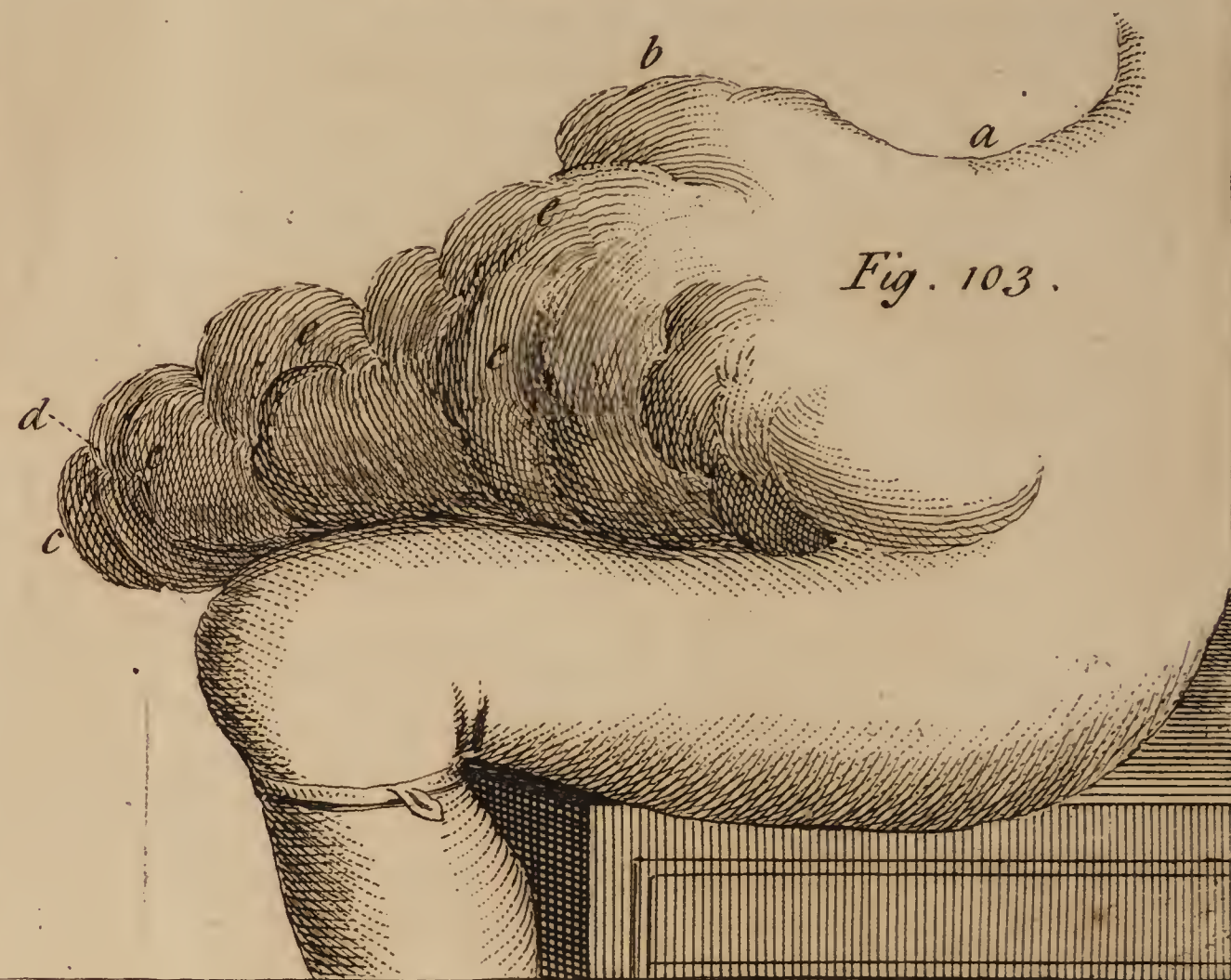


Fig. 105.



1 2 Feet

Fig. 106.



1 2 3 4 Inches



stantly as ever we put this method alone in practice to remove it; so that, even in case of it's being considered as a palliative remedy, or a relief of the symptoms, by repeating the use of the *trochar*, we must suppose, at the same time, an equal certainty of pain, continual anxiety, and, perhaps, death; which, at last, must be the inevitable consequence of it. From these considerations, and a sincere willingness to communicate to others what I think herein has been of service to myself, I am inclined to believe the following piece of practice, considering the nature and importance of it, may not be unacceptable to the public; not only as it tends to establish an absolute cure for an *ascites*, but likewise as it may afford us some light in the treatment of an *hydrocele*, *hydrops pectoris*, *ovariorum*, and other diseases incident to the human fabric.

In 1742, among a great many hydropics that fell under my care that year, I was called to the assistance of one *Jane Roman*. She was an inhabitant of the parish of *St Agnes*, near 50 years of age, and confined to her bed, under that species of dropsy called *ascites*, owing it's rise, some years before, to the severity of a lingering intermittent fever. The most remarkable of her complaints were, loss of appetite, difficult breathing, unquenchable thirst, suppression of urine, and a short, importunate, asthmatic cough, joined to that essential symptom of the disease, a large quantity of extravasated waters in the cavity of the *abdomen*, distending it to an enormous size, and perceptibly fluctuating. Her more inferior parts were likewise swoln to an uncommon magnitude, with livid spots and vesications in divers places. Under these circumstances, and already fatiated with tedious courses of ineffectual medicines, I drew from her (*Sept. 20.*) 36 pints of a greenish transparent lymph, by a *paracentesis* made after the usual manner; whereby her complaints vanished, and she was soon re-established on foot again. With some part of the extracted lymph, which I had conveyed to my own house, on my return thither, I made the following observations:

I. Being as warm as it came from the *abdomen*, with one pint of it I mixed the like quantity of fresh *Bristol* water; and immediately a slight *coagulum* ensued. OBS. II.

In mixing equal parts of warmed lymph and *Coburn* claret together, the same phenomenon appeared; the *coagulum* subsided, and the mixture became milky. OBS. II.

Being mixed with *Pyrmont* water, it manifested little or no change, only went turbid. OBS. III.

I mixed a decoction of the *cortex* with the like quantity of warm lymph, and it dropped a branny sediment. OBS. IV.

Lymph *per se*, boiled, became gelatinous; but, being mixed with a strong solution of *terra foliata Tartari*, it soon resumed it's former fluidity. OBS. V.

In bringing the above mixture to a state of boiling, the phenomenon of coagulation appeared more eminently in each of them; especially that with claret. *Eaton's* styptic, tormentil roots, pomegranate-peels, and

and almost every restraining, more or less afforded the same appearances of coagulation. Notwithstanding the disappearance of the symptoms, and the favourable prospect that ensued the evacuation of the waters, the relief which she had thereby was only temporary, and of a short duration: for, *Sept.* 30. an inundation again alarmed her, and obliged her forthwith to remove the bandage, for fear of suffocation. Hence, to the latter end of *Oct.* she re-filled incredibly; and, notwithstanding any method used to prevent it, within 40 days after the *paracentesis*, there was again collected, in the *abdomen*, and depending parts, a quantity of lymph, equal to, if not greater than, that which I had but just before thence extracted. All her former complaints, especially the *dyspnœa*, likewise returned, and oppressed her more violently than ever.

Oct. 29. The waters being ready to break their confines, and the pain and distention insupportable under them, she again desired my assistance to relieve her. I had, by this time, drawn some conclusions from the above observations on lymph and restringents, and flattered myself, that some of them, especially those of the warmest kind, applied immediately to the parts affected (the ruptured lymphatics) must, according to their known *modus* of operation, close up their mouths, and prevent a further effusion of their contents, and, consequently, a return of the disease. In order then to obtain this desirable end, I resolved to try their efficacy, by way of injection, on the emptied cavity: and, for this purpose, the claret and *Bristol* water seemed to claim the superiority in my esteem; not only as they produced the strongest *coagulum* with lymph, but likewise in being the safest, and least liable to create any uneasy sensations on the *viscera*. The experiment, however, being of a pretty singular nature, I communicated my sentiments thereof to *Dr Colwell*, and *Dr Russel*, two eminent Physicians of our county, and had the pleasure of finding them meet in their approbation: the latter, favouring me with his presence, saw the conduct of the whole affair.

My *apparatus* was, a large *trois-quarts*, made on purpose, and dipped in oil; an *injector*, capable of containing 2 or 3 pints, adapted thereto; and 3 or 4 gallons of blood-warm injection, composed of equal parts of *Cobore* claret, and fresh *Bristol* water; besides compress, bandage, &c. as is usual on these occasions. It was conducted pretty near after the manner following: being seated on her bed-side, and proper assistants attending her, I plunged the *trois-quart* into the *abdomen*, about 5 or 6 inches below, and as much on the left side of the *umbilicus*; and thereby soon discharged upwards of 20 pints of such clear briny lymph as I had before; which quantity did not exceed $\frac{2}{3}$ of the whole, though as much as her strength could well bear: the claret and *Bristol* water being then in readiness, I began to replenish the empty cavity therewith; but I had scarce injected 10 or 12 pints of it, before a *syncope*, a very material obstruction, made some advances, and would fain baffle my design. Here I perceived the great expedition necessary in conducting this experiment; that symptom being more or less violent, as I happened

happened to be dextrous, or remiss, therein ; and was, for the most part, the only one of consequence that attended it. Quickening therefore my hand as fast as I was able, and an assistant stopping the mouth of the *cannula* with his finger, to prevent a return, I soon brought her up to her former magnitude, and had the pleasure thereby of seeing the above symptom suspended. I had then time to ask her, what kind of sensation this new piece of practice excited within the cavity ? and whether or not she thought herself capable of undergoing it a second time ? she answered me in the affirmative ; and said, it seemed as it were entering her stomach. Notwithstanding I had reason to believe my intentions already answered, as much as in bringing those restringents in contact with the parts affected, yet, as there was a great quantity of lymph left behind in the cavity undischarged, which, on account of the *syncope*, I could not well prevent, I imagined their action, and full efficacy, might thereby be, in some degree, interrupted. Every thing therefore being in a favourable way, I repeated the mixture for a second injection (the claret being in a double proportion of the water, to render it the more efficacious for that purpose) ; drew off the whole contents of the *abdomen* to as much as would flow through the *cannula* ; repeated my injections as before ; and once more, without the least interruption, replenished her therewith. This total discharge, however, made a great alteration in the face of affairs ; and her being full, and under distention, now, altered not, as it did at first, the case. A pungent pain possessed her breast, frequently darting through all the *viscera* ; her breathing became extremely difficult ; her pulse faltered ; the *syncope* returned ; and she became speechless. Under these circumstances it was high time to conclude my design ; and therefore, having emptied the cavity, as well as the violence of them would permit it, I withdrew the *cannula*, applied proper compress and bandage, and finished the operation after the usual manner ; which was the more agreeable, as it ended with the perfect recovery of her senses.

The day following, (Oct. 30.) on repeating my visit, which I did for divers others successively, I had the pleasure of finding things under a favourable disposition ; a gentle *diaphoresis*, from a liberal use of cardiac medicines, having totally removed the *syncope*, *dyspnœa*, and all other complaints.

Nov. 12. I renewed my bandage, and set her on foot again.

The 24th I waited on her again for the same purpose ; found the swelling in the extremities going off, her heart in a chearful disposition, her appetite strong, and no symptom of a relapse, as she formerly had long before that time approaching.

Dec. 1. the last visit I made her, she told me, That there was no further occasion for my assistance ; and that the only complaint she had then, was, in not having the good luck to experience this remedy sooner. Finding every thing therefore in a favourable way, her appetite well, her urine in due quantity, her breathing clear, and the extreme

parts of their natural size, I left her in pursuit of that health which she soon acquired, and now enjoys in as eminent a degree as any other person whatsoever. To conclude; the integuments, and other parts of the *abdomen*, having not yet recovered their former tone; or, for aught I know, some of the injection being coagulated therein; she still is, and ever has been, since the operation, somewhat above her natural bigness: however, it is so far from being an inconvenience to her, that she made me a visit, on a trifling occasion, a few days since, on foot; and informed me, that she had, of her own choice, walked 7 miles that morning for the purpose. Her welfare now is upwards of a twelve-month's standing.

A method of conveying liquors into the abdomen during the operation of tapping; proposed by the Rev Stephen Hales, D. D. and F. R. S. on occasion of the preceding paper; communicated in a letter to C. Mortimer, M. D. Secr. R. S. Ibid. p. 20. Read Feb. 23. 1743-4.

2. It occurred to me, on your reading, *Thursday* last, before the Society, the case of the woman at *Truro* in *Cornwall*, who was cured of a dropfy, by injecting into the *abdomen* *Bristol* water and *Cobore* wine; after having drawn off a good quantity of the dropfical *lymph*a; that, in case of further trial, that, or any other liquor, shall be found effectual to the purpose, it might be more commodiously injected in the following manner; *viz.*

By having two *trochars* fixed at the same time, one on each side of the belly; one of them having a communication with a vessel full of the medicinal liquor by means of a small leathern pipe: this liquor might flow into the *abdomen*, as fast as the dropfical *lymph*a passed off through the other *trochar*; whereby the dropfical *lymph*a might be conveyed off, to what degree it shall be thought proper; and that without any danger of a *syncope* from inanition; because the *abdomen* would, through the whole operation, continue distended with liquor, in such a degree as shall be found proper, by raising or lowering the vessel with the medicinal liquor in it.

It is probable, that, if the surface of the medicinal liquor be about a foot higher than the *abdomen*, it may be sufficient for the purpose.

It were easy to find the force with which the *abdomen* is distended by the dropfical *lymph*a, by seeing to what height it arose in a glass tube fixed to the *trochar*; which tube being taken away, it might, I suppose, be sufficient to have the medicinal liquor flow in from a lesser perpendicular height, than that to which the dropfical *lymph*a arose in the glass tube.

A letter from Mr Warren, to John Machin, Esq; Secr. R. S. Prof. Astr. Gresham. containing further accounts of the success of in-

3. I beg leave to return my best thanks to the SOCIETY in general, and to you in particular, for their unanimous thanks to me, and their approbation of what I communicated to them in relation to *Jane Roman*, whom I tapped and injected for a dropfy, and yet remains in *statu quo*; though she informed me yesterday, that she had laboured under a tertian ague ever since last *Jan.* Indeed I should not wonder, if, after 19 months space, she should want the same operation again, in a country where poison [a dram] is habitually become the common nutriment, physician, and counsellor on all occasions.

Your

Your kind communication of Dr Hales's judicious remarks and improvement on my discovery, does me great honour and pleasure; and the more so, as I was so happy as to have discovered the use and efficacy of injections by means of one puncture only, on a poor woman, about 10 days before I received your's; from whom I drew near 50 pints of dropfical lymph, by an easy transmutation thereof into an appropriated medicinal fluid; which was, without any difficulty, retained within the cavity near two hours, and, at the close of the operation, drawn all off at once, without the least symptom of a *syncope* from inanition; of which I shall beg leave to acquaint you further, when I see how she holds it, as well as of what else remarkable may occur in the course of my practice.

injecting medi-
cated liquors
into the abdo-
men, in the
case of an
ascites. N^o.
473. p. 47.
May &c. 1744.
Read May 24.
1744.

CHAP. VI.

The Humours and General Affections of the Body.

I. THERE is not perhaps any disease more fatal than the small-pox, when attended with hæmorrhages, purple spots, and other terrible symptoms. I think one might venture to affirm, that, in proportion to the number of the sick, fewer recover under these circumstances, when treated by the common methods, than do even from the plague itself. A remedy therefore in any degree adequate to the malignancy of the disease, which could restore the broken crasis of the blood, and correct the putrid gangrenous disposition of the juices, must be of infinite service to mankind; and such a remedy the *bark* appears to be.

Concerning
the use of the
Peruvian Bark
in the small-
pox; in a
letter from
J. Wall, M.D.
to Edward
Wilmot, M.D.
F. R. S. and
Physician to
His Majesty.
N^o. 484. p.
583. Oct. &c.
1747. dated
Worcester,
Mar. 10.
1746-7. Read
Nov. 5. 1747.

The first author I meet with, who mentions the use of the *bark* in any stage of the small-pox, is Dr Morton; and he recommends it only in the (a) decline of the disease, when the secondary fever is mild, and puts on the type of an intermittent. But as the *primæ viæ* are then very much loaded, even in the mildest forms of the confluent small-pox, this

(a) Ubi viribus veneni, durante exanthematum eruptione, ptyalismo vel quovis alio modo ex parte fractis, licet non penitus deletis, atque inde usque ad stadium declinationis intus cohibitis & revirescentibus, febris recidiva indolem præbuerit benignam, periodicis exacerbationibus & remissionibus sese alternatim excipientibus (quem typum in variolis mediis, prope ad malignas accedentibus, fere semper observare licet) curatio nulla methodo aut remedio certius ac feliciter absolvitur, quam celebri antidoto, cortice sc. Peruviano, qui reliquias veneni subigendo, ut in cæteris quibuscunque συνεχέσι, certandi ansam tollit, atque inde luctam febrilem derepente componit; unde ægrotans post biduum vel triduum ἀπύρετος factus, illico sine quovis ærumnoso symptomate subsequente recon-
valescit, &c. &c. Morton Pyretolog. de Variolis, Cap. 10.

remedy (if used only in that stage) does not seem to promise much ; at least, not till the *faburra* in the first passages be in some degree carried off. And accordingly, (b) Dr *Frewin* found that it did not at all answer, even under his direction, till the first passages had been emptied.

Since that time, the good effects of the *bark* in mortifications and putrid ulcers have been discovered ; and Dr *Monro*, reasoning from thence, did some time ago (c) recommend it in the first stages of the small-pox ; as promoting the maturation, and procuring a mild well-conditioned *pus*. On his authority alone, had I no other reasons, I should have been induced to try this medicine in some bad kinds of that distemper, where the suppuration does not proceed well ; but having also seen the *bark* of great service in other diseases, where the texture of the blood seemed much broken ; and particularly in petechial and purple fevers, attended with hæmorrhages, and other terrible symptoms ; I was the more ready to conclude that it might be of great use in the small-pox under the like circumstances ; more especially, as Dr *Monro*, in the paper above-mentioned, assures us, that, by its use, “ *Petechiæ*, in several variolous patients, became gradually more pale-coloured, and at last disappeared.” I was the less fearful of making an experiment which was new (at least in these parts) in a distemper attended with such fatal symptoms, because scarce any recover under them by the common methods. For, to use the late celebrated Dr *Freind*’s words [*Epist. de Purgant.*], “ *Ad has angustias cum ventum sit, nequaquam oportet dubitare, an anceps præstet dare remedium, an nulum: quod, utcunque anceps, potest fortasse vitam servare, potest saltem ad dies aliquot protrahere. Sed quid si non successerit? satis est, ut quis hoc medicinæ genus jure experiri putetur, si id vel in millesimo corpore profecisse compererit.*”

The first person to whom I gave the *bark* in the small-pox, was one Mr *Hall*, a young Gentleman of about 24 years of age, who lived within 2 miles of this town. After heating himself violently at a ball, he was seized with the previous symptoms of the small-pox. The third day of his illness he sent for me ; and before that time he had been blooded, and taken a few salts and manna, which had given him 2 or 3 stools, and afterwards used the *Decoct. Nitros.* The blood, which had been taken away, appeared highly inflamed. When I saw him, I found the pustules extremely numerous under the skin, and his body covered with purple spots : beside this, his nose had bled profusely, and he had had several bloody stools ; or, to speak more properly, he had voided large quantities of blood from the *anus* ; for it was merely blood without excrement. His pulse was extremely quick, but rather weak, and the pain in his loins violent. The case appearing so desperate, I determined to give him the *bark* ; of which he took every 2 or 3

(b) In his Letter to Dr *Freind*. *Comment. de Febris*, 5°.

(c) *Med. Essays*, *Edinburgh*, Vol. X. Art. 10.

hours, and the *Tinct. Rosar. acidulat.* for common drink. Within 48 hours the purple spots disappeared, and the hæmorrhages stopt intirely, his pulse grew fuller and slower, and the pustules came on well. From this time to the 9th day after the eruption, the appearance of the disease continued well in every particular; and that whole time he persevered in the use of the *bark*, &c. and for 2 or 3 nights had taken a little *Syr. e Mecon.* On the 9th day in the morning I saw him well, considering the disease: his spirits were good, and he spit well, but very largely. About the middle of the day he dropt asleep, when his nurse left the room, and carelessly stayed away a considerable time (as I remember, whilst she eat her own dinner). At her return, he was found with his head dropt from off the pillow, and dead, suffocated, probably, by the quantity of phlegm, which, his head lying accidentally low, he could not discharge without assistance. Though in this first instance my patient unhappily miscarried, I thought I had sufficient reason to be pleased with the effects of this medicine.

Soon after this I was called to a son of Mr *Yates*, a noted Farmer at *Hampton-Lovat* in this county. He was about 12 years of age, and this was the sixth day from the first seizure. The pustules were confluent, and appeared like bloody pimples: his breast was covered with *petechiæ* and purple spots: he had a great itching of the nose so that an hæmorrhage from thence was to be feared. He had been delirious for 2 or 3 days past, and his pulse was very weak, quick, and trembling. I ordered him *Extr. Cort. Peruv.* ʒi every third hour, and acidulated all his liquors with *Ol. Vitriol.* well covered with *Syr. Sambuc.* The purple spots disappeared, after he had taken ʒij of the *Extract* (which nevertheless was continued on through the whole disease). The pustules came on very favourably, and the distemper afterwards, in it's several stages, more resembled the distinct small-pox than the confluent: yet I never saw one where the pustules were more numerous, and more truly confluent, not on the face only, but the arms and the whole body: for, in many parts, the skin was raised for a considerable extent by the included *pus*, as if it had been done by a blistering plaster; particularly the arms from the elbows to the wrists were intirely raw; the skin, upon letting out the matter, coming off like a glove in one intire piece.

About the same time I was called to a young man of 21 years of age in the parish of *Elmley-Lovat* in this county, who, during the eruption of the small-pox (which appeared on the second day of his illness), had a violent hæmorrhage from the nose, purples very numerous, and excessive pain in the loins. He went well through the disease by the same method. Neither of these two had any secondary fever.

In the cases above-recited I trusted to the *bark* alone; excepting only that I acidulated the liquors with the vitriolic acid; but lately I have joined alum with it, and I think to advantage: for, as the chief efficacy of the *Cort. Peruv.* seemed to me to arise from it's styptic and antiseptic qualities, whereby it not only strengthens the solids, but gives a firm-
ness

ness also to the blood and juices, and thereby prevents their degenerating into a putrid *sanies*, I thought it might not unusefully be joined with alum, a salt of singular virtue in the same intentions, and which is well known to assist the *bark* in other cases, where it is ordered as a styptic.

Mr *Higgins*, of *Tedney* in *Herefordshire*, of about 24 years of age, after very violent exercise in hot weather, was seized with the previous symptoms of the small-pox. The pain in his back particularly was very violent, and his anxiety intolerable. On the third day of his illness, when I first saw him, his skin was covered with purple spots, many of them as large as a silver penny; and his pulse was small, and very quick. Notwithstanding this, as he was a sanguine young man, the weather hot, and the sickness came after violent exercise, I ventured to order a small quantity of blood to be taken from his arm, which I found very tender and florid; and at the same time I directed for him as follows:

℞ Extr. Cort. Peruv. ℥ss. Alum. crud. ℥ii. Aq. Cinnam. ten. ℥vii.
Syr. Cydon. ℥i. M. cap. coch. ii. larga alternis horis.

He had emptied this bottle the next day at noon; when I visited him again, and found him much better; his pulse being full and regular, and the anxiety and pain in his back nearly gone. The small-pox now began to shew itself, and the purples declined apace. The urine which he had made the day before they had imprudently thrown away before I came; but they informed me that it was extremely red, as if mixed with blood; somewhat resembling, as they said, the water in which fresh meat had been washed. He had had several stools; so that, fearing lest that discharge might be too violent, in the next prescription I omitted the alum, and substituted the *Terra Japonica* in its stead. This mixture he continued through the whole course of the disease, and used no other medicine; excepting only that I found it necessary to give him a quieting draught in the evenings, containing a little *Syr. e Mecon.* a few grains of alum, and a scruple of the *Extr. Cort. Peruv.* All the bad symptoms soon disappeared intirely: he had no second fever, but got thro' the disease very easily.

Mr *Bradford*, a Farmer at *Claines*, near this town, about 21 years of age, was seized after the same manner, and got very well thro' the disease, tho' it was confluent, by the same method. I did not see him till the sixth day of his illness, and then his juices were in so bad a state, that, besides the *petechiæ*, which were very numerous, a place on his shin-bone, about the size of a crown-piece, which had been bruised a day or two before his first seizure, was now gangren'd; and the edges looked of a very deep red, as if the corruption was spreading further; his throat likewise was so sore that he could scarce swallow at all. I ordered him the same mixture as above, *viz.* *Extr. Cort. Peruv.* ℥ss. *Alum. crud.* ℥ii. in about half a pint of liquid. He took of this two large spoonfuls every other hour. It was not without great difficulty that

that he got down the first 2 or 3 doses; but he had resolution, and persisted; and before he had finished the bottle his throat grew better, and he swallowed tolerably well. The next day the *petechiæ* began to fade away, and soon after disappeared intirely; the gangrene also stopt, and in a few days separated. He continued this medicine thro' the whole disease, and needed no other.

Soon after Mr *Bradford's* recovery, his sister (about 19 years of age) who lived with him, was seized with the confluent small-pox, attended with innumerable *petechiæ* and purple spots, together with a prodigious hæmorrhage *ab utero*, violent pain in the loins, a great looseness, and dejection of spirits. She took the *bark* with alum in the same method, through the whole course of the disease, and recovered. The *diarrhœa* here appeared to me to proceed from the putrid state of her juices; and therefore, notwithstanding that, I continued the use of the mixture, and this with good success; for by it all the bad symptoms were gradually mitigated, and, in a few days, intirely carried off.

But one of the most remarkable instances that has come to my knowledge, both of the efficacy of the *bark* in this terrible distemper, and the whole course of the disease is the following.

A fervant maid, belonging to one Mr *Buttor*, of this town, was seized with the previous symptoms of the small-pox; but as the pain in the loins was excessive, the disease was unhappily mistaken for a fit of the gravel; and accordingly she had been blooded plentifully, and taken opiates and saponaceous medicines. It was the second day of the eruption when I was first called to her. The pustules were then extremely unmerous, small, and in appearance like a violent itch: her whole body, arms, and legs, were covered with broad spots of a deep purple colour, many of them as large as a sixpenny-piece; her eyes were red, and full of tears, and her countenance expressed a great anxiety (a symptom not easy to be described, but a terrible prognostic in all acute diseases); her throat was so sore, that she swallowed with the utmost difficulty; by fits she was delirious, and vastly restless: she had a violent diarrhœa, a prodigious uterine hæmorrhage; and a very quick small pulse; in short, she seemed just on the brink of the grave. I gave her the *bark* with alum, in as large a quantity as she could get down. In about 12 hours her throat grew better, and she swallowed tolerably well; after which she took $\frac{3}{4}$ ss of the extract of the *bark*, and \mathfrak{z} ij of alum in 24 hours. This medicine I continued, without variation, for 3 or 4 days, when the alum made her so sick, that I was obliged to lessen it's quantity, continuing still to give her the extract as before. Within this time the purples had all disappeared; the hæmorrhage was stopped, and the looseness considerably abated; the pustules came on well, and she spit plentifully. The disease continued to proceed very well, till the 16th day after the eruption; but then, growing tired of her medicine, she persuaded her nurse to omit it; which she did for about 24 hours. During this time she took but a very small quantity of liquids; for, as she

she seemed to lie easy, and in a kind of dozing slumber, her nurse thought she ought not to disturb her. It was prodigious to observe the alteration occasioned by this omission and neglect: her pulse grew quick and weak, and the humours acquired the highest degree of putrefaction: so that she died, perfectly gangrenous, on the 20th day.

These are a few instances, out of many others which I could produce, of the surprizing effects of this antidote. Indeed I have given it to very many persons in the first stages of the distemper, where the *petechiæ* have appeared before, or as soon as the pustules of the small-pox; and to others in the time of maturation, where the matter has been crude and watry; and, I can safely say, almost always with success. I now usually continue it through the whole course of the disease, till, the scabbing being perfected, I find it time to cleanse the first passages; and sometimes I order it in the intervals betwixt purging for some time longer, where I find the solids weak, or the humours thin and acrimonious. When I am called to a person, and, from the appearance of *petechiæ*, purple spots, hæmorrhages, miliary eruptions, or the like, find that the texture of the blood is broken, or in danger, I immediately order the *bark*. Nor does the quickness of the pulse deter me from it's use; on the contrary, I think it absolutely necessary to give it, where the pulse is quick, if at the same time it be weak. Because, for very obvious reasons, I judge that, in this case, the solids are weak, and the fluids disposed to a putrid acrimony.

From the cases here recited, as well as from many others which I have met with, I think nothink more effectually or speedily cures a sore throat in the small-pox than the *bark*; nor did I ever find it at all check the spitting in those sorts of small-pox where that evacuation is necessary.

If, in the first stages of the disease, the *bark* seems to run off by stool, so far from being prejudicial, I have commonly found it of service. For, as * *Hoffman* well observes, that a natural looseness often carries off the *petechiæ*, so that produced by the *cortex* herein imitates the kindly efforts of nature; in that respect assisting the Physician in his principal office, which is to be *naturæ minister*.

In most of those, to whom I have given the *bark*, I have found the maturation of the pustules forwarded by it, and the disease shortened in it's duration; an article surely of no mean consideration. I commonly use the extract (made by boiling down a decoction of the *bark*, without the addition of any alkaline salt) preferably to the gross substance; as being, I think, of equal efficacy, and less apt to load the patient's

* Alvi fluxus licet copiosus non adeo pertimescendus est ** sc. sicuti in febribus malignis petechialibus, alvi profluvium morbi facit solutionem; sic etiam in mali genii variolis idem evenire experientia docet. *Hoff. de feb. variolos. Thes. patholog.* § 2

Alvi profluvium non nocet, quando petechiæ pariter ob valde irregularem anni constitutionem complicantur, ut potius remedio sit, & malignos humores optime expurget. *Id. ibid. Cautel.* § 16.

stomach. In children and delicate persons, who are apt to nauseate this remedy, I have with success given it mixed up with thin chocolate; which, if sufficiently sweetened, disguises it better than any thing I know of.

What the *bark* might do, as preventive, or preparative for this disease I know not; but in the latter intention I should think it would prove of use.

I suppose I need not take notice, that when I recommend the *bark*, I would not be thought to decry or discountenance the methods or medicines usually ordered in this distemper; on the contrary, I think, in so terrible a situation, nothing ought to be omitted which can any way alleviate or assist the patient. Dr *Monro* has already made proper remarks on this head, and to his words I refer the reader.

II. 1. This gentlewoman had never had the small-pox that she knew of, and was accounted by her relations likewise not to have ever undergone that distemper. In *Feb. 1700-1*, she was big with child, and within about a fortnight or 3 weeks of her full reckoning, when the following accident happened. A poor widow woman, who lived in a lonely cottage in the neighbourhood, was seized with the small-pox, and had nobody to assist or nurse her; the country people, as much afraid of this distemper as of the plague, would neither send her necessaries, nor suffer her to come to their shops to buy: wherefore in this extremity she made shift to get to this lady's house, who was noted for her goodness to the poor, especially for giving them medicines when sick: her business was to entreat the lady to desire her husband to use his authority with the overseers of the poor to appoint a proper nurse to attend her; for that otherwise she must certainly perish for want of necessaries, for even the parish-officers would not go near her. She expressed a very earnest desire to speak to the lady herself, who consented to go to a window, and spoke to her cross a court-yard at 30 or 40 feet distance, thinking herself safe from infection in that situation. She looked upon her without any surprize, but thought the sight very disagreeable, the woman having her face and arms full of a large distinct sort, in the state of maturation. About a fortnight after, *Feb. 25. 1700-1*, the lady was brought to bed of a fine jolly boy: in a day or two there appeared an eruption all over his skin, which was at first taken by the nurse for the red-gum, though the appearance was earlier than that disorder usually attacks children; but in a day or two more it shewed itself to be the confluent small-pox. The child was immediately removed from it's mother; but the distemper proved to be of the very worst sort, so that the child died before the turn: the mother took no infection, and lived to the year 1736, without ever having the small-pox.

The case of a lady, who was delivered of a child, which had the small-pox appeared in a day or two after it's birth; drawn up by C. Mortimer, M. D. & Sec. R. S. N^o. 493. P. 233. Oct. &c. 1749.

It is very suprising and wonderful to consider the different manners, in which children, while in their mothers wombs, are affected by vari-

ous accidents happening to the mothers. How the imagination only, affected by the disagreeableness of the sight, should convey the infection to this child in the case above recited, is, I own, what I am not able to account for; especially as there was no fright or surprize, and that the mother was under no apprehension of danger.

The above account is what I lately took down in writing from a daughter of the gentlewoman. Indeed many years ago I have heard the lady herself mention the accident; but I did not commit it to writing; but I think it was with this difference that she was surprized, and that the child was born with the small-pox upon it, in the eruptive state.

Some accounts
of the Fœtus
in utero being
differently af-
fected by the
small-pox; by
W. Watson,
F. R. S. Ibid.
p. 235. Read
Dec. 7. 1749.

2. That the human species should only once in their lives be liable to the small-pox, has long been observed with surprize, both by Physicians and Philosophers: nor is it less extraordinary, that the child before birth, which in every circumstance is equally supported by it's receiving and circulating it's mother's fluids, should be so differently affected by that distemper.

From the dissections of those who have died of the small-pox, we find that the *viscera* are subject to the variolous abscesses as well as the skin; but that the *fœtus* does not always partake of the infection from it's mother, or the mother from the *fœtus*, is the subject of this paper.

About 4 years since I attended a young man, a servant to a Carpenter, who had a very putrid and offensive kind of small-pox; of which nevertheless he recovered. His mistress, during his illness, came frequently into his room, and sometimes continued there a considerable time. She was then about 7 months gone with child, but had had the small-pox herself many years before. At the usual time she was delivered of a girl, whom I saw very soon after it's birth; and there appeared very plainly the marks of about forty pustules, in different parts of her body. From this appearance I then informed the parents, that I apprehended the child would hereafter be very secure from the infection: but as about a month ago the parents thought proper to have a little boy of theirs inoculated, I requested that they would permit the before-mentioned girl to be inoculated likewise. As I desired, they were both inoculated, from a child of my own, who had, from inoculation, had a favourable kind. Upon the tenth day after the operation the boy sickened, and had the small-pox, very favourably: about the same time the girl grew pale, and lost her appetite. This indisposition continued for 2 or 3 days, and then she recovered.

In both these children, the incisions, which were made only in one of their arms, were extremely superficial, and inflamed in both as usual: that in the boy produced the variolous fever and it's attendants, as is before-mentioned; but in the girl occasioned only a paleness and loss of appetite without a fever, and one variolous abscess in one part of the incision, such as is sometimes seen in nurses, and in those who have attended persons in the small-pox, who have had it themselves before.

This

This one pustule was a sufficient argument of the variolous matter taking place, and endeavouring to excite the usual symptoms.

Dr *Mead* in his treatise concerning the small-pox takes notice of a woman's attending her husband, who, a short time before she expected her delivery, was ill of the small-pox. As she had undergone the distemper herself a considerable time before, she felt no inconvenience therefrom; but upon her delivery the child was found dead, and it's body covered with the small-pox.

These two histories evince, that the child before birth, though closely defended from the external air, and enveloped by fluids and membranes of it's own, is not secure from the variolous infection, though it's mother has had the distemper before. They demonstrate the very great subtilty of the variolous effluvia; as we find them capable either from their floating in the air, and by their being taken in by the inspiration of the mother, or by penetrating the absorbent vessels upon her skin, and thus mixing with her blood, of exerting their effects upon the child: and we may observe further from the first of these cases, that it is possible for the child to live through the small-pox before it's birth; as well as that after that period under the before-mentioned circumstances it is not liable to the infection again.

The following history is equally remarkable with the preceding.

A lady of high birth and quality now living, well known to several members of this learned body, had the small-pox to a great degree when 7 months gone with child; notwithstanding which she went her time and was delivered of a son, who did not appear to have upon his body any marks of the distemper. As this lady had been severely handled by the small-pox, it was judged that her child would never after be liable thereto; nevertheless when about 4 or 5 years old, he was attacked with the distemper, but got very well through it, and is now alive.

A case in some respects resembling this last is taken notice of by *Mauriceau**, who delivered a woman of an healthy child at her full time, who during the fifth month of her gestation had had the small-pox to a great degree; although the child, from any marks of it's body, did not appear to have been affected with the distemper.

These cases are the very reverse of the former; where though from inoculation the most minute portion of lint, moistened with the variolous matter and applied to the slightly wounded skin, is generally sufficiently to propagate this distemper: yet here we see that the whole mass of the mother's blood, circulating during the distemper through the child, was not sufficient to produce it.

It generally happens, as we are informed by medical writers, and as I myself have seen in practice, that if women are delivered during the course of the small-pox, the distemper of the child does not keep pace with that of it's mother, but is subsequent thereto. Thus the child of

* See *Mauriceau sur les maladies des femmes grosses*, Case 576.

the lady, mentioned by Dr Mead*, who was brought to bed on the 11th day, when labouring under a very malignant small-pox, was born without any appearance of the pustules; but on the 4th day after it's birth the child was seized with convulsions, and died at the eruption of the small-pox. And in a woman, whom I attended, and who was delivered of an apparently healthy child upon the 9th day of a distinct small-pox, the child was not seized until the 8th day after it's birth, which is about the time that the infection would have taken place, if it had been received from any other quarter, independent of it's mother's having the distemper before it's birth.

From these histories it appears, that the child before it's birth ought to be considered as a separate, as a distinct organisation; and that, though wholly nourished by it's mother's fluids, with regard to the small-pox, it is liable to be affected in a very different manner, and at a very different time, from it's mother.

*A letter from Edward Wil-
mot, M. D.
F. R. S. and
one of His
Majesty's
Physicians &c.
to the Pres.
serving to in-
close the two
following*

papers: 1 Of the extraordinary effects of musk in convulsive disorders; by J. Wall, M. D. 2. Of the effects of the Tunquinese medicine; by Alex. Reid, Esquire. N°. 474. p. 212. June &c. 1744. Read Dec. 20. 1744.

*I. Of the ex-
traordinary
effects of musk
in convulsive
disorders; by
J. Wall, M. D.*

III. 1. Inclosed you have some observations upon the virtues of musk, from Dr Wall, an eminent Physician at Worcester. I have likewise sent you an account, I have procured from my ingenious friend Mr Reid, of the event of some experiments made by him with the same drug; which, being communicated to his friends, gave occasion, probably, to the liberal use of musk, of late years in practice here. As both these accounts well merit the attention of the public, I could wish they might be both printed in the same Transaction.

It seems highly probable, that the virtues of several articles in the *Materia Medica* are not hitherto fully discovered, or well ascertained, from want of due attention to their proper doses. The success of some medicines, we know, depends upon this; viz. That a determinate quantity be given within a certain space of time; so that if less than that be used, it seldom proves of use. Of this the *bark* is a well known instance; and Dr Hales has observed the same of soap †. It is not improbable, that the like might be found to hold good in many other medicines, were their operations carefully enough attended to: a discovery this, much to be wished for, as it would probably furnish us with a new set of specifics; and thereby enable us to cure several distempers, which at present, baffle our skill. Of the reasonableness of which conjecture the subject of this paper seems no contemptible instance.

Musk, and other perfumes of the same tribe, have been long celebrated as excellent antispasmodics; but, as they are usually ordered in

* Vide *Traët. de Variolis*, pag. 66.

† Account of some experiments on Mr Stephens's medicines.

very small quantities, they are seldom found to answer the expectations of the prescriber. The mistake seems to have taken it's rise from hence; that most persons imagining the medicinal virtues of these drugs to consist in their fragrancy; and finding that a very small quantity in substance would communicate it's smell to a very large vehicle; in their prescriptions therefore they have usually ordered so much only as was requisite to give the whole composition an agreeable perfume. But the case is widely different; for as a very small quantity of musk, civet, &c. when properly opened and diluted, will yield a much greater, as well as a more agreeable fragrancy, than a large parcel without such preparation: so the smell of the perfume is often found to be of disservice, where the substance, inwardly taken, produces the happiest effects; as *Etmuller* and others have observed.

Most of the writers on Pharmacy seem extremely cautious about the use of musk; and therefore direct it only in exceeding small quantities. In very few of their compositions does the dose of it reach so high as gr. i.; and the highest that I meet with is not above gr. v. *. But the *Chinese*, who are much better acquainted with the nature and uses of this excellent drug, order it in much larger quantities; the usual dose amongst them being $\frac{1}{10}$ of an ounce †. In the powder mentioned by *Dr James*, at the end of his treatise on the cure of the bite of a mad dog, which is said to have been brought from *China*, the quantity of musk in each dose is gr. xvi. and two of these are ordered to be taken at two hours distance.

The prescription last mentioned had, to my own knowledge, been given to several persons with great success: two in particular; the one an acquaintance of mine, a student at *Oxford*; the other || a servant-maid

* In *Fuller's Julap. Moschat. & Mistur. Moschat.* the quantity for a dose is not half a grain: In the *Pill. Solenandri & Syncopales* only two grains. *Bates*, in all the compositions where musk enters, orders it only in the quantity of half a grain to a dose; excepting only in the *Julap. Hysterie. Moschat.* He gives it to the quantity of five grains. In this quantity *Etmuller* once orders it in the like case. In the College, *Augustan*, and other Dispensatories, the quantity of musk in the great compositions, such as the *Conf. Alkermes; Elect. de Sassafras; Conf. de Hyacinth. Conf. liberans; Spec. laticans; Diarrhodon Abbat.; Diamb. c. od. &c.* is very trifling; in very few of them amounting to half a grain in a dose. *Schroder* makes the highest dose of musk four or five grains. *Lemery* but four grains. *Hermannus* gives five grains; but his commentator *Boecler* gives great caution about it's use: "*Circa Moschi usum* (says he) *caute procedere debemus; præsertim in mulieribus. Sanguinem impetuofum admodum reddit, & hæmorrhagias narium facile excitat.*"—And *Juncker, Consp. Therapiæ general.* TAB. 18. seems not a jot less fearful of it. For, after he has told us, that it is wonderfully extolled in nervous disorders, and many others, he adds—"*Sed notandum est internum moschi usum, nisi parçissimus sit, sæpe noxias humorum commotiones inferre; cæterum non adeo probatum, nedum tam gravibus plerisque malis superandis parem reperiri,*" &c. &c.

† *Vide Du Halde* on the physic of the *Chinese*.

|| This girl had been bitten about a year before the symptoms came on; and had, immediately after the bite, taken the usual precautions of being dipped in salt water, &c. She continued well for the interval; but, about twelve months after the accident, she began

maid to Mr Rogers, an innkeeper in this city. In both these the symptoms of the bite were come on; viz. an ichorous gleet from the wound, with extreme pain in the part; frequent convulsions, and *subfultus tendinum*; extreme anxiety, horror, and want of sleep. After the second dose of the powders, each of them fell into a gentle sleep, and broke out into an universal breathing sweat, both which continued for near 48 hours (except only whilst they were taking some refreshment); after which time they awoke perfectly well; their sores being healed, and they chearful, and free from all complaints. In all the persons (which are several) to whom I gave these powders, I constantly found them to have the same effects; procuring ease, quiet sleep, and a copious *diaphoresis*.

These surprizing and sudden effects I could not but attribute, in a great measure, to the musk (because we well know, that the cinna-bar alone never produces the like): and, from the safety and great success with which this large quantity had been taken, I saw little reason to be so scrupulous or fearful about it's use, as most authors appear to have been. On the contrary, I was rather induced to think, that it had hitherto been usually under-dosed; and that very great advantages might be expected from it, if given in an increased quantity. Nor was it long before I had an opportunity of trying it in another case, where the success justified my conjectures, and more than answered my utmost expectations.

Being called to a patient at *Bewdley*, a market-town in this county, I was desired by Mr *Malpas*, a very ingenious Apothecary of that place, to visit a poor man there, who had lain some time ill. I found him afflicted with a terrible hiccup, which had continued on him 4 or 5 days without a minute's intermission; so that he had had no sleep all this time. His pulse was exceeding quick, small, and unequal: The blood, which had been several times, and in large quantities, taken from him before I saw him, was covered with a prodigiously thick buff coat; his urine limpid; his face of a leaden hue, and ghastly: his extremities cold, and covered with a clammy sweat; and whenever the hiccup returned, which it did at almost every inspiration, the *hypochondria* were strongly convulsed and drawn upwards. Recollecting that *Fuller* recommends his *Julap. Moschat.* in the like cases *, but thinking the

began to be dull, and to have an extreme dread of the consequences of the bite. The part where she was bitten (which was the lower part of the cheek) began to give her great pain, attended with frequent convulsive catching therein: at last the wound broke out, and gleeted a thin ichor, with all the symptoms mentioned above.

* *In febribus mali moris, quæ vapores deliterios ad stomachum suffundunt; spiritusque labantes irritant; in confusiones & spasmos adigunt; & singultum inde excitant; nonnumquam præstantius novi remedium: aliquando enim divinitus succurrit, & singultum superat. Ut ingenue tamen fatear, tam funestum hoc sæpe notavi esse symptoma, ut neque hoc neque aliud quodcunque vel excogitare vel invenire, mihi adhuc datum sit, quod illi par esset; sed totis licet viribus certavi, fatis tamen frustra obvisus sum.*

Fuller. Pharmacop. Extemporan. sub tit. Julap. Moschat.
quantity

quantity of musk, therein prescribed, too small to be depended on in so desperate a case (more especially having before seen the surprizing success of a large dose), I ordered him a bolus composed of musk, nitre, crabs-eyes, \overline{aa} gr. xij. camphire gr. i. mithridate q. s. to be taken immediately, and repeated in 8 hours, taking a spoonful or two of *Fuller's* julap, occasionally, once in 2 or 3 hours. He had no sooner swallowed the bolus, which I staid to see him take, than his hiccup stopped: in less than an hour, he broke out into a general breathing sweat, and fell into a quiet sleep, which lasted 6 or 7 hours, when he awaked much refreshed. The hiccup never returned again, but the man soon recovered.

The next person to whom I gave this medicine was a gentleman, who had been ill 11 or 12 days of a petechial fever. I found him delirious with convulsive catchings in the tendons; and an hiccup very frequent and so loud, that I very plainly heard it at the bottom of the stairs, before I went into his chamber: his pulse was, at the same time, very weak, but exceedingly quick. I immediately ordered him a bolus with gr. x. of musk mixed with camphire *, as in the former case; and staid an hour by him to see the effect of it. In less than two minutes the hiccup left him; in about an hour his skin grew moist, and soon after he fell into a sound sleep. The hiccup as I was informed, returned again the next morning, though with much less violence; but both that, and the *subsultus tendinum* went off intirely, upon a repetition of the bolus, and never returned again; so that in a few days he was able to go out of his chambers.

In these persons the *hiccup* was the symptom that induced me to give the musk; but, observing that this convulsion, which, every one knows affords but a melancholy prognostic, was so speedily removed by it; and that, in the last-mentioned, the *subsultus tendinum* also gave way to it; I have since ordered it in many other convulsive cases, and always with success. I shall mention 2 or 3.

About two months ago, I was called to a daughter of one Mr *Carter*, in *Bewdley*. She was about 7 years of age, and had been ill of a fever (proceeding, as it was imagined from worms) for about 4 weeks. At the time I saw her, she had been senseless and speechless for several days; as I remember, not less than 10 or 12. A great part of this time she had had strong convulsions, and could scarce swallow any thing at all; so that they every hour expected her death. The Apothecary, told

* I added the camphire mixed with nitre, on account of the character given of it by *Hoffmann*; but, upon further experience, I found the musk succeed as well without it. *Hoffmann*, I find, touches upon the virtues of this drug, which he considers as a sedative: *'Sedantia hæc variis modis efficaciam suam perficiunt: — Quædam antispasmodica sulphure quodam tenui vaporoso ex regno animali; adeoque magis amico effectum persequuntur ut lumbrica terrestres, &c. nec non Moschus & Zibethum, quæ in convulsivis ac epilepticis motibus, ac in ipsa etiam chronica epilepsia, insigni cum fructu usurpantur.'*

Med. Rational. Systemat. tom. 3. sect. 2. chap. 7.

told me, that, the day before I saw her, she had had a general *tetanus*, her joints being quite inflexible: when I saw her, every muscle was, by turns, convulsed; her head, in particular, was so forcibly drawn backwards, as, at different times, to raise her body from off the bed. As she swallowed with so much difficulty, there was little hope of giving her any medicine by the mouth; I therefore ordered gr. xij. of musk, ground with oil and sugar to be mixed with some thin broth, and thrown up by way of clyster; and this to be repeated at 2 or 3 hours distance. In an hour's time after the first clyster, the convulsions, as I was informed, manifestly abated, and the second entirely removed them. After this the child gradually recovered its understanding and speech, and is now perfectly well.

About the same time, I was called to a young girl in this town, about 10 years of age, who had been seized with convulsions about 3 days before. At the coming on of the fit, she usually complained of a violent pain in her belly and loins, which, she said, seemed to draw her bowels on an heap: after this, she soon lost her senses, and was variously convulsed; not much unlike one in an epileptic fit. When the violence of the paroxysm began to abate, and she a little recovered her senses, she likened the pain to a sword running through her. From the first seizure, she had seldom had an hour's interval betwixt the fits. I ordered gr. xij. of musk to be given her in a clyster, which was thrown up in the decline of the paroxysm: upon this she instantly recovered, and has never been so affected since; proper care being taken to remove the *stimulus* which occasioned these spasms.

Mr *Malpas* tells me, that, since he has seen the good effects of musk he has twice tried it himself with great success. A gentleman's groom, having been kicked by a horse on the pit of the stomach, was thereby immediately thrown into a violent hiccup, with great difficulty of breathing, and incessant reachings to vomit, the stomach and diaphragm being greatly convulsed. After bleeding, &c. the musk removed the hiccups, &c. immediately. The other case is more remarkable: a farmer's wife in the country, as she sat at table, was suddenly seized with an inability of swallowing. She said, when the meat or drink came to a certain part of the gullet, she was immediately affected with the sensation of a tight cord round her neck, which seemed almost to strangle her, so that she was obliged to throw her food back again. As this was manifestly a spasm of the *oesophagus* and muscles of deglutition, he ordered her our antispasmodic, mixing ℥ij. of musk (as I remember) in a six-ounce phial, and ordered her to take this by spoonful. Several of the first doses she rejected, as she did other liquids; but, perceiving that the disorder gradually abated upon the use of this medicine, she persevered in it, and, at last, could swallow freely and easily as before. This single bottle completed the cure.

I could cite many more instances to the same purpose, both from my own practice, and that of the other Physicians of this place, to whom

whom I communicated my observations. One I cannot omit, which Dr Mackenzie has furnished me with. ‘Musk, (says he) given, according to your method, in a proper dose, that is from gr. vi. to sixteen, is the best antispasmodic medicine that I know. The efficacy of it in hiccups, arising from any nervous disorder, (and not from any wound or inflammation) is suprisingly great and sudden. Of many instances I have seen, I shall only mention one :

‘A young lady, reduced to an extreme degree of weakness, by a slow wasting fever, attended with a great disorder and lowness of spirits, had when I first saw her, the most frequent hiccup I ever heard, it returning with every inspiration. I ordered her a bolus with gr. vi. of musk, and the Apothecary stayed to give it. The moment she had swallowed it, she screamed out so violently, that she alarmed the whole house, and raised such an outcry, that Mr Holyoake, of Henly in Warwickshire, the Apothecary, with all his good sense, found it difficult to compose the tumult. From screaming she fell into a laughing fit : soon after she grew calm, and fell into a sleep, which lasted several hours. The hiccup ceased from the moment she took the bolus : it returned some weeks after from taking cold, but was quickly removed by the same medicine.’

Those who are acquainted with the learned Hoffmann’s writings, well know of what extensive use antispasmodics are ; and the instances here given, are, as I imagine, sufficient to prove, that *musk* is a medicine of *uncommon efficacy* in convulsive disorders. I can boldly affirm, that, when given in it’s due dose, it has never once failed my expectations. I have sometimes indeed been obliged to repeat the doses 3 or 4 times ; but it has always answered at last, in all cases where I had reason to expect it should. It is not to be imagined, that where the spasms proceed from any *stimulus* fixed in a certain part, that *this* or any other medicine can absolutely cure the convulsion, if the cause be not removed* ; yet I think it appears, from some of the foregoing histories,

* Hoffmann’s rules, regarding the use of specifics, are most certainly just, and must necessarily hold in respect to *this*, as well as any other ; ‘*monendum quod quum hæc ipsa (sc. specifica remedia) neutiquam viribus polleant absolutis, sed relativis, & certis conditionibus ac circumstantiis limitatis, in eorum usu harum imprimis exquisita habenda sit ratio, quo speratus respondeat effectus. Deinceps singularis efficaciae hæc quidem recte usurpata sunt remedia ; sed nihil certe juvant, nisi prius corpus fuerit præparatum ; & obstacula quæ virtutem infringere aut impedire possunt, remota. Hinc quando sanguis in venis redundat, & primæ viæ sentina sordium obsessæ, necesse est, ut sanguis prius subtrahatur & prima corporis regio a crudorum, biliosorum, recrementitiorum succorum saburra repurgetur ac depleatur.** Denique in specificorum usu accommo servanda methodus ; ita quidem ut artifex non solum tempus, dosin, ac regimen teneat ; sed etiam quamdiu iisdem insistendum, & quæ victus ac vivendi ratio, simul injungenda, exacte calleat. Etenim medendi methodus specialissima, quæ unice in medentis prudentia, judicio, diligenti attentione, meditatione, & observatione, naturæ ægrotantis consistit, tanti ad ipsum effectum est momenti, ut ipsis specificis & appropriatis solis longe sit anteponenda ; & sine ea, remedia quantumvis egregia, alienæ & infructuose sint operationis ; nec quicquam solidi in arte præstare possint*

tories, that, even without that, it may greatly alleviate them, and gain time for other remedies.

Under the quantity of gr. vi. I never found much effect from it ; but it succeeds best, when given to ten, and upwards. In the larger quantities, it never fails to produce a mild *diaphoresis*, without at all heating, or giving any uneasiness to the patient (how much soever it has been decried by some writers on these accounts) : on the contrary, it raises his spirits, and eases his pains. After the sweat breaks out, he commonly falls into a sweet refreshing sleep. Some persons I have given it to have observed, that their sweat is affected with the scent of musk. When it is taken in the quantities and manner ordered in the prescription mentioned by Dr *James*, the sweat it procures is very copious, and seldom goes off under 30 or 40 hours ; yet, though it continues so long, it gives not much fatigue to the patient, on account of the easy sleep and spirits which the medicine gives at the same time. In these respects, the operation of musk much resembles that of *opium* ; but is, in this, much preferable, that it leaves not behind it any *stupor* or languidness, which the latter often does ; so that it seems rather to approach to what is said of *oleum animale*. Musk, therefore, seems likely to answer in those low cases where sleep is much wanted, and opiates are improper.

I always chuse to give it in a bolus, if that form be not disagreeable to the patient ; because the perfume in that way is not near so strong as in any other : indeed when given in a large quantity, it is rather fetid than fragrant ; and I never yet met with any hysterical person, how averse soever to perfumes, but could take it in that form without inconvenience.

I cannot deny but I have once or twice prescribed musk, and not found the usual success after repeated doses ; but this, upon a more exact scrutiny, I found was owing only to some sophistication of that which had been used : for, after having tried 3 or 4 doses from one shop without success, upon sending for the same quantity as had been before ordered, from another person, whose musk I had before experimented, I found the very first dose answer as usual. Indeed it is much to be lamented, that a medicine of this consequence should be so liable to adulterations, and the *criteria* of it's genuineness so ill settled. It is therefore to be wished, that some Gentleman, who has skill and leisure enough, would oblige the world with an accurate *analysis* of it, and settle the characteristics of that which is genuine ; discovering, at the same time, the frauds and artifices, by which it is adulterated and counterfeited, and a certain method of detecting them : for, where the musk is not genuine, all that is hitherto said will prove but of little service, either to the Patient or Physician.

^a *possint. Et hæc quicumque studiose observat omnia, is certe desiderato potietur fine, quantum scilicet malo plus valet ars nostra salutaris.*

Med. Rational. Systemat. tom. 3. sect. 2. chap. 8.

About 15 years ago, I learned in *China*, that the *Tonquinese* had an II. *A letter* infallible cure for the bite of a mad dog; and, being very desirous of *from Andr.* possessing so valuable a *recipe*, I was, 2 or 3 years after, favoured *Reid, Esq;* with it by the late Mr *Hart*. *to Dr Wilmot,*

They take of the best musk about gr. xvi.; of the purest native cin- *concerning the* nabar, and finest vermilion, each about gr. xxiv.; and, having reduced *effects of the* them separately to impalpable powders, mix and administer them in *Tonquinese* about a gill of arrack; which, in 2 or 3 hours, generally throws the pa- *medicine.* tient into a sound sleep, and perspiration; if not, they repeat the dose, and think the cure certain.

As I had no room to doubt the fact, I began to consider attentively the symptoms of the distemper, and the nature of the remedy. The former seemed to proceed immediately from the irritations of the nerves by the acrimony of the juices; which, being constantly and violently hurried about, are, by that motion, and the heat attending it, broken, colliquated, and gradually rendered rancid, putrid, corrosive, and even caustic: in the mean time, the nerves, being more and more vellicated by the increasing sharpness of the humours, become proportionably more rigid and constricted; at once augmenting the velocity of the blood, and shutting up all the pores and passages of the natural excretions and secretions; while what should, but cannot, pass off by them, exasperates the disorder, till the juices become so corrosive and caustic, as to produce mortal convulsions.

Believing this theory to be just, so far as it goes, I readily concluded, that a medicine capable of relaxing the nervous system could not fail of relieving it from the above-mentioned effects of irritation, and thereby putting a stop to convulsions, opening the constricted passages of nature, moderating the velocity of the blood, and procuring sleep; imagining also, that, by the same soothing quality, the juices themselves might, not improbably, be rendered more mild and innocent when impregnated with the medicine. And such a medicine I judged musk to be, on account of its known, and almost instantaneous, effects on persons of a lax habit; whose nerves are so suddenly slackened, and the motion of their blood so diminished by the least smell of it, that many of them faint away: besides, its odour is so exceedingly subtile, as to penetrate through the closest substances; and may therefore be supposed easily to pervade the minutest vessels of the human body, and to diffuse its softening balsamic virtue thro' all the juices thereof.

The arrack seemed also a very proper vehicle for the musk; not only as they make together a very agreeable bitter, but also because inflammable spirits resist putrefaction, and also, in some measure, coagulate animal juices, which are not already corrupted; by which effects the too much rarefied blood is condensed, and hindered from putrefying further; while the bad juices, being separated from the sound, are plentifully thrown off by the passages, which the musk has relaxed, and opened for them. The native cinnabar seemed to be sufficiently recom-

mended by it's known uses in phyfic; against acrimony, obstructions, and convulsions: but of the vermilion I can only say, That tho' it be a preparation of the former, yet, as the *Tonquinese* seem to think it's virtue different, it were to be wished, that we knew their method of preparing it, in which they certainly excel.

After I had long considered and examined these principles in my mind, I satisfied myself, that they might justly be applied to many other cases; and that the medicine would be of especial service in malignant putrid fevers, and convulsions: and having, in the year 1739. contracted with the government for the transportation of convicts, I communicated my opinion to Mr *David Ross*, an ingenious and sensible Surgeon, whom I had employed to take care of their healths; and prevailed on him to make the experiment in a very desperate case; for I did not care to venture on any other. Please, in the mean time, to observe, that, as *Tonquin* vermilion was not to be had, I substituted an equal quantity of factitious cinnabar in it's stead; and sometimes gave rum or brandy instead of arrack; in other things I generally adhered to the original prescription.

CASE I.

In Dec. 1739. two convicts in *Newgate* were at the same time very ill of the putrid, infectious, malignant fever, commonly called the *Gaol Distemper*. All the usual methods of practice having been tried in vain, their condition appeared to be quite desperate. One of them died in the evening, and the other was not expected to survive till morning, being covered with flat petechial spots, and delirious. Mr *Ross* therefore administered to him the above described medicine about 9 or 10 at night; and, next morning, to his great surprize, found him quite free from the fever, eating water-gruel, and crying out for meat, after having slept well, and perspired plentifully. The spots on his skin rose, and the next day scaled off.

CASE II.

Soon after this, a convict, who had lived in good credit, laid his condition so much to heart, and drank so freely of spirituous liquors to drown his care, that he fell into a violent fever. He was on the master's side, where his relations looked after him; so that I heard nothing of his case till it was very desperate. He was delirious to a high degree, and had catchings in his hands and face. He took the above medicine at night, slept and perspired well, and next morning waked intirely free from his distemper, excepting that he had such a tremor left in his hands, that he could not carry a glass to his head; on which account I ordered him a second dose, and he was perfectly cured.

Encouraged by these successes, we administered the medicine to a great many other transports, who had the *Gaol Distemper*, and generally found it to have the same salutary effects; more especially where the patients were delirious or convulsed; as can be attested by Mr *Loutil*, Apothecary, who made up the medicines for my Surgeons, and was himself a witness of several surprizing cures performed by it: nor did I ever hear of any bad effects from it.

Mr *Ross*, the Surgeon above-mentioned, having caught the *Gaol* CASE III.
Distemper, by attending those who were sick of it on board, came ashore
at *Gravesend*, and desired me to provide another Surgeon; for that he
was so ill he could not go the voyage. He was blooded, and took
gr. xvi. of musk in a glass of rum, without the cinnabars, which were
not to be had ashore, nor easily to be fetched from on board. This was
at night; he slept, perspired, and waked about 10 in the morning so
well, that he went on board directly, and continued his voyage.

Encouraged by many instances of the first case, I ventured, about 3 CASE IV.
years ago, to give half the *Tonquin* dose (but without the spirits) to my
own child, then about 3 years old, who was seized with the small-pox
and convulsions. She slept sound, and perspired plentifully after it: the
small-pox rose kindly, and she did very well, having never had a single
fit of convulsion since, though extremely subject to frequent and dan-
gerous ones before.

About 2 years ago, Mr *Ross* gave much the same dose to my servant's CASE V.
child, who was then about 8 month's old, and had catchings in her
hands, occasioned by a violent teeth-fever. She slept, perspired, and
waked perfectly well.

About the same time, Mr *Gordon*, a Clergyman, who lived near CASE VI.
Greenwich, having come from thence to *London* with a fever upon
him, became quite delirious; if he was not so before he set out. Mr
Ross gave him the *Tonquin*-remedy, which made him sleep and perspire,
so that by next morning he was perfectly cured.

A Lady of my acquaintance, being very subject to violent hysterical CASE VII.
convulsions, was seized with a fit, as I sat at supper with her about 3
years ago, by which every part of her body was terribly agitated. Hap-
pening to have in my pocket a bolus made of the musk and cinnabars,
I bruised it in the palm of my hand, and clapped it to her nose, which
suddenly relieved her. I left it with her, and she has since told me, that
she never goes abroad, nor to bed, without it.

Observing the efficacy of this medicine in curing *deliriums*, I con- CASE VIII.
ceived that it would be of use against maniacal distempers; and happen-
ed, about two years and a half ago, to say so, in the presence of a Gentle-
man of *Oxford*, whose son had been, for some time, exceedingly disorder-
ed in his senses, by a disappointment in love; being unable to sleep, re-
fusing sustenance, and attempting to throw himself out of the windows
of a high room where he was confined. The father begged me to give
him the *recipe*, and assured me he would make use of it, as the methods
formerly tried had proved unsuccessful. He soon returned me a letter
of thanks, acquainting me, that the medicine had made his son sleep
sound for 23 hours, that he had perspired plentifully, and waked in his
senses. I have since heard, that he continues well, and, from a skele-
ton, is grown fat.

A particular friend of mine went mad about 1 $\frac{1}{2}$ year ago, by too CASE IX.
intense thinking. I mentioned the preceding case to Dr *Armstrong* and
Mr.

Mr *Ferguson*, who attended him ; and, with their approbation, gave him musk, native and factitious cinnabar, of each a scruple, in about a gill of arrack. In about 3 hours, he fell, or seemed to fall, asleep ; upon which, supposing the medicine, had taken effect, we left him : but, soon after we were gone, he waked ; and, next day, seeming very little, if at all, better, was removed to a private mad-house. There nothing else was done to him ; but at night he slept tolerably well, appeared much better next day, and continued mending, till he was in a little time quite well, as he is now. How much of this cure may be attributed to the medicine I do not know, as it did not operate immediately, nor in the usual manner. I mention it chiefly to shew, that even gr. xx. of musk had no *bad* effect upon him, if they had not a *good* one.

CASE X.

Not long after, Mr *Louttit* and I gave the same dose to a Gentlewoman, whose brain had long been turned by religious terrors, which first affected her about the time that her *menfes* ceased. I was, at first, surprized to find her suddenly become quite gentle, obliging, and reasonable ; but these good effects, as she slept but little, went off next day, and she soon appeared neither better nor worse than she was before ; and in the same condition she still continues : so that neither in this case did the large dose of musk do any mischief.

These are some of the many experiments made with the above recited prescription by myself, and the Surgeons under my direction. You may depend on the facts as near as I can recollect them ; and if you desire to be further satisfied, Mr *Louttit*, my Apothecary, and Mr *Mackenzie*, one of my Surgeons, now in town, shall wait upon you. Give me leave only to observe, that, where I thought the case required it, I have given as far as gr. xxiv. of musk to convicts, and never found any ill effects from it, tho', on some occasions, it disappointed my hopes. Whether there was any error in altering the *Tonquinese* proportions, the cause of which I cannot recollect, or whether the medicine would have succeeded in the instances where it miscarried, had the original prescription been kept to, and repeated as directed, I leave to your judgment and experience.

But I cannot help asking your opinion and begging you to consider, whether, upon the whole, the the virtues of this medicine may not reasonably be thought to extend to many other cases ; and particularly to the epilepsy and plague. But, whatever judgment you may form of the medicine which common good-will to my fellow-creatures obliges me to recommend to your thoughts, I intreat, and hope, you will pardon my presumption in going so far beyond my own province.

A remarkable instance of the happy effect of musk, in a very dangerous case ; by James Parsons, M.D.

2. Mr *Darlington*, a man of a robust habit of body, was taken ill about the beginning of Dec. last of a *rheumatic fever*, attended with the loss of the use of his limbs, excessive pain in every part, and swellings in his knees and hands, with all the other symptoms usual in this kind of fever : besides which he coughed up grumous blood, and had a pain which was very violent in his right side, from a fall against the edge of a table,

table, a few days before he was seized with this fever, which rendered his case the more dangerous. F. R. S. N^o. 478. p. 75.

During the first 10 days he was attended by his Apothecary only; who, finding him grow worse, proposed a Physician, who was accordingly called in, and ordered a bleeding, with such other remedies as he thought necessary: this, I understood, was the second time he had been let blood. But the patient by this time was so averse to taking medicines, that he did not duly follow the method ordered by this Gentleman, and at length absolutely refused to take any more, and so discharged the Doctor on the 4th day of his attendance. Jan. & Feb. 1746. Read Feb. 20. 1745-6.

On the 15th day of his illness he sent for me, having been formerly concerned for some of his family. I found his symptoms increased, and his disease gaining ground apace. I had him let blood immediately, which was $\frac{1}{3}$ size, and ordered it to be repeated, and put him under such a regimen as the state of his case then required; but as soon as I was gone, he refused to comply with my prescriptions also. I expressed my concern for his obstinate temper, and left him on my 4th visit.

Thus were eighteen days passed, and his case growing more desperate every day. I heard no more of him till 5 days after, which was on the 23d day of his disease, when I was intreated to visit him again, and found him most miserably afflicted with two of the most dangerous symptoms that can appear at the end of such a dangerous distemper; viz. a long *intermission* of his *pulse* every 3d or 4th stroke, and a most fatiguing *hiccup*, which struck him violently about ten times in a minute.

His case was now deplorable and desperate (thro' his own folly), and what I was almost ready to give up, especially as he was then very weak and delirious: however, willing to assist him, and calling to mind the accounts of the musk some time since communicated to this learned *Society**, was resolved to have recourse to it here. Accordingly I directed a draught, consisting of ʒiſs of strong cinnamon-water, ʒij of compound piony-water, and gr. xv. of *musk*, with orders to increase the dose of musk to gr. xx. and repeat the draught every six hours. I also told his wife to give him a glass of sack, as often as he would have it; and at the same time caused 3 blistering plasters to be laid on to his neck and arms, which, among other things, he had refused before. In 4 hours after the first draught the intermitting pulse was altered to a very calm regular one; but the hiccup continued with the same violence, till he had taken the 5th draught; and then returned only once in 6 or 7 minutes. His senses were now restored, and he grew chearful and easy, and said, he would take no more of any kind whatsoever: but, being unwilling to cease the exhibition of a medicine which bid so fair for his recovery, I gave him a glass of sack, into which the 6th draught was privately poured, which took away his hiccup intirely.

* See the preceding paper.

The next day he had an appetite to eat, and was indulged by his wife with a large chicken, a great deal of bread, and a pint of beer and ale, which he eat greedily: this overcharged his stomach, and brought on his hiccup again, which fatigued him much, before I visited him in the afternoon. Whereupon I directed a purging draught immediately, which emptied him well, and conquered his hiccup, and every other bad symptom. Next day I found him well, limited his diet for a few days, with directions to repeat his purge once more, after 3 days; and in a fortnight he went abroad.

The blisters might perhaps conduce, in some measure, to do him service; but, as the man was so many days ill, and reduced to a condition very little, if at all, better than that of a dying man, I believe the *musk*, rather than the blisters, was the medicine that restored him: for I have often seen the latter applied in a greater number in vain, even when the symptoms were not so seemingly desperate as in the case before you.

He took near 105 grains of musk in about 30 hours; but I can't say he either slept or perspired more than ordinary upon it.

The case of a person bit by a mad dog; communicated to the Pres. by Mr Ranby, F. R. S. Serj. Surgeon to His Majesty; from Charles Peters, M. D. F. Coll. Med. Lond. N^o. 495. p. 257. Jan. &c 1745. Read Jan. 24 1744-5.

IV. *John Neale*, of a robust constitution, aged 45, had, for some years, followed the occupation of curing dogs: and, on *Thursday* preceding *Mich.* day 1741, being employed in that calling, he, endeavouring to drench one supposed to be mad, was bit in the thumb. The day following the dog was observed to droop, refuse his food, and at night he died.

The patient, having been frequently conversant with the like accidents, was sufficiently alarmed at the danger; and, having been, the year before, received a patient into *St George's* hospital, repaired thither for relief.

Dr Hoadley attending, in the room of *Dr Baily*, the Physician of the week, directed him to have the wound scarified, be blooded, use the *pulvis antilyssus*, and cold bath.

About a fortnight after the accident, being the full of the moon, his symptoms became so violent, that I was desired to meet my brethren in consultation. I found him sitting on a bed, with one of his legs tied to the post; and, upon inquiring how he came placed in that posture, he told me, he himself fastened the cord, apprehending he might grow mischievous: and, upon our proposing to him to put on the strapped waistcoat, he readily consented to it; expressing great dread of becoming hurtful. He told us, he had felt a numbness in the wounded thumb, which shot up his arm to the shoulder; and that he was not sensible of having taken a moment's rest since the accident had happened; and that, upon endeavouring to compose himself, he fell into startings, and dreadful apprehensions of mischief from dogs. His eyes looked wild, and he complained of an excruciating pain in the head. For some days past he had been troubled with a difficulty in swallowing:

I pro-

I proposed to him to get down a small piece of bread; but he seemed to refuse it with great abhorrence: however, being encouraged to make use of his resolution (which he possessed to an extraordinary degree), he forced it into his mouth; where holding it for some minutes, he endeavoured to swallow, but was seized with violent spasms, beginning at the bottom of the *abdomen*; which, by a convulsive progression, heaved itself into the *thorax*; from whence the spasms were extended to the *pomum adami*; when the patient fell into strangulation, and afterwards privation of sense. Upon recovering from the fit, as we perceived, that, notwithstanding these obstacles, he had swallowed the bread, allowing him time to rest, we proposed to him to endeavour to swallow a spoonful of liquid, which he seemed most shocked at: he answered with fierceness, that he could not away with drink: however, upon recollection, he said, he would endeavour it: and, taking a spoonful of ale-house drink into his mouth, he was instantly seized with convulsions, beginning from the bottom of the *abdomen*, and ascending with great violence to the head, till he fell into a fit of longer duration than the former: however, he swallowed the liquor; and, upon his recovering his senses, he pointed with great vehemence to his arm, signifying, that he desired to be bled; from which, he afterwards told us, he had before found relief.

As his pulse was extremely hard, we directed the Surgeon to take away ℥xvj , which proved *ad deliquium*: however, that soon passing off, his pulse still continued hard, his flesh hot, with grievous complaints of the pain in his head.

Upon considering his case, as he had received no relief from the ordinary treatment of this distemper, and that his symptoms were now become highly inflammatory (blood fizy, and flammeous urine), we resolved to lay aside all thought of infection, and to betake ourselves to the method of cure in inflammatory fevers; with this difference only, that, as he had passed some days without stools, he was directed to take an *enema*, immediately: and then the following *bolus*; *Nitri purif.* ℥ss . *Confect. Mithridat.* ℥i , *sexta quaque hora, addendo dosi vespertin.* *Extr. Thebaic.* gr. i . ℞ *repetatur tertia quaque hora (nisi interveniant symptomata), donec concilietur somnus: epispastica applicentur brach. ℞ crurib. intern.*

Upon visiting him in the morning, the nurse informed me, that, after having taken two *bolus*'s, he had slept about $\frac{1}{2}$ an hour, to his infinite refreshment. His blisters discharged plentifully; his mind more composed; and his horrors were so far mitigated, to swallow half a pint of ale at one draught, though not without repugnance. He still complained of living in a flame; his eyes ready to start out of his head, where his pain still remained acute, not without numbness in the diseased arm; inquietude; difficulty in swallowing and respiration. *Mittatur sang. e br. ad ℥xij ℞ pergat in usu præscript..*

The night following he took two bolus's, and slept near 3 hours. The symptoms appeared less violent the next day, but still threatened mischief. *Applicentur cucurbit. occipit. ad extract. sang. ℥viij. Epispastic. laterib. colli, &c.*

His body was kept soluble with *manna*, and other cooling laxatives.

This method, with little variation (such as leeches, *pulv. sternutatorius, &c.*), was continued for the space of about 14 days, the blisters being kept open during the whole time; during which the symptoms gradually abated. He fell into languors, which were easily removed by the use of *assa fetida*, valerian, &c.

As he was now free from any disorders in his head, and his pulse beat with a natural softness, I advised him to return to the use of the cold-bath, *pulv. antilyss.* with a caution to bleed, and discontinue the use of them, whenever he found himself heated.

He is now restored to a tolerable state of health (except at the new and full moon): for, tho' he feels some alteration in the *quarters*, they are not so considerable; at which time his symptoms return in some degree; but so slightly, as not to prevent him from following his calling, which he has changed to selling of greens; not being intirely freed from the dread of dogs.

I had forgot to mention, that, during his illness, he voided so great a quantity of *saliva*, that his teeth, tho' naturally firm, became loose, and continued so, till the abatement of the complaint.

As copious bleeding is generally prescribed for the cure of this distemper, I shall make no other remark upon that advice, than by observing, that frequent recourse was had to it, to the apparent relief of the patient; who thought himself likewise much benefited by the *nitre* and *mithridate*.

As the difficulty of swallowing, in the present disorder, was evidently spasmodic, and infinitely abated by rest, tho' for $\frac{1}{2}$ an hour only, I submit to your better judgment, how far opiates may be conducive to the cure in this distemper; not only by giving them internally, but likewise by externally rubbing in such a quantity, as may seem reasonably calculated for the removal of spasmodic tensions.

During this illness, he complained of coldness in the extremities, with four eructations; so that, as soon as it was judged safe, he was directed to make use of a vomit; which was repeated many times with success.

I observed, when any liquid was offered him, he poured it into his mouth with uncommon hastiness: and, upon inquiring his reason for doing so, he told me, he had experienced, that, by throwing in a large quantity of liquid into his mouth at once, his faculty of swallowing became more easy: and that, whenever any hindrance happened in the performance, it was not without difficulty that he recovered himself.

I desire I may no way be thought to depreciate the efficacy of the *pulv. antilyss.* * and cold-bath; for I believe them more generally suc-

* Of *Dampier*, as altered by Dr *Mead*.

cessful than any other means: yet I think it clear in the above case, that they were so far from alleviating the complaints, that they tended evidently to promote them; the patient never making use of the cold-bath, but his head-ach increased, and his feverish complaints grew more violent.

As the *contra Rabiem* powder now stands in our *Pharmacopæia*, it is compounded of two drugs only. I have endeavoured to discover what effect might be procured by the *liverwort*; but, upon trying it, for experiment's sake, in several different cases, even in large doses, I could never perceive the least alteration either in the pulse, or secretions.

V. The most Rev. Father being turned of 50, but in his full strength, tall, fat, and very sanguine, indulging his appetite pretty freely living at *Milan*, where he was born, *Bologna* and *Rome*, was frequently seized, especially in *Aug.* and *Sept.* by a tertian fever, sometimes single and sometimes double, generally attended with vomiting and reaching. But it was commonly removed by bleeding, purging, and the bark: he was however often subject to vomiting. During the last years of his life he often complained some hours after eating, of a troublesome heat in his stomach, to relieve which he used morning and evening and both before and after his chocolate, to take a draught of water, sometimes cold, or if the season required it warm; he made it a rule, every year at the beginning of the spring, to purge gently and open a vein, to guard against the autumnal fevers. The last spring he grew very uneasy in his mind, dull, sleepless, and very thoughtful. In *July* he had the rash, but no fever; to cure which he drank plentifully of emulsions of pompion seeds, and water, sometimes pure, and sometimes a little sharpened with citron. On the 26th of this month he threw out the rash again, by heating himself with walking, but he was afterwards cooled very much, by rest and the free air. He was heated again by walking, and stopping by a fountain he was very much offended by a cold N. W. wind. He supped moderately; after which he was taken with shiverings, a fever, vomiting and reaching: but a gentle feverish heat came on, which was not at all troublesome, and he rested very well that night. The next day he was better; but the day after, his fever returned with shivering, vomiting, and a violent pain of the head. Dr *Camillus* found his fever declining, and no rash; at night a sweat broke out, and the fever went off. The next morning he took a laxative potion of manna and *cremor tartari*, with citron, according to the prescription of *Hoffmann*; after which he found himself better. His stools were partly bilious, and here and there stained with the colour of chocolate. At 18 h. the fever returned, with the usual vomitings, but declined in the evening. At night he took a bolus of *cort. peruv.* \mathfrak{D} ij; but on the fever's returning, he grew restless, and brought up the bark mixt with a viscid, tenacious lymph, a little stained with the colour of chocolate; the substance of which colour was

A short history of the disease which put an end to the life of the most Rev. Father Jos. Bolognini, Abbot of St Boniface, &c. extracted by C. M. Sec. R. S. from an epistle written by John Francis de Camillis, M. D to the Abbot Didacus de Revillas. N^o. 472. p. 40. Jan. &c. 1744.

afterwards discovered, in the course of the distemper to be blood, reduced in a manner to powder. The next day the pulse was low and quick; there came on a grievous pain in the left orifice of the stomach, and a difficulty of lying on the left side. It bore however the pressure of the hand, and both *hypochondria* were soft and pliable: he took another bolus of the bark, and soon after was taken with more violent efforts to vomit, which were quieted by drinking warm water. At 19h. he took ℥ij of bark in a draught of *aq. card. benedicti*: his *hypochondria* were fomented with sponges dipt in water, and he often took some oil of almonds. After noon his fever remitted, and he took ℥j of the bark in *aq. card. ben.* which was repeated after 4 hours. The next morning his discharges were stained with florid blood, the pain in his stomach, the inclination to vomiting, and the want of rest continued, with no small diminution of strength. A little before noon the fever insensibly returned, and his pulse became weaker and quicker. His inclination to vomit seemed to be increased, and he threw up a viscid lymph tinged with blood. Then he discharged both upwards and downwards a large quantity of coagulated blood. He took now and then the *confectio de hyacintho* dissolved in *aq. totius citri*. Afternoon an elder Physician was called into consultation, when it was resolved to bleed him, to repeat the potion of bark, mixt either with armenian bole, or sealed earth, and dissolved in plantain water, and to allow a moderate use of cordials. The bleeding was of no service, but rather made him weaker: therefore, to repair his strength, he took some veal broth with the yolk of an egg; after which he soon grew delirious. In 3 or 4 hours he lost his speech, and within 3 hours after that his head was covered with a cold sweat, after which he expired in great pain, on the sixth day from the invasion of the fever, and the fourth from his keeping his bed. The weight of blood discharged was near ℥ viij; of *cort. peruv.* taken ℥ss; of *ol. amygd.* ℥iv.

In 18 hours afterwards I undertook to open him; and found the body solid, and full of juice, the *abdomen* remarkable elevated, and the back variegated with some blackish spots.

The outer surface of the lungs, especially in the hinder part, was blackish: on farther examination they were found to have a preternatural tension and hardness, and the inner substance of them was void of blood, and of the colour of coffee. There was a small quantity of dark-coloured lymph found in the *pericardium* and the heart very weak and flaccid. The *foramen ovale* was open; the cavities of the auricles and ventricles contained a small quantity of black coagulated blood. The convex side of the diaphragm was inflamed, and the concave side blackish: and the blood vessels were turgid. The other parts of the *thorax* were free from fault.

On opening the *abdomen*, which was full of fat, we found the cawl pretty thick, but naturally formed. The convex side of the liver was nearly of the natural colour, but the concave side dark and black.

The

The inner substance was pale and faint, easily yielding to the knife, and almost void of blood. The substance of the spleen was blackish, and quite putrid; but contained in a sort of membranous purse. The kidneys, mesentery, &c. were almost void of blood. The whole internal surface of the *oesophagus* was unequally spread with black and coagulated blood. The stomach was swollen and inflamed. The cavity of it contained a great quantity of aqueous fluid, mixt with black fetid blood. In this fluid we found some drops of *ol. amygd.* floating. The vessels which interlace the stomach were turgid with blood and elevated, especially in that part where the *brevia* are placed. In the bottom, at the distance of about 4 fingers breadth from the *pylorus*, there was found, beyond expectation, a conspicuous tumour of an orbicular form, something like an orange, and weighing 3 v. it was elevated among the membranes of the stomach. We observed in it's more eminent part an orbicular perforation, about half the thickness of a finger in breadth. The tumour was of a dark red colour, of a sarcomatous substance, and of the carcinomatous kind. The whole length of the intestinal tube was red on the outside; and the small intestines were greenish. From the *pylorus* through the whole extent of the *duodenum* there was found a remarkable quantity of black coagulated blood. The short part of the *jejunum* was not stained with blood, and the rest of it, with the adjoining *ileum*, was filled partly with black and coagulated, partly with florid and reddish blood. The whole *colon*, and especially it's larger part, as also the *rectum*, were loaded with a putrid and fetid blood; but there did not appear any opening of the blood-vessels, in the whole system of the intestines.

Fig. 107.

Fig. 107. represents the natural magnitude of the tumour, and it's figure immediately after dissection, which grew flatter in 18 hours afterwards. *A* the perforation in the more eminent part of the tumour of an orbicular figure. *B, B, B*, the inferior part of the tumour strongly fastened to the bottom of the stomach, 4 fingers breadth from the *pylorus*, where it is filled with blood-vessels extended into it's body. *E, E*, part of the stomach expanded to shew the magnitude and figure of the tumour.

Fig. 107.

Fig. 108. represents a vertical section of the tumour. *A, G, H*, the winding cavity of the tumour from the perforation *A*, Fig. 107; descending obliquely into it's body, and dividing into 2 smaller cavities *HI, HK*. *IK* another larger cavity in the bottom of the tumour, through which there is a passage to the smaller ones. Here we found a small quantity of *ichor*, which by only touching gave a black colour to a silver probe. *MM*, the internal and sarcomatous substance of the tumour.

Fig. 108.

VI. In a woman of 40 I found the *vena cava* so enormously straitened between the origin of the left renal and the iliacal veins as hardly to permit any thing to pass thro' it. Some little quantity however of hard polypose blood was found in that cavity, which was compressed between the

Two observations of a diseased conformation of the

body found on
dissection; by
Albert Haller,
Prof. Med.

Gotting. and
F. R. S. N^o.

483. p. 527.

Mar. &c.

1747. Read

May 7. 1747.

Obs. I.

Obs. II.

the thick membranes. But the right spermatic vein was extraordinarily dilated, and being an inch in diameter supplied the place of the *vena cava*, and delivered the blood, which was excluded from its common passage, to the vein of the ureter, otherwise small in a sound body, and rising from the right iliac.

I have met with another example of this very rare coalition in *Jo. Rhodii Mantiss. Anat. Obs. xxi.*

It appears from both, that diseases may arise in the greatest trunks of the vessels of a human body, and that there may be a real obstruction in the veins; and that the very smallest canals, when the *impetus* of the blood, which is excluded from the ordinary passages, is thrown upon them, may be very greatly distended.

In a decrepit old woman, said to be 100, I found some marks of very great age, though I cannot affirm, that her years came up to the report.

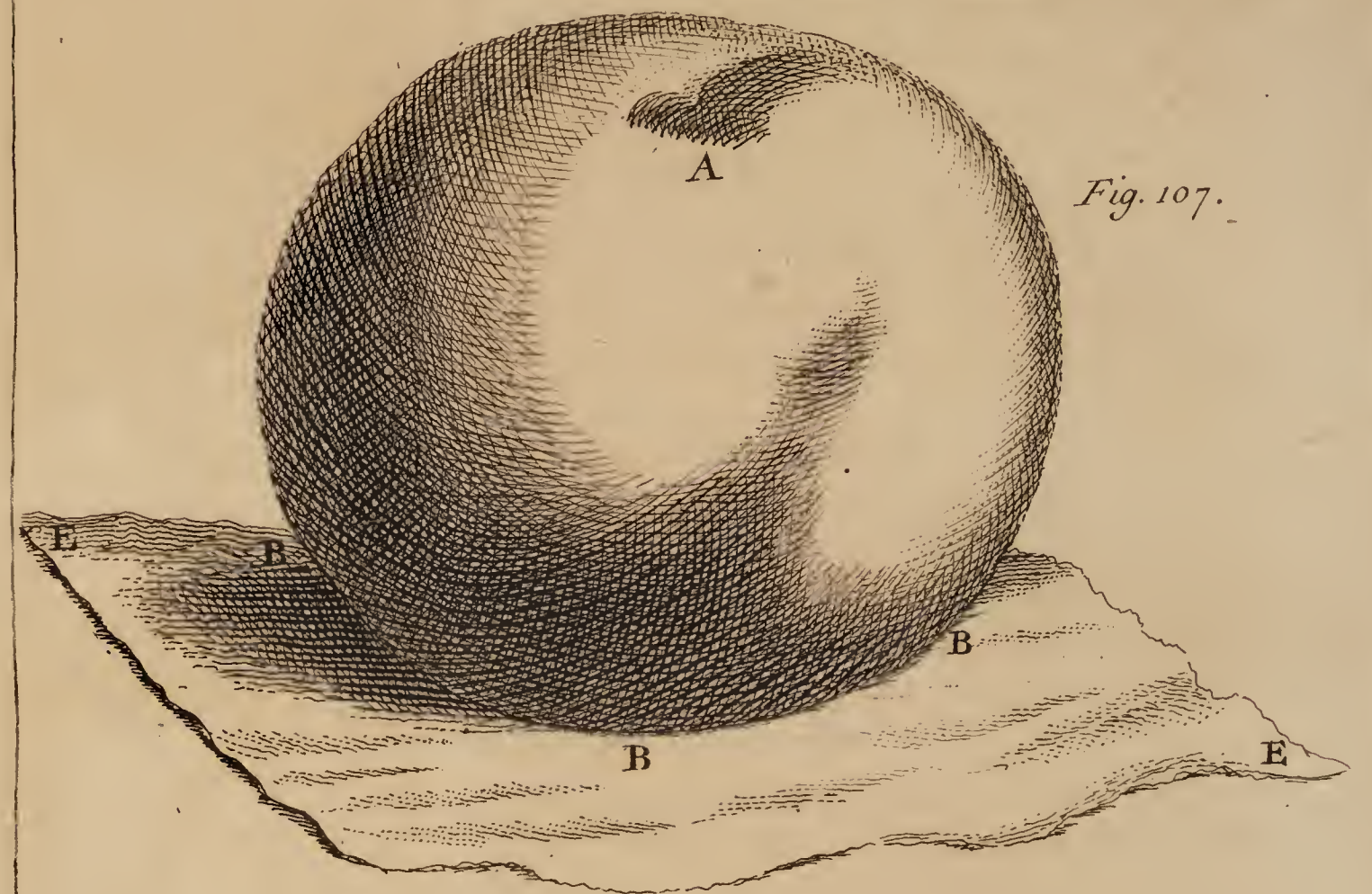
The whole fabrick of her body was found by the knife to be pretty hard. The conglobated glands were found, but had a firmness like the flesh of a kidney; the nerves were very hard; the cellulous texture was every where scarce divisible; the cartilages of the ribs were not quite osseous, except the upper one, where it joined with the *sternum*, and shewed hardly any difference, which is not very extraordinary in that rib.

But in the great artery there appeared many causes of her death. The *aorta*, where it proceeds from the heart, was very large, 5 ounces and 2 lines in circumference. When this was opened, there did not appear indeed any aneurism, but a very large *sinus*.

The arterial valves of the heart were partly indurated, and partly interspersed with stony tumours, much after the manner of *Cowper's* figures*; the other arterial and venal valves of the heart were hardly altered.

In the *aorta*, both at the heart, and in the *thorax*, and even in the abdomen, the internal membrane was every where torn, as if it had been scratched into several loose, fluctuating films, or wasted by some ulcer. These scales were generally osseous, and in some places stony, and heaps of tephaceous grains blocked up the mouths of most of the vessels rising from the *aorta*. The muscular membrane was found, as was also the outer part, so that the whole fault was in the inside. In the hypogastric, and iliac arteries, and those of the *pelvis*, and in those also which go from the *pelvis* to the *nates*, were a great many osseous crusts, but something flexible, into which the internal membrane of these arteries was changed; but yet the fleshy fibres adhered in such a manner, that calculous scales were frequently inscribed on the transverse ducts. Here, however, was nothing stony. In all the arteries of the body were hard and round *crassamenta* of blood, but yet less than their canal.

* *Myol. reform. tab. II.*



The gall-bladder was full of yellow bile, but hardly bitter, and had 20 small, angular stones, one of which so stopped the mouth of the *ductus cysticus*, being a little nearer to the *cholidochus*, than the first valve of the *cysticus*, that the bile, contrary to it's custom, could not be squeezed out of the gall-bladder. I generally find this sweetness in the bile, when it is concreted into stones.

These were chiefly what deserved observation, and they demonstrate that the internal membrane of the arteries, by the repeated strokes of the heart, partly harden, and are partly broken between the ossified spaces, and that so the trunk of the *aorta* is weakened, and rendered liable to aneurysms. It is also demonstrated, that a true calculous earth is every where carried round in the blood, and is not only deposited in the passages of the kidneys, but also stops there and is congested, where the smallest membranes of the vessels are bursten, and the attraction of the earthy *moleculæ* is greater to the rough unequal surfaces. *

VII. A man of about 22 years married a healthy young woman, much of the same age. Soon after he went to *America*, and at the end of 7 years returned cachectic, anasarous, and deeply tinged with the jaundice, endemical in hot latitudes. In a few months after his return his wife became pregnant (with her first child) of which she was delivered in due time. The child was born with a jaundice upon it, and died about 6 months after, under ascitical and icterical symptoms, which the mother had not the least impression. Soon after this (and before the husband, tho' much better, was quite cured) she became again with child, and after about 3 months pregnancy turned yellow, and was the whole time of her going with child, and some months after her delivery, deeply affected with the jaundice: but the child was born quite fair, white, and healthy, without any thing of that distemper on it; and is still living, and the last born.

VIII. The city of *Cartagena* in *America* is situated in $10^{\circ}, 25', 48\frac{1}{2}''$ N. Lat. The weather there is always sultry hot. A Thermometer constructed by M. de *Reaumur* gave, on *Nov. 19. 1735*, one of their winter-months, the degree of the warmth of the air $1025\frac{1}{2}$ divisions, and this with little variation, both night and day. The greatest height to which the spirit ascended at *Paris* the same year, by a Thermometer graduated in the same manner, was $1025\frac{1}{2}$; so that the heat of the cool nights at *Cartagena* was nearly equal to that of the hottest days at *Paris*.

As the heats in this climate are so great, without receiving any sensible mitigation from the nights, it is no wonder that the perspiration of the inhabitants is very great. From this it comes about, that all those

* This paper is reprinted word for word in N^o. 492. p. 172. and said to have been read June 8. 1749.

Extract of so much of Don Antonio De Ulloa's, F.R.S. account of his voyage to S. America, as relates to the distemper called there vomito prieto, or black vomit. Translated from the Spanish by W. Watson, F. R. S. N^o. 492. p. 134. Apr. &c. 1749. who Read May 4. 1749.

who make their abode there any time, appear pale and weakly, as tho' newly recovering from a fit of illness. You remark in all their actions, even so far as in speaking, a certain idleness, and, as our author expresses himself, a disjointedness: notwithstanding this they are in good health, though their aspect indicates the contrary. The people who arrive there from *Europe*, hold the appearance of strength and colour in their countenances during 3 or 4 months; but after that time they lose both one and the other from the quantity of sweat, until they become like the former inhabitants. These effects are most observable in younger people; on the contrary, those who are farther advanced in life, when they go thither, preserve their former appearance better, and enjoy so good a state of health, that they live commonly to more than 80 years of age.

As the temperament of this country is particular, so are some of it's distempers. These may be considered of two kinds; *viz.* those distempers to which the *Europeans* newly arrived there are liable, and *they* only; and those which are common to all persons, as well *Criollos* as *Chapetones*.

The distempers of the first class are many, as the resort of the *Europeans* there is very great. They are are very dangerous, and often mortal. They frequently destroy a great part of the people, both sailors and others, who arrive there from *Europe*. The continuance of these distempers is very short; they last but 3 or 4 days, in which time the sick either die, or are out of danger. The particular distemper, to which they are most liable, is very little known; though it takes it's rise in some from taking cold, in others from indigestion; but from whichever of these, or from what other cause it takes it's rise, it becomes in the short time before-mentioned the *vomito prieto* or black vomit, which is what kills them; it being very rare that those, who have it, escape. It is observed in some, that their delirium is so violent, that they are obliged to be tied down in their beds, that they may not tear themselves in pieces; and they often die raving with the greatest degree of agony.

It is to be remarked, that those only are subject to this distemper, who are lately arrived from *Europe*: the inhabitants of the country, as well as those who have abided there any time, are by no means liable to it, and enjoy perfect health during it's greatest violence. As the crews of ships are very liable to this distemper, and more so than the officers and passengers, who have greater variety of food and liquor, it has been conceived, that the great exercise and labour of these people, and their feeding upon salt provisions, prepares their constitutions to be liable in this climate to a corruption of the blood and humours, from whence is supposed to proceed the *vomito prieto*. What must be observed is, that although the crews of ships suffer the greatest slaughter, nevertheless passengers and others, who go the voyage under the greatest advantages, with regard to the conveniences of life, are not free from being exposed

exposed to it. It must be remarked also, that those persons, who, after having been used to this climate, go from thence, and are absent even 3 or 4 years, are not liable to it at their return, but retain their health like the other inhabitants; although in their way of living they have not observed the most exact regimen.

The desire of knowing the cause of this terrible calamity has occupied from time to time the minds of the Surgeons who make this voyage in the galleons, as well as those of the Physicians of the country; and their opinion has been, that it chiefly takes its rise from the labour to which the ships crews are constantly exposed, and their manner of living. There is no doubt but these may greatly contribute thereto; but then it will be difficult to conceive, why persons who are better circumstanced are likewise liable to it: and it is somewhat extraordinary, that, notwithstanding many endeavours have been made towards finding out remedies equal to this disease, none have been discovered, either as specifics, or preservatives; for the inconstancy of the symptoms is so great, that they are not in the beginning to be distinguished from those which are in common to this with slighter distempers; but the principal complaints are at first a weariness, and great disorder in the head.

This distemper does not always attack the ships of *Europe* at their arrival in the bay of *Cartagena*; nor is it very ancient in that country; for what heretofore was called *chapetonada*, so denominated, as those from *Europe* were only liable to it, were indigestions: and though they were in that climate always attended with danger, the women of the country, as they do now, cured them with ease, especially when they are taken in time. The ships afterwards going from *Cartagena* to *Porto Bello*, it was there succeeded with the great mortality, which was always attributed to the unseasonableness of the climate, and to the fatigue of the ships crews in unloading their ships, and in the business of the fair there.

The black vomit was not known at *Cartagena*, nor in its neighbourhood, until the years 1729 and 1730, when it first carried off a great part of the crews of the ships of war, which Don *Domingo Justiniani* then commanded, and were then there as *Guarda Costas*. These ships were first attacked at *Santa Martha*, where the severity of this distemper, and its great slaughter, had cast a great terror upon their crews. The second attack of this distemper was on board the galleons commanded by Don *Manuel Lopez Pintado*, when its mortality was highly formidable, and death followed the attack so quick, that persons, who were one day seen walking at large, were next day met carrying to their graves.

Our author is of opinion, that this, as well as some other distempers to which *Europeans* are liable at first, or soon after their arrival at *Cartagena*, and other places under the same circumstances, should be considered as arising from the great alteration that happens in their constitutions there: and this change, which from the climate is soon brought about, makes them suffer this and other distempers, which either de-

stroy them, or generate in them a disposition to bear the heats; after which, being as it were naturalized, they enjoy the same share of health with the natives.

Our author remarks, that at *Cartagena*, when the ships from *Spain* fail in their arrival, the *European* productions, which at all times are dear, and much valued there, are sometimes quite expended: these more particularly are wine, oil, and raisins. When this is the case with regard to wine, the people there suffer much in their health; as every body, except the negroes, and those who use brandy, accustom themselves to drink it with their food. From the want of this, their stomachs fail, they grow sick, and this sickness becomes general. This want of wine happened when our author arrived at *Cartagena*, and the sickness in consequence was so general in that city, that mass was celebrated only in one of their churches.

Extract of a letter from Mr B—B—r, containing an account, in pounds and ounces, of the surprising quantities of food devoured by a boy, 12 years old, in 6 successive days, who laboured under a canine appetite, at Black Barnsley in Yorkshire. Communicated by Dr Mortimer, Sec. R. S. N°. 476. p. 366. Apr. 1745. Read April 25. 1745.

IX. 1. The boy was regular as other children, till about a year ago, when this extraordinary craving of appetite, first began; which afflicts him to such a degree, that (they tell us) if he was not fed as he called out for it, he would gnaw the very flesh off his own bones; so that, when awake, he is constantly devouring; it can hardly be said eating, because nothing passes his stomach, all is thrown up again.

| ℥ | ʒ | Thursday. | ℥ | ʒ | Friday, | ℥ | ʒ | Saturday. |
|----|---|-----------|----|----|------------|----|----|-----------|
| 6 | 4 | Water | 3 | 0 | Rye | 8 | 0 | Milk |
| 2 | 0 | Milk | 2 | 0 | Milk | 1 | 4 | Bread |
| 3 | 0 | Rye | 0 | 4 | Sugar | 2 | 0 | Water |
| 0 | 4 | Sugar | 2 | 0 | Bread | 2 | 0 | Milk |
| 0 | 8 | Treacle | 4 | 0 | Milk | 4 | 8 | Bread |
| 1 | 0 | Bread | 2 | 0 | Water | 2 | 0 | Milk |
| 3 | 0 | Milk | 2 | 0 | Milk | 1 | 0 | Mutton |
| 0 | 8 | Butter | 1 | 8 | Meat-pye | 6 | 8 | Water |
| 0 | 4 | Sugar | 8 | 0 | Milk | 2 | 0 | Milk |
| 1 | 0 | Beef | 2 | 0 | Meat-pye | 1 | 12 | Rye |
| 0 | 4 | Bread | 4 | 0 | Water | 2 | 0 | Beer |
| 6 | 4 | Milk | 2 | 8 | Small-Beer | 1 | 4 | Pudding |
| 0 | 8 | Bread | 0 | 12 | Pudding | 0 | 12 | Veal |
| 6 | 4 | Water | 0 | 3 | Mutton | 0 | 4 | Cheese |
| 3 | 0 | Milk | 0 | 12 | Bread | 0 | 8 | Treacle |
| 1 | 0 | Apple-pye | 4 | 0 | Milk | 0 | 12 | Bread |
| 1 | 4 | Pudding | 2 | 0 | Water | 0 | 8 | Water |
| 1 | 0 | Veal | 2 | 0 | Milk | 4 | 0 | Broth |
| 0 | 8 | Meat-pye | 2 | 0 | Water | 2 | 0 | Water |
| 6 | 8 | Beer and | 1 | 0 | Bread | 2 | 0 | Milk |
| | | Water | 0 | 4 | Butter | 0 | 8 | Butter |
| 00 | 0 | | 00 | 0 | | 00 | 0 | |

℥ ʒ Thursday

℥ 3 *Thursday.*

00 0
7 1 Milk
3 0 Bread
3 0 Milk
3 0 Water
1 0 Bread
2 0 Milk
1 4 Beef
4 0 Small-Beer
1 0 Fruit

69 8

℥ 3 *Sunday.*

3 0 Rye
6 0 Milk
0 8 Sugar
6 8 Water
2 4 Bread
2 0 Milk
8 8 Water
4 0 Milk
2 0 Rye
2 0 Milk
4 0 Broth
1 8 Pudding
2 0 Beer
1 8 Mutton
6 8 Water
4 0 Milk
1 12 Bread
3 0 Rye
2 0 Milk
0 8 Butter
0 8 Sugar
2 0 Milk
2 0 Water
2 0 Milk
2 0 Beer
0 8 Bread
2 0 Milk
0 8 Fruit
2 0 Mutton

77 0

℥ 3 *Friday.*

00 0
0 8 Sugar
1 4 Meat-pye
2 0 Water
2 0 Milk
4 0 Milk
2 0 Beer
0 10 Bread
1 0 Fruit
2 0 Milk

61 14

℥ 3 *Monday.*

2 12 Bread
8 0 Milk
4 0 Beer
2 0 Milk
2 0 Water
0 4 Butter
0 8 Bread
5 0 Hasty-pud.
6 0 Water
1 4 Mutton
0 8 Bread
1 0 Beef
1 0 Potatoes
1 4 Pudding
6 8 Water
4 0 Milk
0 8 Bread
0 8 Treacle
0 12 Bread
2 0 Water
4 0 Milk
3 0 Potatoes
4 0 Milk

60 12

℥ 3 *Saturday.*

00 0
0 4 Sugar
4 0 Beer
1 0 Mutton
1 0 Veal
0 12 Fruit

58 8

℥ 3 *Tuesday.*

5 0 Bread
8 0 Milk
0 8 Butter
2 0 Water
4 0 Milk
5 0 Hasty-pud.
6 8 Water
0 8 Treacle
1 12 Meat-pye
1 0 Mutton
1 4 Pudding
6 8 Water
2 0 Beer
2 0 Milk
2 0 Water
1 0 Beef
1 0 Mutton
4 0 Beer
0 8 Sugar
1 0 Fruit

55 08 *Tuesday*

60 12 *Monday*

77 00 *Sunday*

58 08 *Saturday*

61 14 *Friday*

69 08 *Thursday.*

Salt 01 0 in the 6 days.

Total 384 2

A letter from
J. Cookson
M. D. to Mr
Latouche at
LittleChelfey,
concerning the
boy who has
an extraordi-
nary Bouli-
mia, or cra-
ving appetite:
communicated
to the R. S. by
J. Martyn,
F. R. S. Prof.
Bot. Cantab.
Ibid. p. 380.
Read May 9.
1745.

2. *Matthew Daking*, a healthful and sprightly boy, about 10 years old was, about 15 months ago, seized with a fever, which continued above a fortnight. In the beginning he had frequent provocations to vomit, which induced his Apothecary to give a gentle vomit of *ipeca-cuanha*. The reachings continuing, he gave him another: they seem-
ed to operate well, but yet did not answer the end in settling his stomach: however, the fever gradually went off, but the vomiting rather increased, notwithstanding some other methods were used.

He then began to have a craving appetite; to satisfy which he was indulged in eating and drinking more plentifully, but always vomited most of what he had taken, almost immediately. His appetite kept increasing, so that, in a few weeks his eating was come to the pitch you now see it in.

Thus he has continued above a year. His urine and stools do not exceed those in health; so that he vomits most of what he takes in.

He has tried crude *Mercury*, and all sorts of medicines, and mineral waters. At present he looks pretty well in the face, and is chearful; but has lost the use of his legs and thighs, which are much emaciated. He is sometimes so hungry, that he says he could eat them all: he often wishes he were in the King's kitchen.

One pig was fed with what he had vomited, and was sold in the market: but the country-people, getting hold of the story, put a stop to the feeding of any more.

To account for the disorder I am much at a loss, so shall not trouble you with my conjectures.

By the journal annexed it appears that he eat the following quantities of various sorts of food both meat and drink, as is specified in the other journal, which I have given at large, in the preceding article.

| | lb | 3 | | lb | 3 |
|-------------------------|----|----|------------------------|-----|----|
| 1745. Apr. 4. the whole | } | 65 | Apr. 7. it amounted to | 76 | 12 |
| quantity amounted to | | | Apr. 8. to | 60 | 8 |
| Apr. 5. | | 60 | Apr. 9. to | 55 | 8 |
| Apr. 6. | | 58 | | | |
| | | | Total in six days | 371 | 10 |
| | | | Salt | 1 | 10 |
| | | | | 373 | 4 |

N. B. He died a few months after, quite emaciated. C. M.

An extract,
by Mr Paul
Rolli, F. R. S.
of an Italian
treatise, writ-
ten by the Rev.

X. The Countess *Cornelia Bandi*, in the 62d year of her age, was all day as well as she used to be; but at night was observed, when at supper, dull and heavy. She retired, was put to bed, where she passed 3 hours and more in familiar discourses with her maid, and in some prayers; at last, falling asleep, the door was shut. In the morning, the

the maid, taking notice that her mistress did not awake at the usual hour, went into the bed-chamber, and called her; but not being answered, doubting of some ill accident, opened the window, and saw the corpse of her mistress in this deplorable condition.

Four feet distance from the bed there was a heap of ashes, the legs untouched, from the foot to the knee, with their stockings on; between them was the Lady's head; whose brains, half of the back-part of the skull, and the whole chin, were burnt to ashes; amongst which were found 3 fingers blackened. All the rest was ashes, which had this particular quality, that they left in the hand, when taken up, a greasy and stinking moisture.

The air in the room was also observed cumbered with foot floating in it: a small oil-lamp on the floor was covered with ashes, but no oil in it. Two candles in candlesticks upon a table stood upright; the cotton was left in both, but the tallow was gone and vanished. Somewhat of moisture was about the feet of the candlesticks. The bed received no damage; the blankets and sheets were only raised on one side, as when a person rises up from it, or goes in: the whole furniture, as well as the bed, was spread over with moist and ash-colour foot, which had penetrated into the chest-of-drawers, even to foul the linnen: nay the foot was also gone into a neighbouring kitchen, and hung on the walls, moveables, and utensils of it. From the pantry a piece of bread covered with that foot, and grown black, was given to several dogs, all which refused to eat it. In the room above it was moreover taken notice, that from the lower part of the windows trickled down a greasy, loathsome, yellowish liquor; and thereabout they smelt a stink, without knowing of what; and saw the foot fly around.

It was remarkable, that the floor of the chamber was so thick smeared with a gluish moisture, that it could not be taken off; and the stink spread more and more through the other chambers.

It is impossible, that, by any accident, the lamp should have caused such a conflagration.

There is no room to suppose any supernatural cause.

The likeliest cause then is a flash of lightning; which, according to the most common opinion, being but a sulphureous and nitrous exhalation from the earth, having been kindled in the air, did penetrate either thro' the chimney, or thro' the chinks of the windows, and did the operation. All the above-mentioned effects prove the assertion; for those remaining foul particles are the grossest parts of the *fulmen*, either burnt to ashes, or thickened into a viscous bituminous matter. Hence no wonder the dogs would not eat of the bread, because of the bitterness of the foot, and stink of the sulphur that lodged on it. The impalpable ashes of the Lady's corpse are also a demonstration; for nothing but a *fulmen* could produce such an effect.

They

Joseph Bianchini, a Prebendary in the city of Verona; upon the death of the Countess Cornelia Zangari & Bandi, of Cesena. To which are subjoined accounts of the death of Jo. Hitchell, who was burned to death by lightning; and of Grace Pett at Ipswich, whose body was consumed to a coal. Ibid. p. 447. Read June 20. 1745.

Remarks.

They say that there was not any noise ; but may be there was, and they heard it not, being in a sound sleep : besides, there have been seen lightnings and *fulmina* without noise ; as one may very often observe.

This is the whole *narration* ; after which I think proper to place what is said in the preface relating to it.

In the *Acta Med. & Philosoph. Hafniensia*, published by the celebrated *Thomas Bartolin*, 1673. Vol. II. p. 211. n. 118. one may see such another accident related in these very words.

“ A poor woman at *Paris* used to drink spirit of wine plentifully for the space of 3 years, so as to take nothing else. Her body contracted such a combustible disposition, that one night she, lying down on a straw-couch, was all burned to ashes and smoke, except the skull, and the extremities of her fingers.”

John Henry Cohausen relates this fact in a book printed at *Amsterdam* 1717, intitled, *Lumen novum Phosphoris accensum* ; and in the first part, p. 92. relates also, “ That a *Polish* Gentleman, in the time of the Queen *Bona Sforza*, having drank 2 dishes of a liquor called Brandy-Wine, vomited flames, and was burnt by them.”

Remarks.

Such an effect was not produced by the light of the oil-lamp, or of any candles ; because common fire, even in a pile, does not consume a body to such a degree ; and would have besides spread itself to the goods of the chamber, more combustible than a human body. It seems also, that it was not what is commonly taken for a *fulmen* ; for there was not left in the place any sulphureous and nitrous smell : there did not appear any blackish tracks on the walls ; all signs of the *fulmina*, as they have been remarked by the exactest observer of *phænomena*, Mr *Boyle*. But if it was not a real *fulmen*, it was certainly of such a nature.

Some thought, that in the ground under the room might have been a mine of sulphur : which granted ; what then ? I know, by experience, that in the very mines of sulphur have perished some of the miners, but only by suffocations caused by some sudden copious exhalation of kindled sulphur ; and never by having been burnt to ashes. The miners have informed me on the spot, that those of them who perished, have been only choaked by a strong nitrous and sulphureous *effluvium* ; but none of them by having been set on fire.

The author relates, that, going once out of curiosity into a sulphur mine by *Montefiascone*, when near the place from whence the miners digged out the sulphur, he was advised by one of them, who was carrying out his load, not to go farther ; for, either the smell, or some sudden exhalation, might have done him great injury ; and when got again in the open air, told him, a few days before 3 of his fellow miners fell stone-dead, while they were at work, by a violent suffocation, caused by a strong exhalation of bituminous smoke, which burst violently from the place where they were digging ; which misfortune was too frequent

in

in such mines ; but he never heard nor saw, that any of them had been burnt.

Thence it is concluded, that if the *fulmina* have such an effect, the *incendium* proceeds originally from their nitrous, and not from their sulphureous parts ; because the air, very closely imprisoned in the nitre, and not in the sulphur, either by it's own elasticity, or by some other agent being put in agitation, produces the flame of the *Fulmen*, which burns and consumes any thing to ashes.

I have, says he, seen the famous sulphur-spring, a mile distant from *Pozzoli*, mentioned by *Petronius Arbiter* : at the lower end of the plain there is a pit of liquid sulphur, whose boiling æstuations rise 10 or 12 feet. It's liquid matter consumes the flesh of any corpse, but does not affect the bones in the least. In our case the very bones were burnt to ashes ; and still the pavement was not damaged. No sulphureous smell remained in the chamber.

All this he advances, to oppose the opinion of an Academician at *Ravenna*, who insisted, That underneath that chamber must be a sulphureous mine. Which opinion he founds on this, That, in the very house, in a room near that the Lady was burnt in, there was set on fire a good quantity of hemp, and could not be found out by whom ; as also, that, all on a sudden, part of the palace had fallen, and not by any earthquake ; so that one might conjecture all this to be effects of the sulphureous mine under-ground ; which is not proved by those assertions. Nay, on the contrary, if there was a mine of sulphur, one should smell the stink of it in those dull days, when the nauseous south wind blows ; the sulphur mines then stinking at a great distance : besides, the effects of sulphur are not to reduce a body into impalpable ashes.

The fire was caused in the entrails of the body by inflamed *effluvia* The AUTHOR'S opinion. of her blood, by juices and fermentations in the stomach, by the many combustible matters which are abundant in living bodies for the uses of life ; and, finally, by the fiery evaporations which exhale from the settlings of spirit of wine, brandies, and other hot liquors in the *tunica villosa* of the stomach, and other *adipose* or fat membranes ; within which (as Chymists observe) those spirits ingender a kind of camphire ; which, in the night-time, in sleep, by a full breathing and respiration, are put in a stronger motion, and, consequently, more apt to be set on fire.

Fat is an oily liquid separated from the blood by the glands of the *membrana adiposa* ; and it is of an easily combustible nature, as common experience shews. Proofs.

Our blood is of such a nature ; as also our lymph and bile : all which, when dried by art, flame like spirit of wine at the approach of the least fire, and burn away into ashes. [*Observ. 171. in the Ephem. Germ. Anno X.*]

Such a drying-up of matters may be caused in our body by drinking rectified brandy, and strong wines; as Monsieur *Litre* observed in the dissection of a woman 45 years old, in the *Hist. R. Acad. Sc.* 1706. p. 23. Which effect may oftener happen, if the spirit of wine has any mixture of camphire: for that liquor is but a sublimated oil, whose sulphureous particles, being attenuated by the fermentation, when separated from fixed and salt matters, are easily put in motion, and, rolling thro' the air, become flame and fire.

Besides, although the salts which are in living and vegetable creatures are not naturally inclined to kindle; nevertheless they often contribute to it, particularly, when there is joined some strong boiling fermentation. It is from such a cause, that we know how the mixture of two liquors, altho' cold to the touch, produces a flaming fire.

Becher was the first discoverer of this marvelous *phenomenon*, by mixing oil of vitriol with that of turpentine. *Borrichius* afterwards did the same, by mixing oil of turpentine with *aqua fortis*; and at last *M. Tournefort*, by joining spirit of nitre with the oil of *sassafras*; and *M. Homberg* with this acid spirit, together with the oil and quintessences of all the aromatic *Indian* herbs: nay, *M. Homberg* asserts, that with a certain cold water cannons were fired, *Anno* 1710. in the abovesaid *Hist. Acad. Sc.* p. 66.

It is out of question, how, by a strong fermentation, magazines of gunpowder, barns, paper-mills, and haycocks, have been set on fire.

The acid particles in our bodies are much united with the fat and oily parts; nay, all our limbs abound with oil and acid. What wonder then, if they may kindle? as *M. Homberg* well observes, in the aforesaid *Hist.* 1712, 1717. from p. 13, to 31. where he takes notice, that all our limbs have abundance of fetid oil, and volatile salt, and therefore easily combustible.

We ought not to omit how the teeth are formed by so many short tubes, the bones by long ones, and easier therefore to be set on fire. *Malpighi* observed also, that the bones contain a fat oily matter.

Besides all this, we know that the *sebaceous glands* are spread all over the body; and that an oily moisture, with now-and-then a nitrous sulphureous smell, perspires from our skin; to which *Dr Blancard* ascribes the whole circulation. Abundance of combustible matter, shut up in a great number of cells, lies in the *omentum*.

There is further to be considered the vast quantity of *effluvia* that emanate from our bodies. *Sanctorius* observed, that, of ℥ viij. of food and drink in a day, there is an insensible perspiration of about 5; computing with them those *effluvia* which go out of the mouth by breathing, and which might be gathered in drops on a looking-glass. [*Seet.* 1. *Aph.* 6.]. As also, that, in the space of one night, it is customary to discharge about ℥ xvi. of urine, 4 of concocted excrements by stool, and 40 and more by perspiration [*Aphor.* 59.]. He teaches also, that numbness is an effect of too much internal heat, by which is prevented such an

an insensible transpiration; as we will shew in this very case. On this supposition I say, that the *effluvia* of such an insensible transpiration are an inflammable mine, easily apt to kindle, whenever a friction, be it ever so small, puts them in quick motion, and increases their velocity.

We acknowledge the discovery of this evident truth from Mr *Hauksbee*, F. R. S. in the experiment so much known of the *glass globe*, p. 30. to which I refer the reader. I saw this experiment at *Rome*; and altho' it seems that the light be only *phosphorus* produced by the *effluvia* coming out of the hand, and of the glass, it may nevertheless occasion further meditation on the present case.

The friction of the palms of our hands, or of any other parts of our body, may produce those fires commonly called *Ignes Lambentes*.

We learn of *Eusebius Nierembergius*, that such was the property of all the limbs of the father of *Theodoricus*: such were those of *Charles Gonzaga*, Duke of *Mantua*, as the celebrated *Bartolin* took notice of. By the testimony of *John Fabri*, M. D. a noted Philosopher, who saw it, sparkles of light flashed out of the head of a woman, while she combed her hair. *Scaliger* relates the same of another woman. *Cardanus*, of a *Carmelite* Monk, whose head continued 13 years to flash out sparkles, every time he tossed his cowl on his shoulders. *Ezekiel à Castro*, M. D. a famous *Jew*, and afterwards a *Christian*, wrote a little treatise, intituled, *Ignis lambens*; on the occasion that the countess *Cassandra Buri*, of *Verona*, when she rubbed her arms with a cambrick handkerchief, all the skin shined with a very bright light. *Eusebius* relates the same of *Maximus Aquilanus*. *Licetus* heard say by his father, that he saw the same quality on *Francis Guido*, a Civilian; and that he himself knew *Antony Cianflo*, a Bookseller in *Pisa*, who, when he shifted, shined all over with great brightness. *Libavius* relates the same of a youth; and *Cardanus* of a friend of his; saying, that, when he shifted, clear sparkles of fire shot forth of his body. Father *Kircher*, a Jesuit, relates, how he, going in company into a subterranean grotto at *Rome*, saw sparkles of fire evaporate from the heads of his companions, grown warm by walking. Father *Alphonso d'Ovale* was eye-witness on the highest mountains of *Peru* and *Chili*, how both men and beasts there seem shining with the brightest light from top to toe.

These flames seem harmless, but it is only for want of proper fuel. *Peter Bovisteau* asserts, that such sparkles reduced to ashes the hair of a young man. *John de Viana*, in his treatise intituled, *De Peste Malagenfi*, p. 46. relates how the wife of Dr *Freilas* Physician to Cardinal *de Royas*, Archbishop of *Toledo*, sent forth naturally, by perspiration, a fiery matter, of such a nature, that if the roller that she wore over her shift was taken from her, and exposed to the cold air, it immediately was kindled, and shot forth like grains of gunpowder *.

* *Pet. Borelli* gives an instance of such *effluvia* not only producing light, but likewise fire. See his *Obs. Cent. II. Obs. 75.* p. 174. where he says, That there was a certain peasant, whose linen, hempen thread, &c. if laid up in boxes, tho' wet, or hung upon sticks in the air, did soon take fire; which had been seen by a great number of spectators.

After all this I say, that a feverish fermentation, or a very strong motion of combustible matters, may rise in the womb of a woman, with such an igneous strength that can reduce to ashes the bones, and burn the flesh. Two such cases are known, one in the *Acta Medica & Philosophica Hafniensia*, Anno 1673. by the observation of *Matt. Jacobei*; and the other in *M. Marcell. Donato, de Medic. Hist. Mirab. lib. IV. cap. 25. p. 248. & lib. VII. Cosmog. c. 1. of Cornel. Gemma.*

I say also, that the bile, which is a necessary juice for our digestion, was observed by *Peter Borelli*, that, being vomited up by a man, it boiled like *aqua fortis*. [*Centur. II. Obs. 1. p. 109.*]

Besides, very strong fires may be kindled in our bodies, as well as in other animals of an hot temperament, not only by nature, but also by art; which, being able to kill, will serve for a better proof of my argument. It is necessary, for clearer instance thereof, to read the 77th observation of *John Pisano*, in the *German Ephemerides*, printed in *Lipsia* 1670.

Tie the upper orifice of the stomach of an animal with a string; tie also it's lower orifice; then cut it out above and below the ligatures, and press it with both hands, so that it swells up in one side; which done, let the left hand keep it so that the swelled part may not subside; and, with the right, having first, at an inch distance, placed a candle, open it quick with an anatomical knife, and you will see a flame there conceived, coming out in a few seconds of time: and such a flame may, by the curious, be perceived not only in the stomach, but also in the intestines. The first discoverer of this was *Andrew Vulparius*, Anatomy Professor at *Bologna* in *Italy* 1669. Thus you see, that a quick and violent agitation of spirits, or a fermentation of juices in the stomach, produces a visible flame. *Pisano* was an eye-witness of the above-related operation.

In the *German Ephemerides*, anno X. p. 53. of the continuation by *John Christopher Sturm*, one may read, That often, in the northmost countries, flames evaporate from the stomachs of those who drink strong liquors plentifully. About 17 years ago, says the author, three noble-men of *Courland*, whose names, for decency-sake, I will not publish, drank, by emulation, strong liquors; and two of them died scorched and suffocated by a flame forcing itself from the stomach.

The most celebrated *Borelli* relates how he was told, that a woman vomited flames in the point of death: you may read, says he, in *Bartholinus de Luce*, and in *Eusebius Nierembergensis* his history *Nat. peregrin.* how such accidents did often happen in great drinkers of wine and brandy: where is related also, how fire came out from the privy parts of a woman.

My Lord *Bacon*, in his *Nat. Univ. Hist.* assures, he had seen a woman's belly sparkling like fire; and truly such flames would often rise in us, if the natural moisture did not quench them; as *Lucretius* observes, from *Verse 868. Lib. IV. and Verse 1065. Lib. VI.* Moreover,
Marcellus

Marcellus Donatus, in his *Mirab. Hist. Medic. Lib. VI. Cap. IV.* intitled, *Of a new Distemper*, says, *Albertus Krantzius*, *Lib. V.* of his *Saxon* history, That, in the time of *Godfrey of Bologna* his Christian war, in the territory of *Niverva* or *Nivers*, people were burning of invisible fire in their entrails, and some had cut off a foot or an hand where the burning began, that it should not go further. *Ezekiel de Castro*, in the abovesaid work of his, of *Lambent Fire*, relates the famous instance of *Alexandrinus Megetius*, a Physician, who, from the *vertebra* of the *coxa*, after great pain, relates how fire came out, which burned the eyes, as *Simplicius* and *Philaseus*, eye-witnesses, did attest.

After all these instances, what wonder is there in the case of our old Lady? Her dulness before going to bed was an effect of too much heat concentrated in her breast, which hindered the perspiration through the pores of her body; which is calculated to about $\frac{3}{4}$ xl per night. Her ashes, found at 4 feet distance from her bed, are a plain argument, that she, by natural instinct, rose up to cool her heat, and perhaps was going to open a window.

The learned Marquis *Scipio Maffei* was told by Count *Atimis* of *Gorizia*, who passed through *Cesena* a few days after the accident, that he heard say there, how the old Lady was used, when she felt herself indisposed, to bathe all her body with camphorated spirit of wine; and she did it perhaps that very night. This is not a circumstance of any moment; for the best opinion is that of the internal heat and fire; which, by having been kindled in the entrails, naturally tended upward; finding the way easier, and the matter more unctuous and combustible, left the legs untouched; which may have been saved also, by remaining cut off at the combustion of the tendons, where they join with the knees. The thighs were too near the origin of the fire, and therefore were also burnt by it; which was certainly increased by the urine and excrements, a very combustible matter, as one may see by its *phosphorus*. *Galenus* (*Class. 1. Lib. III. de Temperam.*) says, That the dung of a dove was sufficient to set fire to a whole house: and the learned Father *Casati*, a Jesuit, in his *Phys. Dissert. Part 2. p. 48.* relates to have heard a worthy Gentleman say, That, from great quantities of the dung of doves, flights of which used, for many years, nay, ages, to build under the roof of the great church of *Pisa*, sprung originally the fire which consumed the said church*. After all this, the Author concludes, that to be sure the Lady was burnt to ashes standing; drawing the consequence from her skull fallen perpendicular between her legs; and that the back-part of her head had been damaged more than the fore-part, was because of her hair, and of the nerves, whose principal seat lies there: and besides, because in the face there were many places open, out of which the flames might pass; as it happened in the time

* Which effect is confirmed by *Galen*, *lib. II. de Morb. Diff. cap. 2.* where he says, That he hath seen pigeons dung take fire, when it was become rotten.

of the *Roman* Consuls *T. Gracchus* and *M. Juventius*, when a flame came out of a bull's mouth, without hurting the beast, by not finding any resistance to it's way.

Extract of a pamphlet, intitled, "Fire from Heaven burning the body of one John Hitchell of Holnehurst, within the parish of Christ-Church, in the County of Southampton, the 26th of June 1613." By John Hilliard Printed at London, 1613.

The manner of the accident is as followeth: he (*John Hitchell*), having been, on *Saturday* the 26th of *June* last, at work at the house of one *John Deane* of *Parly Court*, where he truly and painfully laboured at his trade, being a Carpenter, and having ended his day's work, went home to his house; and, after his coming home, betook himself to his rest; and, being in bed with his wife and child, in the deep of the night, the lightning came on so fiercely, that an old woman, named *Agnes Russell*, mother to the wife of the said *John Hitchell*, having received a terrible blow on her cheek (by what means I know not), was therewith awakened, and cryed to the said *John Hitchell* and his wife to help her: but they not answering, the poor old woman started out of her bed, and went unto the bed where they lay, and awakened her daughter, who was, upon the sudden, most lamentably burnt all on one side of her, and her husband and child dead by her side. Yet nevertheless his poor wife, when she saw her husband and child had thus strangely finished their days, she (as it seemeth) thought not so much of the hurt she had received herself, as she was careful to have preserved the life of her husband, if by any means possibly she could; and therefore (notwithstanding all her grievous wounds) she dragged him out of the bed into the street; and there, by reason of the vehemency of the fire, she was inforced, to her no small grief, to forsake him; where he lay burning upon the ground for the space of 3 days after, or thereabouts. Not that there was any appearance of fire outwardly to be seen on him, but only a kind of smoke ascending upwards from his carcase, until it was consumed to ashes, except only some small shew of part of his bones, which were cast into a pit made by the place.

An extract of the minutes of the R. S. of Nov. 8. and 15. 1744. concerning the woman at Ipswich, who was found burnt to ashes on April 10. preceding.

The first account of this extraordinary accident was in a letter from *Mr R. Love* to his brother *Mr Geo. Love*, Apothecary at *Westminster*, dated *Ipswich*, *June 28. 1744.* which was laid before the *Society* by the *Pres.* on *Nov. 8.* following; wherein *Mr Love* says, "That it appeared, upon the Coroner's inquest concerning the death of this woman (at which he attended), that she, having gone up stairs with her daughter to bed, went down again from her, half undressed; and that, the next morning early, her body was found quite burnt lying upon the brick-hearth in the kitchen, where no fire had been; with the candlestick standing by her, and the candle burnt out, with which she had lighted herself down; and that the daughter could assign no reason for her going down, unless it were to smok a pipe but said she was not addicted to drink *Gin*. The Jury brought it in *Accidental Death*."

Nov. 15. *Dr Lobb* communicated two letters concerning the same woman; one from the *Rev. Mr Notcutt* at *Ipswich*, to the *Rev. Mr*

Mr Gibbons; this dated July 25. 1744. and the other from the said Mr Gibbons to a friend, dated Sept. 2. following.

They both agree in all the material circumstances relating to the fact; both giving their relations from the mouths of the eye-witnesses, who viewed the body when it was first found burning; particularly Mr Gibbons from the woman's own daughter, and from 2 other persons living in the same house, whose names are *Boyden*. The case was this; one *Grace Pett*, a Fisherman's wife, of the parish of *St Clement's* in *Ipswich*, aged about 60, had a custom, for several years past, of going down stairs every night, after she was half undressed, to smoak a pipe, or on some other private occasion. The daughter, who lay with her, fell asleep, and did not miss her mother, till she awaked early in the morning, April 10. 1744. when, dressing herself, and going down-stairs, she found her mother's body lying on the right side, with her head against the grate, and extended over the hearth, with her legs on the deal-floor, and appearing like a block of wood burning with a glowing fire without flame; upon which quenching it with two bowls of water, the smother and stench thereof almost stifled the neighbours, whom her cries had brought in; the trunk of the body was in a manner burnt to ashes, and appeared like an heap of charcoal covered with white ashes; the head, arms, legs and thighs were also very much burnt.

It was said, that the woman had drank very plentifully of Gin, overnight, on the occasion of a merry-making, on account of a daughter who was lately come home from *Gibraltar*. But the difficulty is to account for the fire by which she was burnt; since there was none in the grate, and the candle was burnt out in the socket of the candlestick, which stood by her; and a child's cloaths on one side of her, and a paper screen on the other, were both untouched: and although the melting of the grease had so penetrated into the hearth, as not to be scoured out, yet they observed, that the deal-floor was neither singed nor discoloured; and the manner of the fire burning in her body is described as the working of some inward cause, and not from the burning of her cloaths, which were only a cotton gown and upper petticoat.

XI. A Gentlewoman aged about 21, of a quick and lively temper, beautiful, tall, and slender, of a complexion inclining to sanguine, enjoying in general a pretty good state of health, being for the first time with child, miscarried in the fifth month after conception. It happened unfortunately for her, that she was at this time in the house of a relation, at a considerable distance from her own family. In her necessity, the nearest midwife was called, who being very ignorant in her profession, and accustomed only to the rough country people, treated the *uterus* of this tender lady in such a manner, that there proceeded such a violent flux of blood from the torn vessels, that it was not in the power of art to stop it, till at last a most grievous fainting took away for the present both the motion of the fluids, and the force of the solids.

Of a cleaving
of the dia-
phragm, and
of the situation
of some of
the viscera
being altered,
observed in
the body of
a female child
of 10 months.
in a letter
from J.
Fothergill
M. D. to

R. Mead,

M. D. F. R. S.&c. N^o. 478.

p. 11. Jan. &

Feb. 1745-6.

Read June 16.

1746.

solids. She recovered however in such a manner, that by the paleness of her face and all her body, it was manifest, that she had suffered a great loss of her vital spirits.

From all these evils she had hardly recovered, when she was again with child. It would be hard to recount all the miseries that she suffered during this pregnancy, which exceeded all that are usually suffered by women in her condition.

She was delivered however of a daughter, at the proper time, whose diseases, death, and dissection, I shall now relate, taking leave of her mother, who was brought into good order.

The form of the infant seemed to be without fault; but she was small and lean: her skin being more flaccid than ordinary, and a little wrinkled, shewed that she had received her nourishment a little sparingly; which will not surprize any one, who has given attention to the history of her mother.

Her respiration from the beginning was a little too frequent, and in a few days, as if she had taken cold, she had a heaviness, with a great flux of a mucous discharge from her mouth, eyes, and nostrils, so that her breath was almost stopped, whenever she sucked her nurse. This would cause her to cry, and put her into greater passions than such little infants are subject to, and so violent, that those who attended her were often afraid, that they would have put a period to her life. But by the use of gently opening medicines she seemed to be in a manner relieved from the heaviness; and her strength increasing with her growth gave her parents some hopes of her out-growing her disorders. She was however subject to sudden vomitings more frequently than is usual: what she threw up had not the resemblance of food either crude or concocted; but rather of a sort of pulp, sometimes sour, and often fetid: her stools also were rather too frequent, and of the same sort. Her respiration in the mean time was frequent, hard, and in the night very difficult.

At length about the 7th month, some eruptions appeared, first in her face, then in her neck, shoulders, and extreme parts. They were round, broad like flea-bites, reddish and hardish, very troublesome by their itching, and by degrees rose into small bladders, filled with a thin transparent water; and at last ended in a scurf like bran, and left a reddish livid stain, which lasted a considerable time. These made their appearance after a slight fever of a few days. But as they observed no just rule either in their appearance or disappearance, and itched most violently, and succeeded each other in fresh crops, I judged that the disorder would be lasting, and had taken deeper root than to be carried off by a slight fever: and as the force and itching of the eruptions daily increased, I judged that the disease derived it's increase from some cause or other that vitiated the humours. I therefore had recourse to very mild purges, such as might blunt the acid or acrid humour, and dissolve the viscosity; and least the diet, tho' it suited the nurse very well, might

not

not agree with the tender child, I ordered both to be fed with broth, white meats, and such things as were very easily digested. Hence the number and itching of the eruptions were diminished, and the other disorders were so far diminished, that her doubtful state of health seemed now to be confirmed.

She was now entering into the tenth month, when her parents ordered her to be weaned. She endured it with little difficulty: but on the 6th day being a little costive, she took a solutive medicine, and having 2 stools she was very well. About evening she was taken with a violent vomiting, which did not arise from any apparent cause, but continued with hardly any intermission, till in about 24 hours it put an end to her life.

It is needless to relate the several medicines that were used in this troublesome disorder. The chief were tincture of rhubarb made with *Aq. Cinnam. ten.* often repeated in small doses, and a *Linctus* of *Ol. Amygd. Syr. peët.* and a very little soap. In her last extremities, were given to stop her vomiting such medicines as use to temperate and blunt acid and acrid humours; grateful, mild, and anodyne aromatics, and at last even soporificks, but all in vain: the body in the mean time was not naturally open, nor could it be loosened by any irritations whatsoever.

I was very desirous of inquiring strictly into the cause of this grievous disorder; and having obtained leave proceeded upon it the very next day: but before I proceed to an account of the dissection, it will not be amiss to recollect the symptoms from the beginning to the end.

1. The pulse was always intermitting, low, trembling, and very quick.

2. The respiration was always too frequent, and more difficult by night than by day.

3. It is also to be observed, that the respiration was more frequent in proportion to the looseness of the cloathing, even in the day-time; but nearer to what is natural, when the garment encompassing the *thorax* and *abdomen* was tight.

4. When she lay down the respiration was difficult; so that after she had taken her first sleep, the rest of the night was usually spent in sitting in the nurse's lap.

5. A cough, inquietude, and a slight fever, attended her more or less in the night, when she seemed to approach nearest to health, and were always more troublesome than in the day.

6. She was frequently troubled with vomitings, without any evident cause. What she voided was frequently crude and various, sometimes acid, sometimes putrid, fattish, or fetid.

7. Her stools were very like what she brought up.

8. She was unfortunately so exquisitely sensible, that the smell of flesh, howsoever prepared, would often cause most cruel vomitings.

9. The more she was costive, the greater disposition she had to vomiting: but a continual purging did her more harm than the vomiting.

10. The smell of the urine shewed it to abound with acrimony, being mostly like a sharp urinous spirit: the linen was tinged with yellow.

11. The skin also was sometimes tinged with the same colour.

12. The humour, which is usually discharged behind the ears of young children, was extremely fetid.

13. Her temper was mild, and patient; but, when provoked, her anger was violent, but soon over; otherwise she must have been suffocated.

I believe, that any one versed in medical affairs will, from what has been said, judge that this was no common disorder; that the force of the stomach was greatly diminished; that the secretions were interrupted, the blood vitiated, and that the lungs did but ill perform their office; perhaps also that the liver was not wholly without fault. But who would ever have guessed, that the diaphragm was split, and that a large portion of the stomach, and no inconsiderable part of the intestines had thrust themselves up thro' the fissure into the cavity of the *thorax*? Yet this and more appeared the next day, upon the dissection that I made with the assistance of Mr *Pigot*, an eminent Surgeon at *Wandsworth*.

The body appeared dry, and wonderfully wasted in the space of one day and night. The form and magnitude of the parts appeared tight, only the *thorax* seemed longer and narrower than usual; for the ribs were not placed so obliquely as usual, with regard to the *axis* of the body.

The left side was livid, as if it had been gangrenous; and a great many little bladders appeared about the shoulders and arms; filled either with a reddish, greenish, or yellowish liquor.

On dividing the skin, and the thin muscles that lay under it, the cellular coat was found entirely void of fat.

On opening the belly, the lower edge of the caul had not the least fat, and was scarce distinguishable. The liver was large and pale; the gall-bladder large and turgid, containing near 3iss of blackish bile, so thick and tenacious, that it would hardly be squeezed out thro' a large orifice. All the neighbouring parts were deeply tinged with yellow.

The *ileon* was here and there inflamed: the *colon* out of it's place: the spleen, kidneys, &c. sound.

After having raised the skin and the muscles, we carefully divided the *sternum* up to the throat. On opening this wound we were surprized with a view of what I believe was never seen before. A large portion of the stomach had forced itself into the left cavity of the breast, and entirely hid the lobes of the lungs and the heart in it's *pericardium*. On removing the stomach to see what lay beneath it, we found a part of the *ileon* about 3 spans long; the *cæcum*, and it's appendage, with no inconsiderable

inconsiderable portion of the *colon*, all under the same covering. Being moved at this novelty, I examined all over again, being afraid that we might have made a wound in the diaphragm our selves, and so have made way for the bowels to come thro'. But a most accurate inspection removed all doubt, and shewed us that it had been long divided: for the lips of the wound were quite callous, and there did not appear the least sign of any fresh injury.

Being now satisfied, that we had committed no violence on the diaphragm, we put all into their places again, in order to examine them carefully one by one. The transverse *septum* being torn in the fore part from the bone and cartilages of the *sternum*, and being divided and drawn quite back to the tendinous centre, had formed a tight lunar arch, the horns of which reached to the cartilaginous ends of the ribs, nearer to the *sternum* on the right side and farther off on the left: and thus there was formed an unequal *sinus*, more open on the left than on the right side. The *viscera* being raised through this ample *sinus*, found such a secure reception in the left angle of the *thorax*, that, in my opinion, they could never have been reduced into their former situation, by their own weight, or by strong shaking, or by any situation of the body whatsoever: I say *former*, because it is not easy to believe, that they were so situated originally before the birth. For there are many reasons to believe that this misfortune happened at the very time of delivery.

That bag of the stomach, which immediately receives the food by the *oesophagus*, was much larger than usual, and kept it's place, it was also paler and thinner than usual; the remaining part being thrust upwards, and then obliquely backwards, was elevated above the arch of the *septum*, and straitened in it's passage by the margin of the *septum*. When it had passed through these straits, it increased again into a pretty large bag, which rested upon the arched *septum*, and occupied the whole circumference of the left part of the breast. The little heart in the mean time, was thrust upon the spine of the back, and had straitened the lobes of the lungs on that side in such a manner, that we thought at first, that they were quite driven out of their place, or were reduced to nothing. The *pylorus* emerging from this bag near the third rib, and verging downwards with a sinuous flexure ended in the *duodenum*; which must also have been straitened, in it's descent thro' the *septum*, as often as the stomach was filled either with food or wind. And as it must necessarily have deviated from it's natural course, it is no wonder, if the common biliary duct, being straitened and almost closed by the strait course of the gut, could not always perform it's office: hence arose the discolouring of the skin, and sometimes of the urine; and the stools without colour.

In following the windings of the intestinal tube, we are again led into the cavities of the *thorax*: for above the diaphragm, and behind the stomach, a convoluted portion of the *ileon*, some spans in length was concealed: as we were searching for the end of it, we found a vermi-

form appendage, and a *cæcum*, distended with *fæces*; it was large, compressed by the incumbent bulk of the stomach, and thereby fitted to the extreme angle of the cavity: lastly about half the annexed part of the *colon* was remarkably full of deep sinuations. We found the *colon*, in that part where it passed above the straitened edge of the *septum*, almost cut off; for the stomach being often filled, and pressing the sides of the yielding gut against the margin of the resisting membrane, had so worn them, or straitened them, that it did not seem fitted for the passage of the thicker excrements.

When we had duly considered these things, we set about the examination of the other contents of the same cavity; and discovered the lobes of the lungs covered with a firm membrane, which we judged to be the left side of the *mediastinum*: and as they not only adhered on all sides, but grew firmly to it, they seemed to have not performed their office for some time. Besides a strong *plexus* of the fibres, as it were interwoven with the cellular membrane, went between the whole *compages* of the lobules, and the surface of the *pleura*, in such a manner, that the lungs were not able by any means to expand themselves, even tho' the intestines and portion of the stomach had permitted a free space of moving from the back. We then examined the cavity of the other side, and having perforated a strong membrane, which we took for the other fold of the *mediastinum*, the right lung appeared sufficiently sound and entire, and had without doubt for a long time performed the office of both. A little below the under margin of the lung, above the diaphragm, was a bag, which seemed to be composed of the cellular membrane, which being opened discharged about ʒij of a viscid, greenish yellow liquor contained in membranaceous pouches. Hence in all probability was derived that *sanies*, which had nourished those incurable tubercles, which had deformed the face, neck, and other parts; for the colour of the fluid in both was the same.

The *pericardium* was opened next, which was also full of a similar liquor; for there flowed from the aperture near ʒij of it, very like that above described, but a little thinner and yellower. The heart was small, hard to the touch, and divided into 2 nearly equal *loculamenta*: for on one side the right auricle, being distended with blood, was hardly inferior to the heart in bigness; on the other, the ventricles were stuffed with some black, grumous blood: between both were the coronary vessels binding the base of the heart very close. On opening the auricle a very black, thick blood, covered with a tenacious coat was squeezed out; the sides of the *saccus* itself seemed to be composed almost wholly of livid, varicose, little veins. In cutting the larger veins, I found less blood to flow out, than I had ever observed before: it was constantly thick and black, as if it had been quite deprived of *serum* and diluting lymph.

Having now examined the *viscera* in their proper situation, I had a mind to take out the stomach, and part of the intestines, in order to discover

discover how much they were altered as to form and situation: but as I gently touched the bottom of the stomach, I found it quite rotten, not able to endure the least touch, and the very lowest part was entirely mashed between my fingers. There were no signs of any gangrenous inflammation, no redness or streaks, or livid spots: all was whitish, and easily became rotten without any force; so that its texture appeared to me rather to be corroded by hot humours, or relaxed by moist ones, than corrupted by a gangrene. This will not seem absurd to any one, who considers the situation of the stomach. For from the bottom of this bowel to the upper part of the *pylorus*, the ascent was very steep: in our subject the force of the *septum* was taken away, by means of which alone the contents of the stomach could be raised upwards: wherefore more labour was required in the muscles alone of the belly, to perform what is done in others by the assistance of the power of the diaphragm. Hence also will appear, why she was worse when she lay down in the night than in the day, when her cloaths were loose than when tight: and how many disorders she must needs have had, whensoever the horizontal situation, and loose dress gave an easy ascent to the food into the breast. The heart itself must necessarily have been oppressed, and sustained an unequal conflict with its enemies.

After this history of facts, I believe the explication of the symptoms is so obvious, that I need not explain them. I shall only make the few following observations.

1. It is plain, that an animal may live, grow, be lively, and in some degree seem in health, when the diaphragm is split or bursten.

2. Hence there is the less reason to be afraid of perforating the *thorax*, when an *empyema*, or dropsy of that part, may require that doubtful remedy to be used.

3. It may perhaps be discovered, that the diaphragm is thus affected, if (1) there is any ample and gaping wound, by the form of the *thorax* being lengthened, by the situation of the ribs not being oblique enough, and by a bad respiration at the same time.

4. In the diseases of infants, whose nature may seem something recondite, perhaps some light may be struck out from an accurate observation of the external parts: and therefore it is by no means to be omitted.

5. Whensoever any tubercles, or eruptions of pustules occupy any particular region of the body, we may reasonably suppose the cause of the disorder to lie underneath, in its neighbourhood.

XII. In the neighbourhood of *Halifax* in *Yorkshire* live two brothers named *Stoneclift*, whose bulk and weight is very extraordinary: the eldest is a married man, and has several children; about 40 years of age. He weighed 35 stone, odd pounds; at 14 pounds to the stone; which we may reckon near 500 pounds weight. His brother weighs 34 stone odd

Of 2 men of
extraordinary
bulk and
weight; by
Mr Mark
Catesby,

F. R. S. N^o. 479. p. 102
Mar. & Apr. 1746. Read
Mar. 6. 1745-6.

odd pounds; but they make between them 70 stone, or 980 pound weight. As one was mounting an horse, the poor creature's back broke under him, and he died on the spot.

Of one, who had no ear to music naturally, singing several tunes when in a delirium; in a letter from the Rev Dr Doddridge at Northampton, to Mr H. Baker, F. R. S. N^o. 484. p. 596. Oct. &c. 1747. Read Nov. 12. 1747.

XIII. A Clergyman's Lady, whose husband is of some eminence in the learned world, in a frenzy after a lying-in, which was quickly removed, found, during the time of it, such an alteration in the state and tone of her nerves, that, whereas she never had before or since any ear for musick, nor any voice, she was then capable of singing, to the admiration of all about her, several fine tunes, which her sister had learned in her presence some time before; but of which she had not then seemed to take any particular notice.

A letter from Dr le Cat, F. R. S. to C. Mortimer, M. D. Sec. R. S. concerning the cure of dry gangrenes: together with a description of a new-invented instrument for the extirpation of tumours out of the reach of the Surgeon's fingers; translated from the French by Ph. H. Zollman, Esq; F. R. S. N^o. 491. p. 72. Jan. &c. 1749. Read March 9 and 16. 1748-9

XIV. From 1725 to 1733, when I frequented the hospitals of *Paris*, I saw that a great many persons there died of a dry gangrene, and even that nothing was done to them. I knew that amputation had been attempted in vain; but I did not see any of the practitioners endeavour to cure that distemper by remedies; nor did I think that cure impossible. I suspected, that the cause of the dry gangrene was the want of a flow of the arterial blood and of the spirits into the part affected: and this want, as it accounts for the deadness, which cannot miss befalling the part, and the dryness which attends this sort of mortification; whereas in the humid gangrene, it is chiefly the return of the blood which is hindered, and thereby the fluids are accumulated, that swell and distend the part.

Two causes so opposite must demand also very different cures. The distension which characterizes the humid gangrene shews the necessity there is for scarifications and evacuations; as the dryness of the other gangrene points out the uselessness of these operations in this sort.

In the humid gangrene the solids are choked up, and overflowed with the accumulated liquid; the nerves are there stupefied and benumbed: what can be more proper then after scarifications, which disgorge and relax those regions, than to apply stimulating *topics*? *Tonics*, which restore the spring, the tone of the solid parts; help them to expel those superfluous liquors, the spirits of which are as it were drowned and suffocated; and in short the defect being local, if those succours do not suffice, it is very common to cut off a limb; the loss of which may bring on that of the whole person.

On the contrary, in the dry gangrene the solids are void of fluids of all sorts. The neighbouring regions, which begin to share of that want, are affected with the most cruel pains: if you attack those parts with the cutting instrument, you increase the irritation of the solids, the constriction of the vessels, the want of fluids, the exsiccation, and hasten death.

death. On the contrary, the general indication, which this distemper affords us is therefore to soften, to relax the vessels, to draw thither the liquors by topics, whilst inwardly all remedies must be given that are capable of bringing the blood and the spirits from the center to the circumference. If this method is not successful, death is inevitable; for even supposing that the amputation was not liable to the dismal consequences just now specified, there is no room for this operation in an internal defect, which depends on the whole habit, as the case is with the dry gangrene. And supposing that there are dry gangrenes purely local, as the critical *depositums* of certain malignant fevers, you may assure yourself, that the very same nature which has caused this crisis, if you assist her but a little, will be able also to separate this mortification from the sound parts; and she will do it more gently and more dextrously than we.

These were the notions I had formed to myself of these two sorts of gangrenes; I only waited for opportunities to make the trials which this theory suggested to me. I did not find any before 1738. in the person of a wood-merchant of our town, called Mrs *Fournaise*. She was then 65 years of age, extremely corpulent; the gangrene seized her at the heel, by a black and round blotch, of 2 inches diameter, without any tumour, with some small scorbutic spots, great pains, and a little fever. The *plethora* made me begin with bleeding and purging; the latter I repeated every 8 days. I applied all over the foot and part of the leg, a pultis made of herbs and *farina's*, emollient, resolving, and aromatic, the suppurative ointment, and *storax*. I gave inwardly diaphoretic ptisans: in the morning, broths of vipers, of crayfish prepared with proper herbs, and above all with water-creffes: in the evening a bolus of *theriaca*. In short, I followed intirely the theory I had formed to myself about the dry gangrene, and in 9 or 10 days I saw the supuration formed; so that my patient was perfectly cured in about 2 or 3 months.

This success has been followed by many others, both in our town and in our hospital. I have been particularly successful in the latter place, because I am more master there than any where else, to see my method exactly followed, which I have still improved since 1738.

The report of these cures having spread itself as far as *Paris*, I was sent for thither in *Feb.* 1746. to attend M. *Rondé*, Treasurer-General of the fortifications of *France*, who was at the last extremity, by a dry gangrene, which had sphacelated his foot; the cause of which had kept him in a languishing way for 4 years. M. *Rondé* was in a dreadful condition, and I was vexed at having been sent for in so desperate a case: I declared it to his relations, and to the eminent Surgeons who attended him: I therefore applied my remedies to the patient, at the request of his relations, only by way of trial, which I publicly declared to be most doubtful. However, contrary to our expectation, from the 3d day there appeared plainly a beginning of supuration, and a separation

ration of the eschars. On the 9th there was a complete suppuration, and the regeneration of the flesh was even so far advanced, that the singularity of it raised the curiosity of some of the first Surgeons of *Paris*. At last, on the 15th day my patient found himself in a more favourable crisis; which, according to the testimony which M. *Morand*, an assiduous spectator of my cure, publicly gave of it, gave hopes of recovery in a case that had hitherto been thought desperate. The suppuration, the separation of the eschars, and the regeneration of the flesh, being all that a Surgeon can desire in a like cure, I then thought that my mission was fulfilled, and that I might return home, whither I was called by more material affairs. I had reason to repent of this journey: my patient having naturally a very voracious appetite, I had confined him to a severe diet; but he could hold out no longer; and, by the connivance of his nurse, he took various food, and that plentifully too. This conduct had soon ruined our progress. I returned to *Paris*; my representations were useless: the patient had shaken off the yoke of the faculty, and of reason. Indigestions ensued one upon another. The looseness, which never left him after, totally suppressed the suppuration, and made him void the matter quite crude by stool, and at last he died.

I was scarcely returned to *Rouen*, when there came to my hospital a patient seized with the dry gangrene, and who so perfectly resembled M. *Rondé*, that we commonly called him *the Rondé of Rouen*.

This cure being extremely interesting in all its circumstances, I think it is fit to transcribe for you the observation taken from a letter which I wrote on the 28th of *November* 1746. to M. *Morand*, and which he has since made public.

“ As this letter has been made public by Mr *Morand*, I shall not fill
 “ up this *Transaction* with a translation of a long letter already in
 “ print, but only give a short account of the case. C. M.

Thomas le Monnier, a Carpenter, aged 66, was, in *December* 1744. seized with violent pains in his right foot resembling those of the gout, but without swelling, and not preceded by any sickness. He had worked hard at his trade, and still carried it on in marshy places, and in bad and cold weather, when he was seized with this pain. He took various remedies to no effect. In *November* 1745. his great toe turned black; which blackness gradually spread to the other toes, to the whole foot, and at last to the ankle. He was sent to the *Hotel Dieu* at *Roan* May 19. His foot was intirely gangrened, black and dry; his pulse was low, and a little feverish; he never slept but 2 hours a night, suffered cruel torments, was greatly emaciated, and of a yellow and leadish complexion; his other foot was œdematous. “ M. *le Cat*
 “ says, he would not attack this case steel in hand, by scarifications or
 “ amputation; cruel and murdering methods! which quite extinguish
 “ the springs of life in the nervous system, which is already but too
 “ much

“ much ruffled. A barbarous Surgery! which préjudice, ignorance, and unskilfulness alone can adopt, and by which I have seen patients die, when there were great hopes of their recovery.” See *le Dran's Operations*, p. 30. *Saviard's Observations*, p. 98. and *de la Motte Obs.* 303. especially p. 371. *tome III.*

As this disorder arises from internal causes, it's cure is chiefly to be attempted by internal remedies; and of these I give cordials, diaphoretics, and such as are capable of reviving the drooping spirits, and of quickening the circulation even to the extremities: to which should be joined medicines adapted to the particular habit of each patient, whether it be scorbutic, scrophulous, or any other, which may have contributed to have brought on this particular species of gangrene: at the same time emollient, attractive, external remedies, moderately warmed with spices, applied to the part, concur to the same intention, by facilitating the flow of blood and spirits; the interception of which makes the distinguishing characteristic of the dry gangrene. Spirituous and aqueous topics, impregnated with volatiles, charged with saline, active, violently stimulating particles, and others in use in ordinary gangrenes, are to be avoided like deadly poisons; for such applications would rather contract and dry up the part the more, and increase the painful irritations, and dispose the contiguous parts to a dry gangrene. He gives us hopes of a treatise expressly on this subject.

But to return to the case. The suppuration began to appear, and the *eschars* distinguished themselves from the live flesh the very first days of the cure; and, after the time usual in the like suppurations, the foot and lower part of the ankle separated of themselves; and afterwards, healing over intirely, left a stump quite covered over with a covering of flesh, except some points of bone, which were not easily to be seen, but might be felt, by passing the finger over this new flesh. After this separation the tendons of the muscles, which lie along the leg, formed abscesses and *sinus's*. Some practitioners would have laid open these *sinus's* their whole length; but such large incisions would have so affected the system of the nerves, as to have brought back the gangrenous disposition into the internal parts, and so have destroyed the patient; as *M. le Cat* says he has seen it very lately happen by the like practice: wherefore he lets the matter gather in those *sinus's*, till it forms a bag, and greatly thins the skin, when he opens them almost without pain, in the place only where they point. These openings are sufficient both for the discharge of the matter, and for the coming away of the tendons; which no sooner happens, but the integuments, whether opened or not, unite and cicatrize. Thus, at the end of 6 or 7 months the stump itself cicatrized, and the patient was quite recovered. So long a cure could not but be liable to some accidents: the patient was one who would indulge in his diet, and the hospital is open for any one to come in and visit their friends: he had several strong accesses of an accidental fever, and 5 or 6 violent indigestions;

indigestions ; one in *July* kept him insensible for 20 hours, and was attended with a defluxion on the lungs, accompanied with such expectorations as gave the worst prognostications. These were incidents which rendered his recovery the more remarkable. He has picked up his flesh, and promises to enjoy a good habit of body ; so that probably he may live to a good old age. Several that have been cured by the Doctor of this disorder have lived to upwards of 70. Thus, says he, the common opinion, that it is impossible radically to cure the dry gangrene, is as false, as the ordinary method of treating it is bad. He does not establish his method as infallible ; but assures us, that in 8 or 9 years practice it has not failed curing any persons who exactly followed it, and observed the regimen prescribed.

“ We now return to Dr *le Cat*'s own words.”

I shall here subjoin the description of an instrument of Surgery, which I have lately invented.

Forceps for
the extirpation
of tumours too
remote from
the Surgeon's
fingers.

When the fingers can lay hold of an excrescency, the Surgeon need not think of making use of machines for it ; he will never find any convenient enough ; but all tumours that are to be extirpated are not within the reach of the fingers : there are even many of them which the fingers can reach, but where they cannot lay hold of them, nor work as the extirpation requires. Such are the excrescences situated a little deep in the *anus*, in the *vagina*, in the throat, &c. For the like extirpations I have been obliged to invent the *forceps* which I am going to describe.

Fig. 109.

Fig. 109. represents the *forceps* shut, as they are when the instrument is closed, or when it holds a small excrescency. *AC* is the extremity designed for laying hold of the excrescency : it is of silver, pliant as far as *aa*, in order to be able to give to these cheeks the different figures which those of the tumours to be extirpated may require. The inside of these branches is lined with a slip of buff-skin, or close shamoy (*KK*, Fig. 110.) to prevent the tumour's slipping when once it has been laid hold of. *BD* is the extremity of the *forceps*, to be held in the hand of the operator. *EF*, *GH*, are the pieces which connect the 2 parts of the instrument, instead of the common joints or rivets of the other *forceps*, in a very advantageous manner. These pieces make the principal utility of this invention.

Fig. 110.

Fig. 110. represents the pincers taken to pieces in two parts. One easily sees that the piece *E* is to go into the notch *gg*, so that the screw *f* may pass through it's slit, and that the nut *F*, put on upon that screw *f*, is to keep the whole together : but one thing which the figure cannot shew, is, that this piece *E* is moveable in the direction lengthways of the *forceps*, to answer the different openings of the cheeks. You may observe in *b* the pin upon which this piece turns. The piece *G* likewise receives the screw *b* into it's slit ; and the one and the other is stopped by the nut *H*. But an essential remark with regard to the piece *G*, is, that it must have the figure of an arch of a circle, the

radius

radius whereof is the instrument itself; that is to say, it ought to be the portion of an arch of a circle, the center of which is at the extremity of the instrument; and this to the end that in the small extremity of the pincers, the ends of the cheeks find themselves over-against each other, whatever opening one may give to the larger extremity, or to the handle *GH*.

To explain the use of these *forceps*.

I suppose I am to extirpate an excrescence, a *condyloma*, of 2 inches depth in the *rectum*, I cannot lay hold of this tumour with the fingers, nor make it come out; yet it is very troublesome to the patient, and one is disposed to slit or lay open the *anus*, to make room for this extirpation. With our *forceps* we shall avoid this cruel preparative, and shall with great ease make the extirpation. First, I introduce the fore-finger of the left hand into the *rectum* over the tumour that is to be extirpated, to make myself sure of the situation: with the right hand I take the part of my *forceps* *CD*, *Fig. 110.* and thrust it into the *rectum*, under the finger which is already there, and make it slide along the right side of the tumour, which to me is the left side. With the fingers of the left hand I support this instrument in it's situation, whilst with the right hand I introduce the other part of the *forceps* *AB*, *Fig. 110.* and let it slide along the left side of the tumour, which is over-against my right-hand. Without taking the fore-finger of my left hand out of the *rectum*, I put together the parts of the instrument. I press between it's cheeks the tumour by it's root; after which I draw the fore-finger out of the *rectum*. I take with the left hand the handle of the *forceps* *BD*, *Fig. 109.*; I thrust along the right side of their cheeks underneath the knife *Fig. 111.* the button *A* being turned against the cheeks, and the back *CD* towards the inside of the *rectum*. I push this instrument as far as beyond the tumour under the extremity of the cheeks of the *forceps*, of which I can make myself sure with the fore-finger of the left hand. Then I raise towards the upper part the cheeks of the *forceps*, in order to prolong as much as possible the root of the excrescence; and in this condition I pull towards myself, with the right hand, the knife; which does not fail cutting the tumour.

There are cases, in which the instrument, *Fig. 112.* will be of more convenient use. This cuts only by it's crescent *a b*, pushed forward, and moved alternately from one side to the other, to assist it's cutting.

There are circumstances, wherein the knife *Fig. 113.* may be preferable. In fine, there are others, wherein all these instruments are of service in one operation, which happened to me in Oct. 1748. in the extirpation of a scirrhus *polypus* in our *Hotel Dieu*; in which operation I employed not only all the instruments above described, but also a cutting *forceps*, which I had contrived in 1735. for the extirpation of *fungus's* of the bladder; and which alone was proper to take off one part of this tumour, that was as large as a goose's egg, and was falling into the throat. I suffer the wound to bleed a little, and then dress it with

Forceps for
extracting
stones, and
other foreign
bodies, lodged
in the parts
where the
common for-
ceps are of no
use. Fig. 114.

all the precautions usually taken against hæmorrhages in the operation of the *fistula in ano*.

The same mechanism just now described in the foregoing *forceps*, may be applied with advantage to the *forceps* with which the stone is pulled out, and to other instruments designed for extracting bullets, splinters of grenades, pieces of iron, and other foreign bodies. There are several cases in the cutting for the stone, in which no use can be made of the common *forceps*: the most frequent is this; when a stone, laid hold by the ordinary *forceps*, escapes from the instrument half-way, and so remains engaged in the incision. The expedient commonly taken, is to push the stone back into the bladder, in order to have again the necessary room for managing the *forceps*; but besides the cruel pain in thus pushing back the stone into the bladder, this foreign body may enter into the cellular texture which surrounds the bladder, and lodge itself there, and then the *forceps* not having any longer that play which was endeavoured to procure to them, the stone will remain in that fatal lodgment, without possibility of pulling it out, and the patient will die. This has been seen many times. The stone having stopped in the passage of the incision, you slide along the body of it one of the cheeks of our *forceps*, *A* or *C*, well-oiled, which will be done without much trouble, as I have experienced it; the other cheek afterwards will pass on the other side; after which you join them, as has been shewn above, taking care to press close the extremity *AC* upon the stone, and to leave the largest opening on the side of the handle *BD*, as in *Fig. 115*. both to hinder the stone from escaping, and to widen it's passage; then, having well secured the screw *G*, you leave the screw *F* almost at liberty. You grasp the instrument with both hands, as near the stone as you can, and you draw that body out, managing it as is usual with the common *forceps*. A second case of cutting for the stone, where these new *forceps* will be of great use, is this; when the stone is exactly embraced by the internal coat of the bladder; be it that it completely fills this whole organ, or that it fills part of it, which may have closed itself upon the stone; as has happened to *Joseph Bunel*, whom I cut at *Andelys* in 1743. of which I have communicated the observation to the *Royal Society*; or that the stone has made to itself a lodgment or bed in the inside coat of the bladder, prolonging itself towards the cellular texture, which surrounds a small part of those inside coats; as I saw it in the bladder of *M. Pigache*, an eminent Counsellor of our city of *Rouen*.

In short, every foreign body lodged in the substance of any part of the human body, be it of what nature it will, becomes the object of our instrument; and the extraction of it will, in my opinion, become much more easy by the means of these *forceps*, than by the bullet-drawers, and most of the other instruments invented for that purpose; provided however one gets *forceps* made of all sorts of lengths and bigness, in short, of all the shapes which the different cases may require.

XV. The filamentose fabrick of the testicles composed of yellowish *vascula* is sufficiently known. But those vessels, which carry out the *semen* formed in those *vascula*, and convey it to the *epididymis*, have always been doubtful. The only one, who has come near the truth, is *Regnerus de Graaf*; and all the subsequent writers have submitted to his discoveries.

Let the *epididymis* be gently and carefully filled with quicksilver, by the *ductus deferens*, now and then pausing, or dipping the testicle in warm water, that the vessels, being gradually expanded, may give way; for a sudden repletion will be apt to burst the middle or upper part of the *epididymis*. By this method, it has often appeared to me, that the *epididymis*, through it's whole length by which it adheres to the testicle, except the head, is one subtile canal, which is capable of being unfolded, as was perceived by *de Graaf*.

In the upper part of the *epididymis*, which he called the head, *Fig. 116.* *d*, which is strongly connected with the *albuginea*, the fabrick is different: for there that one canal is divided into 10, 12, or more, which are sometimes very subtile, and sometimes much thicker than the duct which complicated the *epididymis*. These canals we shall, for brevity sake, call *vasa efferentia semen*, *e e e e*.

Each fold being gathered into a cone forms it's own *fasciculus*, and returns by a retrograde course toward the lower part of the testicle, and comes within the *albugineum* into the middle part of the testicle, where the *epididymis*, being free on one side, adheres only on the other. These vessels and cones are represented in *Fig. 116. ff.* These vessels are gradually extended on the surface of the testicle, which is continuous with the *albuginea*, and being parallel and conjoined form the *rete* or net *g g*. From this conjunction it happens often, that when only one or two of the *vascula efferentia* are filled from the *epididymis*, they all swell, and are manifestly filled with quicksilver, by those vessels which received it, as it runs back in that part which is nearest the testicle. This net is nearly of the same length with the half-free part of the duct of the *epididymis*. *De Graaf* is the only one, who has seen and figured it: only he has represented the vessels too parallel, and not joined by their middle branches. He has also made them too long, and almost equals to the testicle. The other Anatomists have with one consent taken them either for a single duct, or for a blind membrane.

From that net there comes a single vessel, with a single cellulous *septum*, into the muscular flesh of the testicle, and descends to that province of the divided testicle. These vessels without folds are much larger than one would imagine, and more tender than the vessel of the *epididymis*, so as to be easily bursten by the weight of the quicksilver. I call them *recta vascula testis* from their straitness. These strait vessels receive the yellowish *vascula testis serpentina*, which *Ruysch* resolved into *capilli*. Sometimes it has happened, that the fluid metal has entered

tered even the most tender vessels, so that there can be no doubt at all of their being hollow canals.

This therefore is the fabrick of the testicle, that a yellowish *semen* is generated in the *vascula serpentina*, is sent down into the *vasa recta*, and thence deposited in the *rete* under the *albuginea*, and then comes thro' the tortuous *vascula efferentia* into the *epididymis*. From the *epididymis* the path of the *semen* is twofold, one hidden of which we shall not speak in this place, and the other commonly known, to the *vesiculæ*. I have now thrice discovered it by throwing in quicksilver.

One vessel, for I have not seen any more, parts from the middle of the *epididymis*, is extended into length, and ascends together with the vessels of the testicle: I never traced it indeed to the end; but without doubt it reaches to the lymphatic vessels, which I have often seen in the human *funiculus* of lymphatic vessels. These are the minute vessels in the *abdomen* which *Ruyfch** filled in a hare by the *vas deferens*. For though I have hitherto seen but one, I am far from denying that there are more. It is very probable, that it's use is to resorb the liquid, and thereby to render the *semen* more thick.

The other passage, by which the *semen* is carried to the *vesiculæ seminales*, is manifest; for the quicksilver being injected into it has afforded me some discoveries. I will not greatly insist upon the *ductus deferens* being continued directly to the *urethra*, and the insertion at a right angle into it of the excretory duct of the *vesicula seminalis*, which is less than the duct of the *urethra*, Fig. 117. *pp*; and yet that liquors injected into the *ductus deferens* easily enter into the *vesicula*; for these things are well known. But this I must inculcate, that every *vesicula seminalis* is but one minute intestine, into which many *appendices cæcæ* are inserted. This appears, when the *vesicula* being filled with quicksilver or wax is resolved, on patiently dissecting the cellulous webs which bind both the principal minute intestine, and it's blind appendages. I have often prepared the appendages, and found much diversity in them, with regard to length, simplicity, direction, and diameter. The chief minute intestine of the vesicle, *ll*, is terminated in a thick, obtuse, blind cone. Into this intestine are inserted 8 or 10 appendages, *mm nn*, the first of which are usually branched, as *mn*, but not constantly; the last are rather simple, as *mm*. Something like this was seen by *Lealis*, as also by *Bassius*†, but he has represented the appendages too small: he has also added a ring, which I know to be a fold, and not a circle. I shall only add, that I have seen such great and compounded appendages, that it was difficult to determine which was a trunk, and which an appendage.

Fig. 117.

Explanation
of the figures.
Fig. 116.

Fig. 116. The testicle filled with quicksilver. *a* The *ductus deferens* with the complicated vessels. *b* The lower part, where it begins to ascend contrary to itself, and takes the name of *epididymis*. *c* The whole

* *Catal. Mus.* p. 152.

† *Observ. Anat. Chir.* Dec. 1. n. V. T. 2. ff.

whole *epididymis* filled, composed of the serpentine vessel. *dd* The head of the *epididymis* dissolved. *eeee* So many vasculous cones, into which the head of the *epididymis* is resolved. *ff* The *vasa efferentia semen* arising from the cones: they are marked with few letters, to avoid spoiling the figure. *gg* The net of the testicle. *hh* Some rectilinear ducts. The other globules are dissolved by the quicksilver making way through some bursten vessels, which I would have expressed, that the figure might not in the least depart from truth. *i* The flesh of the testicle naked.

Fig. 117. *aa* The urinary bladder. *b* The posterior plane of Fig 117. the longitudinal fibres. *c* The *prostata*. *dd* The ureters. *e* Arteries of the vesicles. *fg* The *ductus deferentes* in their cellulous end. *h* The right *vesicula seminalis*, not altered. *i* The *ductus seminifer*, which perforates the prostate. *l* The left *vesicula seminalis* filled with wax, and cleared. It is much the longer for the *cornicula* being dissolved. *mm* The blind appendages of the *vesicula* which were short in this body. *nn* Some branched appendages. *o* The *ductus seminifer* going through the prostate. *p* The excretory duct of the *vesicula* inserted into it.

CHAPTER VII.

BONES, JOINTS, and MUSCLES.

I. **T**HERE appeared, covering the lower part of the loins of a lusty infant just born, a large incysted tumour, that seemed capable of containing a pint of water, whose contents had escaped in the birth from a small perforation in the middle of the cyst; from whence, on pressure, issued out a bloody serum. Flannels, wrung out of an hot, discutient, and restraining fomentation with spirits, were twice a day applied, to prevent it's mortifying; to which the upper part seemed greatly tending. The first 4 days there appeared no visible alteration in the child's health: she sucked well; was as hearty and strong as most at that age are; no paralysis in the extremities, but a daily discharge from the perforation of night & day of the same bloody serum which at first issued out. The nurse had observed, that, during all this time, it had not made one drop of water. The fifth day the child was convulsed; which increasing, she died in the night following.

On the division of the cyst, next day, there appeared a thin membranous substance, lining it internally; and might be an expansion of the membrane which envelops the *medulla spinalis*. A number of small blood-vessels appeared about the perforation of the bone; and underneath

An observation of a spina bifida, commonly so termed; by Mr George Ayllett, Surgeon at Windsor. N^o. 472. p. 10. Jan. &c. 1744. Read Feb. 2. 1743 4.

neath a small portion of the *medulla* of a very thin consistence. There was no opportunity of making a further examination, through the mother's importunities: but the lumbar *vertebræ* and *os sacrum* were taken out, as appears in the figure annexed.

Fig. 118, by
C. M.

A B, The *vertebræ* of the loins. *BC*, The *os sacrum*. *CD*, The *ossa coccygis*. *E F*, The *spinal* processes of the *vertebræ* of the loins; which spines are here discontinued, and an opening formed, *F G H I*, quite into the canal of the *vertebræ*; so that the *medulla spinalis* was entirely laid bare without any bony covering. This opening has been mistaken for a parting of the *spinal* processes into two rows; or as if at *F* they had divided into two branches; the two edges *F* and *G* feeling thro' the integuments like a bifurcation of the spine, and so have given rise to the notion of a *spina bifida*; which case I doubt whether it ever exists: for a perfect *spina bifida* must suppose the very canal and *medulla spinalis* to divide into two branches, the bodies of the *vertebræ* to become near twice as wide as usual, and the *spinal* processes to divaricate into two rows or ridges of spines. C. M.

Dr Ratty, late Sec. R. S. has communicated a case like this. See Vol. VII. Part iii. Chap. vii. Art. iii.

Some observations on the spina ventosa; by the late Claudius Amyand, Esq; F. R. S. and Serjeant-Surgeon to his Majesty. N^o. 480. p. 193. May and June 1746. Read May 15. 1746.

II. What practitioners generally understand by the *spina ventosa*, is a caries in the bone, from the extravasation of some sharp juices within it relaxing the tone of the fibres, and swelling and increasing it's bulk beyond the natural bounds. In this case, the humour, or extravasated juices, pent in the bone, works it's way out of it, through the external *cortex*, or into the joints, or both. By detention it acquires an acrimony; and, like vinegar, and other acrid juices, it not only relaxes the tone of the bony tubes, by mollifying them, but also, like a caustic, it tears and lacerates them. At this time the bone swells, tumefies, and spreads; and the sap flowing, running out through the lacerated tubes, overspreads the surface, and adds to the tumefaction, as the liquid matter, forming a *callus*, is indurated there. So that, when this happens at or near the joints, the bones in contact are knit together, and the cariosity is incrusted and covered with an *exostosis*, in as many places as the matter confined within the bone, upon breaking the *cortex* of it, will work it's way out at. And thus this distemper may be considered differently, as it happens to be in the different stages of it. The alteration which the bone suffers from the extravasated matter lodged within the substance or cavities of the bone in the first stage of a *spina ventosa*, becomes the occasion of some exfoliation or detachment from it. As that matter acquires a greater acrimony, the texture of the bone being relaxed, and the *lamellæ* made soft and yielding, the bone is enlarged in it's dimensions; and, in the last stage of it, wherein the bone is carious, the corrosive matter destroys the continuity, as it makes

it's



Fig. 117.

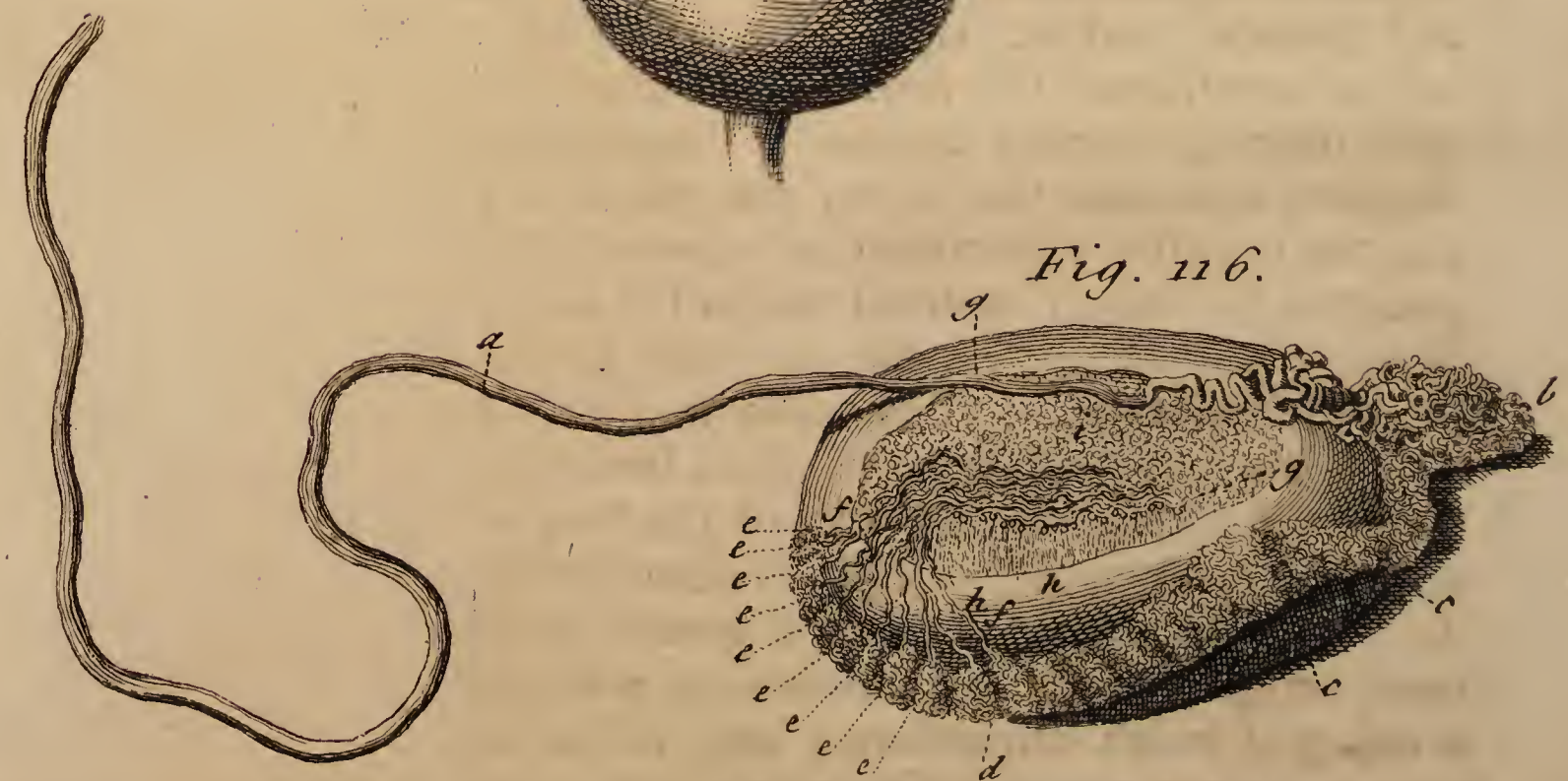


Fig. 116.

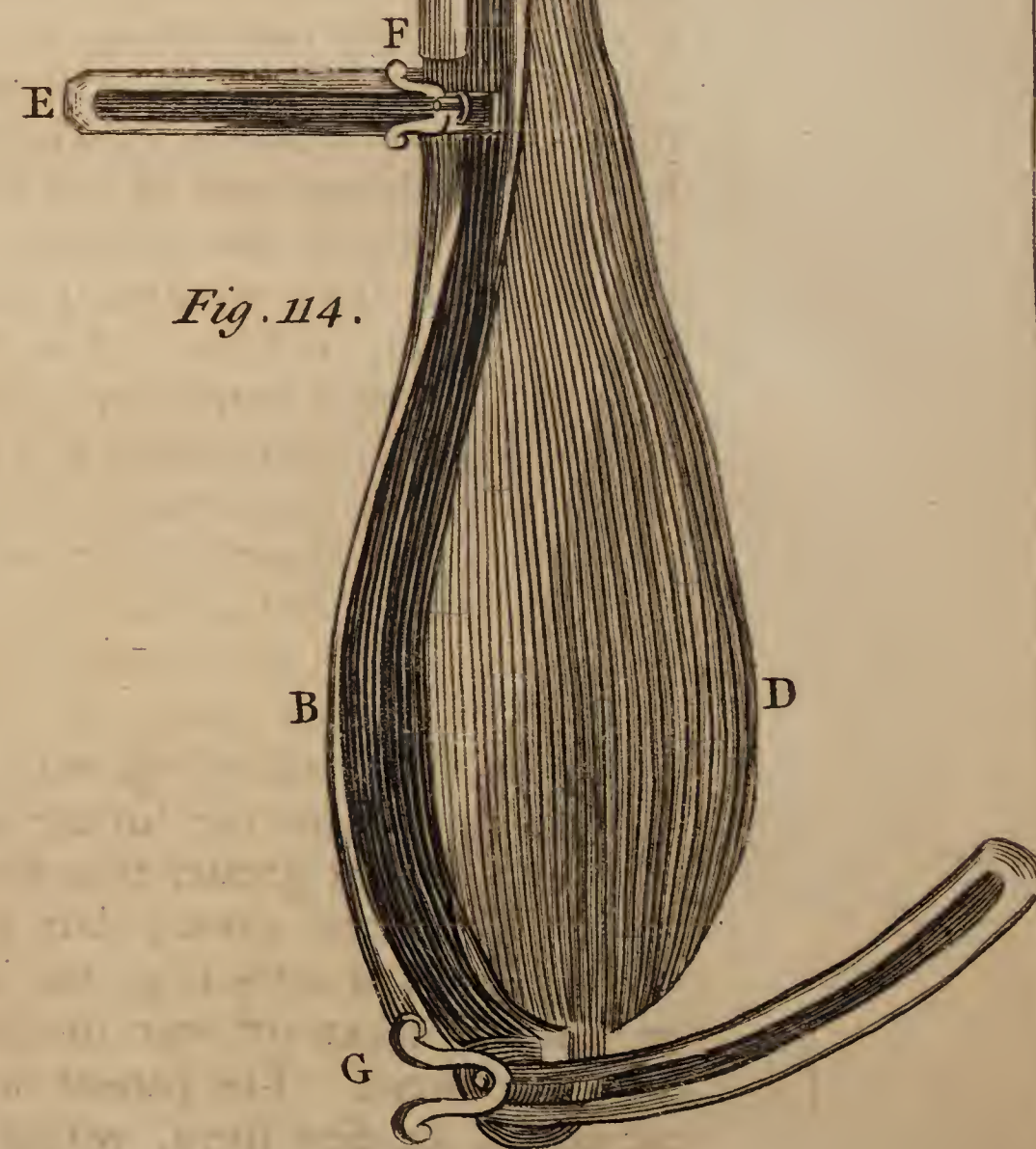
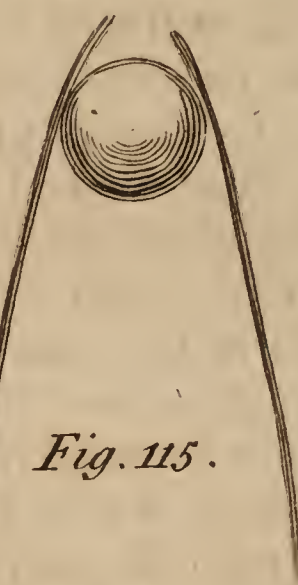
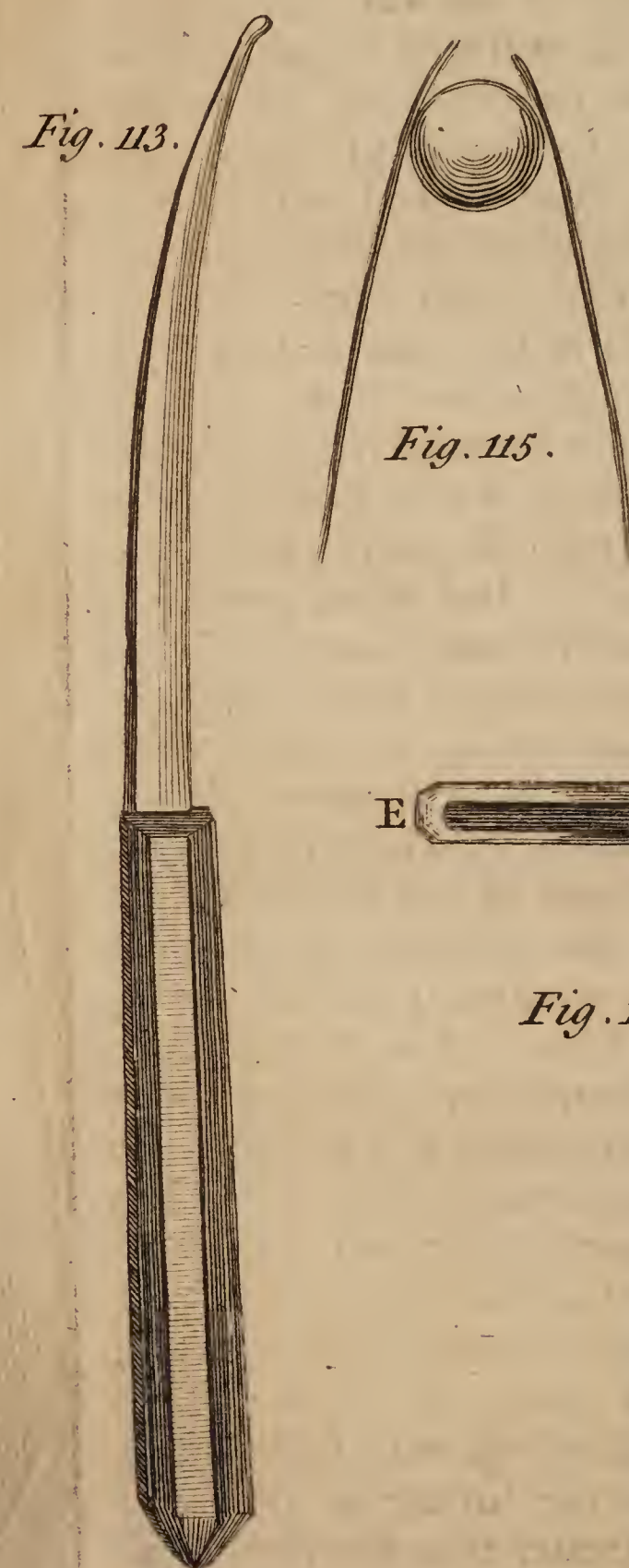
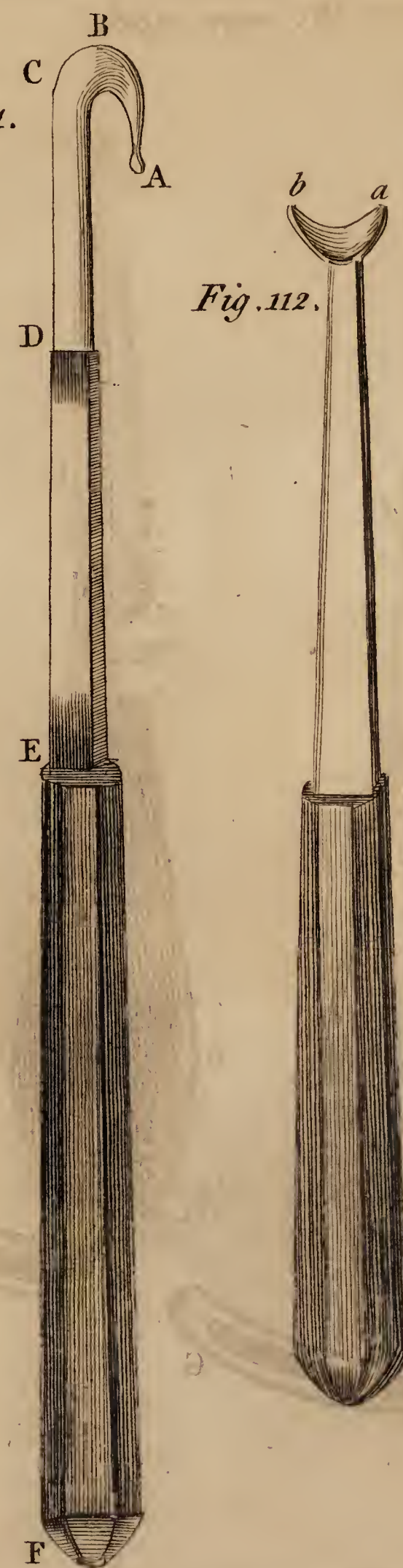
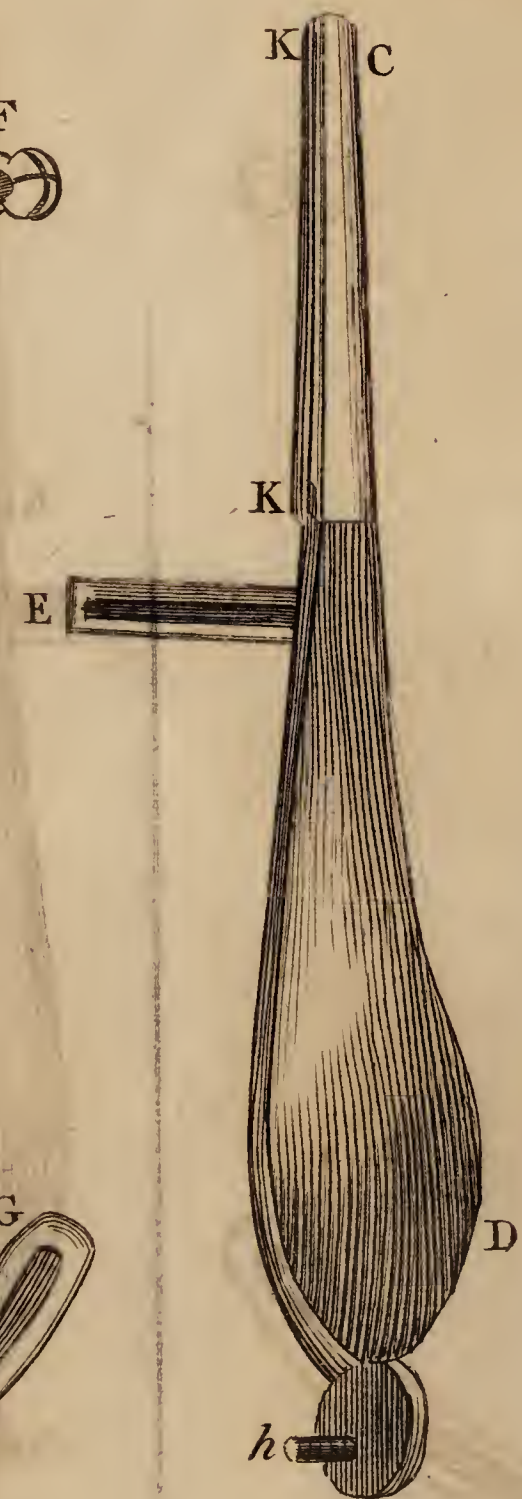
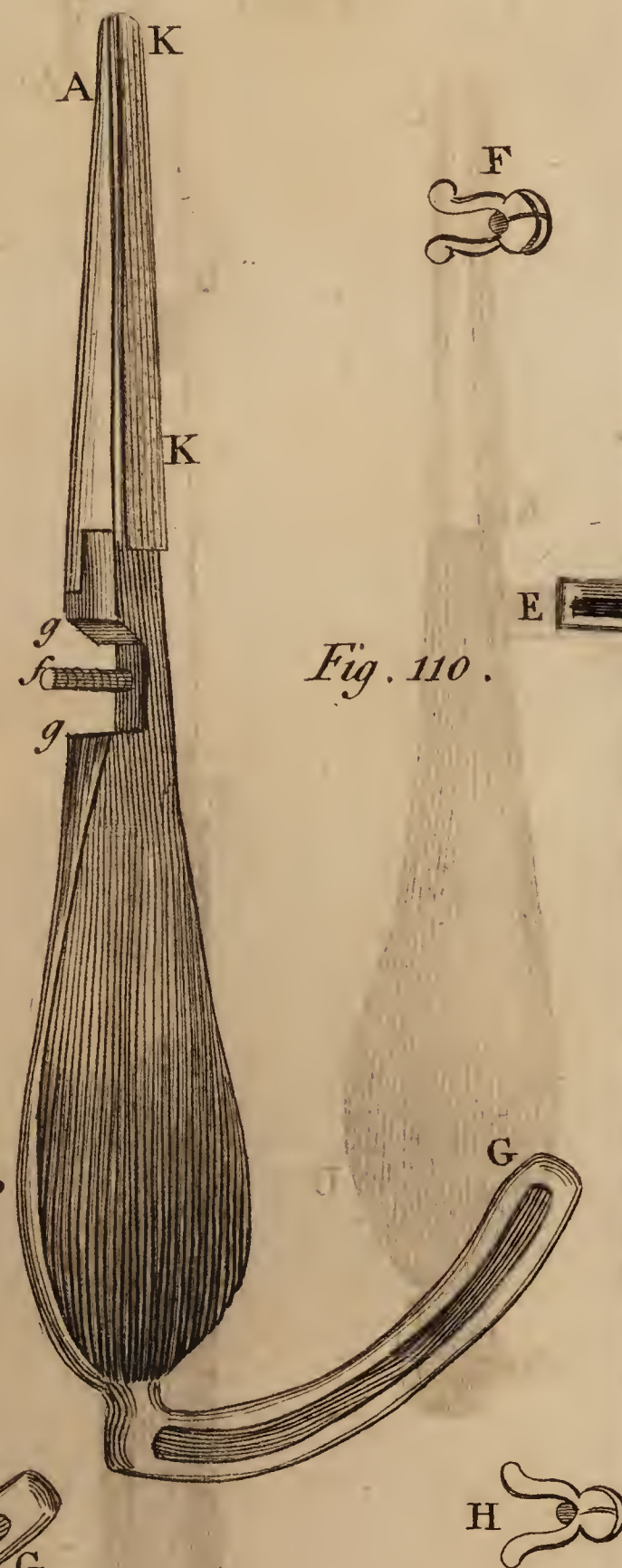
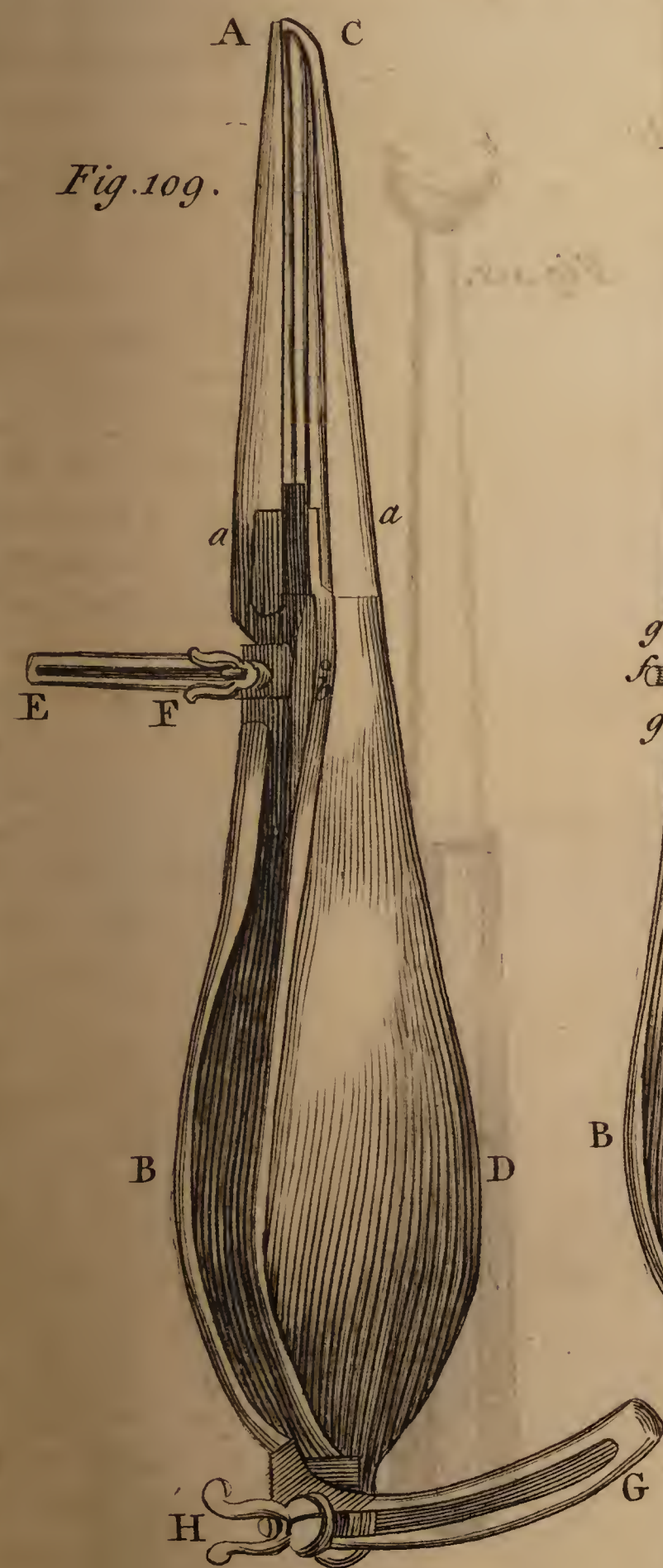
it's way thro' the *cortex*, and into the joints. At this time impostumations appear in as many places as the matter can make it's way out at. The callous matter lodged under the *periosteum*, gradually ossifying, covers the bone more or less with *exostoses*; and the joints are stiffened, by the extravasation and induration of the sap flowing out of the bone there. The impostumations that happen in the bone towards the centre of long bones, are always attended with additional mischief, as the working out of the matter there meets with a greater resistance from their *lamellæ*, which lie close, and are compact; the exfoliations made in the first stage, and, as it were, in the beginning of the *spina ventosa* there, being frequently confined and locked in by the *cortex* of the bone, or some callous expansion on the surface of it. In the last stage of this distemper in this place, the bone is usually perforated with large holes, tubulous cavities, and fistular openings, and the main bone rotten, at a time that the exfoliations inclosed preserve their primitive state and solidity. The patient then cannot survive it long: for, as a hectic fever and *diarrhæa* are fed by the continual absorption of some of the matter into the blood, so the body is drained by the large discharge from such wounds. The amputation of the limb is then the usual remedy; but a better may be hoped for, before it comes to this pass, as will appear from the following observations.

Tho. Pentney, a Shepherd in *Norfolk*, aged about 22, was admitted OBSERVATION I. into *St George's* hospital, towards the beginning of *Aug.* 1739, and committed to Mr *Middleton's* care. Five years before, upon the crisis of a fever, he had felt a great pain in the *os humeri* of his right arm, which continued several months; during which, the dimensions of this bone were so increased, that, towards the end of the year, it was half as big again as is usual in the natural state. About this time the bone impostumated; and the matter being discharged by the breaking of the integuments on the outside of the arm, the patient was eased, so as to have been able to attend his flock as before. When he came to the hospital 4 years after this, he had above 12 holes thro' the integuments on the outside of the arm, answering to, and corresponding with, as many leading into the medullary cavity of the bone; some of which were large enough to admit the finger. These impostumations had been most vexatious of late. There was an *anchylosis* at the elbow; and, for 2 years last past, he neither could bend his arm, nor use it in pronation and supination. The whole bone felt thick and unequal up to the shoulder, where the articulation was free. The patient now did not complain much. The discharge from the wounds was moderate; his rest, stomach, and pulse, as well as could be expected; and therefore he was determined to forego any thing, rather than submit to the amputation that was proposed.

In consultation with Mr *Pawlet*, Mr *Wilkie*, Mr *Middleton*, and Mr *Hawkins*, we agreed to make an incision from the deltoid muscle down to the elbow, thereby to lay the distempered bone bare, so far as it

it appeared to be affected ; and, with the exfoliative *trepán*, to make a fair opening into the medullary cavity of the bone, by taking off so much of it as was perforated in the external part of the arm, and so to make way for the application of the actual cautery, if that was found necessary. The two first operations were performed with ease : for, as in the incision the *periosteum* was readily detached from the bone, so the bone in view, being nearly of a cartilaginous nature, and making little resistance, was soon removed, by the repeated application of the exfoliative *trepán*. In the course of the operation, Mr *Middleton* found a bone loose, which hitherto had lain concealed under the *cortex* of the bone ; which, being taken out entire, measured above 7 inches in length, and more than 2 in circumference ; the centre forming a tubular chanel, wherein the *medulla* of the bone had formerly been inclosed. This was an exfoliation fairly separated from the surface of all the medullary cavity of the bone, but improperly called an exfoliation, as the thickness of the bone throughout was as thick as a shilling : it was more substantial in some places than in others, and opened here and there, so as not to be a complete tube. But what, perhaps, may be thought to deserve more notice is, that this loose bone, though it had for many years been soaking in the matter lodged in the bone (which, at times, had worked it's way out, by perforating the surface of the *os humeri* externally), yet this exfoliation was in no wise altered, or tainted with cariosity, as the main bone was ; but in every respect so sound, as to give ground to hope the posterior part of the *os humeri* might be so ; but it happened otherwise. Upon the removal of this exfoliation, the whole internal surface of the bone was found of a substance like a cartilage : it was bare in some places, and covered with flesh in others. The flesh was fungous in the lower extremity of the bone that was carious ; firmer towards it's upper end, where it was found ; and callous towards the middle, which was degenerated into a cartilaginous substance. The removal of this exfoliation having laid open the whole bone, in the inferior part of it was found a *sinus* leading into the articulation with the cubit, and a cariosity in the bone there ; and, in the upper end, where the bone appeared now sound, but formerly was distempered, several *foramina*, which were filled up with flesh, under colour of the *deltoid* muscle. At this time the surface of the wound was very large, and the discharge from it greater than the patient could support. His stomach hitherto had been good ; but that being defective, and a hectic fever with a *diarrhœa* attending, the amputation of the limb was concluded on. It was taken off near the articulation with the *scapula*, where the bone was found. The patient did not survive it long ; for the *diarrhœa* increasing, he died spent, within 4 days after the operation.

In the explanation of the figures, a further account will be given of the state of the bones in the amputated limb. The *humerus* next to the shoulder-joint did not, after the patient was dead, appear so sound as it had during life : for the matter proceeding from the bone, which was found,



found, in 2 or 3 places of it, under the deltoid and pectoral muscles, shewed, that this part of the bone was not in that sound state it appeared in, when the amputation was made.

Nov. 5. 1739. Mr *Johnson*, about 26 years of age, having com-
 plained for 12 months last past, of a swelling in the bone of his right
 arm, which seemingly had been the cause of several impostumations he
 had had thereabouts since, then applied to me for cure. He could
 assign no cause for this swelling, saving his taking cold, as he imagines,
 after having exercised himself by flinging heavy stones at a great distance:
 for that soon after he was seized with a fever, and a great swelling from
 the neck to the fingers ends of this arm; which settling towards the
 middle of the *os humeri*, where several fistular openings now were, and
 coming to suppuration thereabout in a short time after, a great quan-
 tity of matter was discharged by incision in *Nottingham*; whereupon
 the Surgeon had told him, that the bone was bare; and soon after had
 cut another opening in the hind part of his arm, where there was ano-
 ther gathering, and the bone also laid bare, and dressed both wounds,
 as expecting the bone to scale off; but that meeting with no cure, and,
 on the contrary, the wounds to break open as oft as they had been heal-
 ed up, and that matter was still gathering in new places, and the swell-
 ing in the bone to increase, had determined him to look for a cure in
London.

At this time there were 5 or six fistular openings leading to the bone,
 distilling a sanious matter on the sides of the tendon of the *deltoid* muscle,
 and the hind part of the arm, where the bone was principally enlarged;
 tho' it was very remarkably increased in bulk the whole way down to
 the elbow. I could not with my probe discover the state the bone was
 in; but, being satisfied it was carious, and that this distemper was a
spina ventosa, proposed, for the cure, the laying open all the bone in the
 anterior part of the arm; which the patient readily submitted to. This
 distemper was found to be a *spina ventosa*, or cariosity in the body of
 the *os humeri*, whereby above 4 inches of the solid bone had been de-
 stroyed; all which was cased in by an *exostosis*, or callous expansion;
 saving in a few places, where the matter flowing from the medullary
 cavity of the bone had preserved an opening. This *spina ventosa* was
 treated nearly in the same manner as the above-mentioned, and the cure
 performed as follows. It was entered upon Nov. 7. by making an in-
 cision to the bone upon the external part of the arm, about 6 inches long,
 and one broad, beginning it above the place in the bone where the *del-*
toid muscle is inserted; but, on the side of it, almost down to the *supi-*
natores radii; and then, by destroying with the *lapis infernalis* all the
 flesh growing on the *exostosis* or callous expansion incompassing round,
 and, as it were, incasing the carious bone, which the next day being
 scraped off, the fistular opening leading into the medullary cavity then
 came in view, and the probe going a great way therein, I immediately
 proceeded to the trepanning of the bone, and enlarging that fistular

opening into it with the exfoliative *trepán*, perforating through the callous expansion or *exostosis*, which was spread externally almost $\frac{1}{2}$ of an inch upon it, quite into the medullary cavity. The next day I applied this instrument above and below the preceding perforation; and, by cutting and paring off the angles betwixt them and the sides of the perforations, with an instrument the engravers make use of, then made a fair opening into the medullary cavity of the bone, and a convenient one too; for the discharge of the matter hitherto confined within it, which, whilst pent in, had occasioned the cariosity, and the progress of it, now found to have destroyed above 4 inches of it's body; and also for the removal of the fragments and loose bones confined in the medullary cavity, and the application of the necessary means, as well to stop the progress of the evil, as the promoting the casting off of the morbid bones. But the work was far from being finished, as, upon the extraction of the forementioned loose bones, it appeared that we had a *fungus* sprouting as well from the circumference of the medullary cavity above and below that part of the bone we had operated upon, as from the callous expansion over it's outward surface occasioning a greater discharge of *sanies* than our patient's strength could support; that our opening in that cavity was not yet sufficient to discharge all the matter that was deposited in it, as well through a fistular opening in the internal part of the bone answering to the *axilla*, as another somewhat lower over the large vessels that run upon the surface of the bone internally, that, being sheltered above and below, we could not come at them without enlarging further the opening we had in the bone externally. This laid us under the necessity of enlarging this opening in it's upper and lower part, so as to bring in view the fore-mentioned. These were enlarged with a *terebellum*, without any hazard of wounding the large blood-vessels, which were sheltered by the callous expansion lining the carious bone on this side: and having thus rendered easy the discharge from all these cavities, we had nothing to struggle with then but the running; which from this time became daily less; that from the internal part of the arm, by the matter having a more easy vent for itself; and that from the *fungus* on the bone by a solution of the *lapis infernalis* it was dressed with. Mr *Singleton* being consulted upon the case, the 18th from the first operation, was of opinion, that the amputation was hardly practicable; the *sinus* into the medullary cavity shewing that the cariosity was up to the head of the bone; but that, if it was practicable, he thought the patient had far better chance for his life, by pursuing the cure in the way he was in. At this time we had but in part subdued the *fungus* that was continually sprouting up from the sinuous vacuity in the upper part of the bone, where the *medulla* was all wasted, as well as from that which was yet growing without the bone from the callous expansion the carious bone was covered by: but this was so effectually overcome and conquered by the repeated application of the actual cautery, and by it the bone dried up so, that, in less than 2 months from

from it's use, all the morbid bone did cast off. The quantity of the morbid and carious bone taken out at divers times, being about 4 inches of the solid, was effectually repaired, by the matter flowing as well from the circumference of the callous expansion about it, which all along had steadied the patient's arm so, that he could pull his stockings on and off, as from the matter flowing from the ends of the *os humeri* into the cavity formed round this incasing or incircling bone. If we had been lingering in our proceedings, it is likely the great discharge would have exhausted the strength that was necessary to carry on this cure, before we could have got through so many that were unavoidable; and that the flesh growing from the edges of the external wound would have rendered more painful and difficult the several operations on the bone. By this proceeding, this difficult cure was ascertained in less than a month, and intirely finished in 6; the patient having now the power of his arm as complete as ever. Nor is the limb at all disfigured or shortened; the expanded incircling bone attached to the ends of the *os humeri* preventing this; so that the only appearing defect is, that the bone about the wound is thicker than usual; but that strengthens it, and supplies a defect in the anterior part of the arm, where there is a considerable hollowness.

In favour of the young Surgeons I shall close this account with a few remarks, by way of inference; and describe the figures of some bones, which that expert Surgeon and Anatomist Mr *Hawkins*, Surgeon to His R. Highness the Prince of *Wales*, shewed upon this occasion.

1°. That 'tis highly probable, a suppurated *phlegmon* in the marrow, upon the crisis of a fever, hath been the original cause of the *spina ventosa* in the two cases before us; and that, if the bone had been denuded, and the opening made through it enlarged, when the matter first made it's way thro' the integuments, that the progress of the evil had been prevented in both, and the cure brought about in the last case with a far greater ease.

2°. That a large opening is always more advantageous than many smaller, seemingly equal to it. And this appears plain in the two cases mentioned, inasmuch as the matter which was discharging through the many large *foramina* in the bones corresponding with the medullary cavity in them, have not prevented the progress of the evil; and therefore we may conclude, that as a large opening in the bone, by giving a free vent to the matter, will afford us the nearest prospect of a cure in the *spina ventosa* of all bones, so that must be the work of the Surgeon, when that distemper breaks out towards the centre of long bones.

3°. That, in the *spina ventosa* affecting long bones towards their centre, the application of the *trepan*, or of any other instrument as shall take away a considerable portion of the substance, is particularly necessary, were it only to make way for the removal of such exfoliations as are detached from the inner cavity of the bone in the first stage of the distemper; which, in the two cases before us, were concealed and shut

in; and, in many others, may be wedged and locked in by the induration of the callous matter on the surface of the bone, as may appear in Fig. 121.

4°. That, in a *spina ventosa*, in the centre of long bones, though the discharge attending it is not great, if any of their joints are made stiff by an *anchylosis*, viz. by a callous expansion that shall solder together the bones in contact, the only resource will be the immediate amputation of the limb; forasmuch that, if that is delayed till the patient labours under a hectic fever, colliquative sweats, a *diarrhœa*, or such symptoms as denote a reflux of the matter pent up in the bone into the mass of blood; the operation then will afford us very little hopes of success: whereas the *spina ventosa* that affects the extremities of long bones only, and that which appears in scrophulous cases in the bones of the *carpus* and *tarsus*, when the discharge is not great, are best cured by lenient means, and the most pacific methods.

5°. That, in that stage of the *spina ventosa*, wherein the bone is carnified, that is, turned into flesh, with a painful *fungus* shooting out, as well from the callous matter spread over the carious bone changed into flesh, as from the carious bone itself degenerated, that, in this case, as there can be no hopes of restoring it to itself, the removal of the bone so degenerated, is the only method to be pursued; as that will make way for the application of the actual cautery, wherein the cure principally consists: and if this does not succeed, we must proceed to amputation. This was the case of Mr Coreho in *St Mary Axe*, whose thumb Mr Saintbill took off Oct. 26. 1739, Mr Ferne and myself being present: the last bone of which, affected with a *spina ventosa* about 18 years, was so swelled out, and changed into flesh, that not the least part of this bone, as a bone, was found, but only its cartilaginous covering, in the articulation with the second internode; all the bone itself being nothing else but a lump of flesh.

Explanation
of the figures.

Fig. 119, 120.

Fig. 119, and 120. represent a *spina ventosa* in the *os humeri* of the right arm, after an operation performed upon it, during life, with the exfoliative *trepan*. The distemper in the bone being complicated with an *anchylosis* and cariosity of the heads of the *cubitus* and *radius* in the articulation of the elbow, occasioned by the nutritive juice in the inner cavity of the bone, where the *medulla* was wasted by an ulcer, running out through several holes made by it directly into this joint. Fig. 120. represents the exfoliation, which lay concealed within this bone, until the time the operation above-mentioned, it was taken out by, was performed. This exfoliated bone being harder than the main bone itself it was inclosed in, and seemingly sound, whilst the latter was carious: notwithstanding the cast-off bone had been soaking longest in the matter extravasated and pent up in the medullary cavity, which was the original of the *spina ventosa*, this exfoliation, which was the first consequence of the extravasated matter within the bone having, as it were, been detached from the main bone, which was

was dissolved by it, some time before it could possibly reach the body of the bone itself.

Fig. 119. The *os humeri* amputated near the shoulder-joint *b*, with the two bones of the fore-arm sawed off in *EF*. *aaaa*, several large openings in the bone below and under the deltoid muscle, where the bone had formerly been distempered and affected by the *spina ventosa*; this part of the bone appearing sound, and seemingly restored to it's natural state; whilst all the rest of it downwards is strangely altered by the humour occasioning it, and degenerating, as it were, into a cartilaginous substance, or such a one as could readily be cut with a knife; the interior being destroyed by a cariosity, and the exterior, which was spread out beyond it's natural dimensions, incrust-ed by a callous expansion sticking to it, made solid, and forming a body with it, saving where the *periosteum* was strongly adhering, the *cortex* of the bone, after the *exostosis*, or callous shell spread over it was taken off, being perforated with many blood-vessels, which were larger, and more in number than usual; these vessels perforating it, as it was rendered soft and yielding, and the indurated callous matter sticking to it, as well within as without, was covered with a callous flesh sprouting from it. *BB*, a large opening made in the external part of the bone by the trepan, wherewith the whole medullary cavity of it was laid open; this opening taking into it's scope 10 or 12 large holes in the bone, all leading into that cavity thro' which the matter pent in, and causing the *spina ventosa*, at times had worked it's way out by; which, when laid into one by the operation, favoured the extraction of the tubular exfoliation, Fig. 120. which, though it was intirely loose in, and detached from the circumference of, the medullary cavity, yet was so concealed in it, by the *fungus's* obturating the *foramina* in the bone, as not to have been discovered before the opening, it was taken out by, was made in the bone. *C*, a large hole in the place where the external process of the *humerus* formerly stood; that having been destroyed, by the matter causing a great cariosity here; which also running out of the bone through the *foramina* made into the joint, had occasioned the *anchylosis*, and the cariosity of all the bones there. *G*, an *exostosis*, or callous expansion on the external surface of the *os humeri*, attached to it by a cartilaginous substance forming a crest there, about 2 inches long, and $\frac{1}{2}$ an inch high; which, being wedged in between the interstices of the muscles, by their motion hath been kept in some measure loose upon it. It is observable, that, during life, the substance of this bone had been rendered so soft, as to have yielded when pressed upon, if it had not been stiffened by the indurated callous shell, or *exostosis*, spread over it, wherewith it was incrust-ed from one end to the other: and this indurated callous substance, seemingly imbodyed in the bone internally, ending externally in a grissly, fleshy, and gelatinous substance: all which, sticking to the bone, added greatly to the apparent tumour of

of it ; as doubtless, in time, that would have been one with it ; it appearing that all these substances would have acquired the consistency of it ; though as yet, they had only attained that of a *callus* in it's infancy.

Fig. 120.

Fig. 120. a tubular exfoliation from all the circumference of the medullary cavity of the *os humeri* 7 inches in length, and near 2 in circumference ; which, lying loose and detached from the main bone, was extracted whole thro' the large opening made in it. And thus, upon the whole, it appears, that the inflammation, suppuration, and ulcer in the *medulla*, in the first place, has brought about the consumption of the *medulla* within the bone ; and, soon after, such an alteration in it, as hath given cause to the exfoliation that hath happened : and that the matter confined, in time having acquired a greater acrimony, hath gained the power of softening, dissolving, and destroying entirely the bone, wherein the circulation of the juices was preserved exclusive of any other bone, that hath been laid out of the way of that circulation, or hath happened to be detached from it : tho' this last evidently hath lain more under the power of the matter than the main bone itself was ; and to have been soaking longest in that very matter, which hath destroyed that bone wherein that circulation of the juices hath been preserved ; and therefore, that the way to a speedy cure of the *spina ventosa*, is, the making, as soon as possible, a fair opening into the medullary cavity of the bone that is affected with it.

Fig. 121.

Fig. 121. represents the anterior surface of the left *tibia* of a young person affected with a *spina ventosa*, inclosing an exfoliation detached from all the circumference of the *medulla*, about 7 inches in length ; which is to be seen through a great number of openings in the *cortex* of it.

AA, an *exostosis*, or incrustation in the bone enlarged, with an opening in it 4 inches long, and one broad, through which the inclosed exfoliation *BB* appears ; the *caries* of the bone in the joint of the knee, and 9 large openings in the body of it, having been made by the matter discharging from the medullary cavity, and causing a cariosity in the inclosing bone only ; whilst the exfoliated within is preserved in it's natural state. *C*, the cariosity of the bone in the joint of the knee, communicated to the *os femoris* wherewith it is anchylosed in *D*. *D*, the *epiphysis* of the *os femoris* in the joint of the knee, destroyed by the cariosity. *E*, the lower end of the bone, as yet in a natural state, with it's *epiphyses* forming the internal ankle.

Fig. 122.

Fig. 122. *AA*, the anterior and internal surface of the left *tibia*, cut off in *B*, where the bone was found : the opposite end being carious, and perforated with many large *foramina* or openings made by the matter flowing into the joint of the knee, from the medullary cavity in the bone. The exfoliation, *ccc*, seemingly sound, is detached from the cavity ; but is locked and wedged in by the *exostosis*, or callous matter

matter indurated on the surface, in such manner that it cannot be taken out without cutting off the edges of it. *ddd*, the upper part of the bone, carious within and without, made rough and unequal by the *exostosis*, or callous expansion rendered carious.

Fig. 123. *A*, represents a portion of the *os humeri* sawed off, at the time of the amputation of the limb; which is soldered by an *anchylosis* with the *cubitus B*, and *radius C*, in *D*; *E* being a large exfoliation detached from them, where those bones had been shattered, which is wedged in by a callous expansion, in such manner that it could not be taken out. *E*, the exfoliation from the *cubitus*, 3 inches long, adhering to an exfoliation from the *radius* of the same length, not seen; both which, being almost of the substance of the whole bone, are knit together by a *callus*: so that their re-union is prior to the cause which has occasioned the exfoliation. *FFF*, a callous expansion, inclosing the bones which are detached within, not hardened in the middle *G*; and partly cartilaginous there: so that, in this place, the bones made up by the callous expansion could play upon each other.

III. The outward causes of fractures not being sufficient to break or tear asunder the broken pieces of a fractured knee-pan, in the manner we see them in the fracture of that bone, the cause of it hath justly been ascribed to the power and action of the muscles upon it in a morbid state of that bone, which disposing it to a fracture when that power is strongly applied, and affecting both knee-pans in a like manner, soon brings about a fracture also of the opposite bone, then appearing as thoroughly sound as the fractured part before was thought to have been. Many instances might be given of the fracture of the *patella* in persons thoughtly sound, where the part itself had received no hurt whatever; which seems to imply, that the cause of it was to be ascribed to the muscles only; and the rather, that many instances may be given of the fracture in the neck of the *os femoris*, in the middle of the *humerus*, *tibia*, and *perone*, by the power of the muscles only, or such power of them as sufficiently shewed they have as great a share in several fractures ascribed to outward causes, as they are frequently known to have in the fracture of the *tendo Achillis*. However, as the fractures occasioned by the action of the muscles are cured by the same remedies, as are effectually applied to the cure of others from outward causes, they deserve no further notice. I shall give some instances of such in which the bones themselves are morbid.

An observation of a fracture of the os humeri by the power of the muscles only &c. by the same.
N^o. 475. p. 293. Jan. &c. 1745. Read March 7. 1744-5.

July 15. 1738. I was sent for to a middle-aged Gentlewoman in my neighbourhood, of a seemingly strong constitution, rather fat than lean who had broke her right arm in the middle, while with her hands she was endeavouring to bring together the ends of a piece of tape. The standers-by heard the bone snap, and were the more astonished at the cause, as the patient was sitting distant from any thing that could strike her.

her arm, or contribute to the breaking of it. The grating of the ends of the bone against each other, when the arm was moved, the loss of it's action, and all the common attendants of a fracture, were sufficient convincing proofs of it. It was reduced with ease, and dressed as usual; but, I own, I doubted of the cure, 'till Mr *Shipton*, who was consulted thereon, assured me, that he was witness to several fractures of this kind, where the patient had done well. We agreed, that the bone must have been distempered, and likely with a cariosity; it appearing otherwise hardly credible, that the bone could have been broken from so slight a cause as this.

I have known two patients die after a fracture of the *os femoris* from the like; the first snapping in the middle as he was getting into bed, in whom all the centre of the bones was carious; and the latter getting out of it, wherein only a small part was so, and above $\frac{2}{3}$ of the circumference in every respect appeared sound.

For two years last past my patient, had been attended by Mr *Shipton*, on account of several scorbutic complaints, which I thought proceeded from some venereal taint; but the patient vowed, and hath all along assured us, she never had known man. Two years before this accident happened, I had attended this patient jointly with the late Mr *Fiquel*, on account of a *spina ventosa* in the centre of the *os bregmatis* on the right side; which being laid bare, the *caries* was found to have penetrated the two tables thereof, and all that time she had a gummatous swelling upon the upper head of the *radius* on the right arm, checking the motion of this bone in pronation and supination, both which she then mentioned to have appeared, upon the retreat or disappearance of broad yellow spots (not scurfy, nor in any wise resembling those in the *lues venerea*) her whole body had been bespecked with for some years last past; she having all her life been addicted to the scurvy, and never been in the way of getting a bad distemper; neither were the pains she complained of so vexatious to her during the night as the day. Upon Mr *Fiquel*'s death, for 2 years last past, she had, by Mr *Shipton*'s advice, taken the most powerful antiscorbutics; notwithstanding which, the forementioned humour on the upper head of the *radius* was much increased, and a new one of the same kind, for some months last past, had appeared on the head of the shoulder-bone, and another on the middle of the bone where the fracture now was. She likewise reported, that she hardly was cured of the *caries* I had attended her for on the *os bregmatis*, but another tumour had appeared on the skirts of the *os frontis*; which breaking of itself, the bone there had been found carious, whence she had had a running ever since; and that of late had appeared too puffy swellings over the left brow, of the like nature with those that had broken before, and therefore feared the bone there would also prove bad; and that there was also a hard swelling, attended with pain, springing up upon the head of the *radius* on the left arm. As all the remedies hitherto used had proved ineffectual, Mr *Shipton* and I concluded, that

that a mercurial salivation was the most likely to conquer a humour daily laying hold of new bones ; but we thought it improper to put the patient into it, whilst we stood in need of a *callus* for the knitting of the broken bone. The cure of it has been completed in about 6 weeks, and the patient has ever since had the full use of it. At this time we had an œdematous *phlegmon* over the left brow, with a fluid undulating there in 2 places ; and, by way of addition to the former complaints, a node also was springing up with pain on the head of the *radius* upon the left arm, attended with an œdematous swelling. In this condition she was put into a salivation by unction. This salivation, copious as it was, was kept up a whole month : in the first fortnight of it, the matter, gathered in the œdematous *phlegmons* over the brow, intirely disappeared, and then the *oedema* and tumour in the bone upon the fore-arms where the nodes were ; and these being nearly dissolved in the last fortnight, we were in hopes the cure had been brought about without opening the tumours over the brow, where matter had been felt, and the bone was thought carious : but our joy was not lasting, many of the complaints re-appearing in a short time after, notwithstanding we took the utmost care to confirm the cure by salivation, by a subsequent course of antiscorbutics, the decoction of the woods, &c. For as, upon the retreat or disappearance of the yellow spots in the skin, first-mentioned, the distemper in the skull had begun, and the nutriment of the bones in general had thereupon been vitiated, as appeared by the gummatous tumours, and *nodus's* on the bones, the *spinæ ventosæ* in the skull, and the extravasations of matter, denoted by the œdematous *phlegmons* upon the brow and *radius* ; so it appeared no less evident, that the retreat of these, and the reflux of the matter into the blood, had contaminated it again, being the forementioned symptoms, and worst, re-appeared within less than 3 months after : they had disappeared very fast, as the vessels were emptied during the salivation, but they re-appeared again faster upon the filling. During the salivation, or soon after, the cariofities in the skull, that had been laid bare, having been exfoliated, were cured ; the *nodus's* upon the shoulder-bone, and *radius* on the arm that had been broken, had vanished away ; and, what was thought no less remarkable, the matter extravasated upon the right side of the *os frontis* over the brow, where the œdematous *phlegmon* had appeared and disappeared at times, were intirely dispersed ; and these, saving the tumour on the *radius* of the left arm, never appeared again.

The renewal of the distemper shewed itself upon the bones that had been laid bare, by a new cariofity spreading very fast in the neighbourhood. At this time an inflammatory *oedema*, that had appeared upon the *os frontis* over the right brow (very distant from that collection that had been made on the left before the salivation, which had been dispersed, and now continued well) suppurating, was opened, and the matter found to spring from the frontal *sinus's* on this side thro' the bone that was carious. And as the tumour that had lately appeared upon the

radius of the left arm, and stood out the salivation, was now increasing with pain, with an œdematous *phlegmon* upon the arm where the tumour was ; it was agreed to put her forthwith into a second salivation no less copious than the first, but of a longer continuance ; to denude, first, all the frontal and parietal bones on the right side, that were carious, but to leave untouched those on the left side ; which, before the first salivation, had been suspected of being a *spina ventosa*, but now appeared sound. The patient entered into this salivation about the latter end of *May* 1739, after she had been duly prepared to it. This was continued 10 weeks, because of the frequent interruption we had met with by the returns of a *diarrhœa*, as oft as she was anointed, and the profuse sweats during the *dog-days*. In this, as in the former salivation, all the accidents gave way during the course. The progress of the cariosity in the bones of the skull was stopped, and the exfoliation being made, the wound was cured afterwards. She was put into a milk course, and sent into the country for the recovery of her flesh ; and after that into a subsequent one of a decoction of the woods, &c. but the advantage she had reaped by these did not continue long. In the autumn, she was frequently traversed by irregular shiverings and rigors, upon the re-appearance of the phlegmonous *œdema* about the *node* still subsisting on the left arm ; which now again grew larger with pain in it, still increasing in proportion as the vessels emptied in the salivation were replenished. The œdematous swelling about the tumour was also more phlegmonous, and matter was forming there on the bone, which, it was apprehended, was a *spina ventosa* : I therefore insisted upon the laying of that open : but, whilst she was preparing for her removal to town, this tumour intirely disappeared upon the appearance of a *diarrhœa*, that sunk her too fast to admit either of her removal, or any operation. This followed her to her death. During the 2 last days of her life, she was in a constant *delirium*, and universal convulsions.

The patient dying thus in the country *Sept.* 25. last, I was not acquainted with these latter accidents till after her burial ; and therefore missed the opportunity of dissecting the body, which perhaps might have given some additional light to this case : but, from so much as has been reported of it, I think it may be inferred,

1. That if a scorbutic humour occasioned the complaints in this case, as there is reason to believe it did, from the veracity and character of the patient ; and those symptoms being wanted that usually appear in the *Lues Venerea* ; yet it must be confessed the affection in the bones yielding to a mercurial salivation, as these did, and the return of complaints upon the reflux of the matter extravasated, contaminating and tainting the blood with a new ferment, or the same as before the flux, may ground a strong suspicion, that those in our patient proceeded from some venereal taint.

2. That a ficcidity or brittleness in the bones, inclining them to a fracture, may happen, independent from a *caries* in the bone : for, had the

os humeri of our patient been carious; at the time the fracture happened from so slight a cause as attended the endeavour of bringing and joining together the opposite ends of a string held in the two hands, it was morally impossible the complete cure of such a fracture could be brought about within six weeks. Whence it appears, that the occasional ficcidity of a bone may be no bar to the flowing of that quantity of sap or callous matter necessary to operate the cure after a fracture.

3. That the muscles must have a great share, as well in the fracture of all bones, as in the dislocations of them; as in this case the *os humeri* was broken by the power and action of the muscles only; and therefore that the keeping of them quiet, by placing the broken limb in the most natural and easy posture, must be highly instrumental to the cure.

4. That the absorption, or reflux into the blood of a purulent *sanies*, inflating the part where it is lodged under the appearance of an oedematous phlegmon, or phlegmonous *oedema*, is as dangerous as the absorption by the blood-vessels of a purulent matter extravasated. So that the reflux in either case will again taint the blood with the corruption that had critically been flung out of the course of the circulation; and that, when this happens, there will be cause to fear a renewal of the complaints, and, possibly, worse symptoms; as has happened in the present case.

5. That yet there appears a wide difference betwixt a *phlegmon* oedematous and an oedematous *phlegmon*; inasmuch as, in the first, the *oedema* goes off as the *phlegmon* comes to a crisis; whereas, in the latter, that seldom comes to a crisis, but changes, and appears and disappears continually. When the matter of a *phlegmon* prevails, the tumour may be resolved, without any inconveniency to the patient, if it is not critically determined upon the crisis of a fever; and when it is, the depuration being complete, he fares the better for it: whereas, in the oedematous *phlegmon*, wherein the *serum* prevails, the tumour beginning with an *oedema*, neither the tumour, nor the inflammation of it, are permanent; but appearing and disappearing, as the sanious matter is thrown out of the blood, or refluxes back into it. The sanious matter of it is apt to contaminate the whole mass, as oft as the tumour disappears; and therefore it is evident, that, in this latter case, that is in the *oedema* phlegmonous, we are to give a vent to the matter lodged in the parts as soon as may be, and even before the matter is concocted, or fully collected, as in critical abscesses; viz. so soon as some irregular shiverings, and such other symptoms, have denoted the extravasation of the matter in the *membrana cellularis*, wherein the humour first makes its appearance. For that when in an *oedema* phlegmonous, or oedematous *phlegmon*, the matter changes, at times, with more, and at others with less inflammation; the tumour increasing and diminishing alternately, as the matter becomes more or less sanious and purulent, and refluxes into the blood at times: we may then fear some lodgment of it will be made upon some of the *viscera*, if that again is not critically thrown

out: and if it remains in the mass, that, in the course of the circulation, it will occasion such symptoms as here have happened to our patient, during the course of the distemper, even to her last, as oft as that has happened; and that in so plain a manner, as to make it evident, that the renewal of the symptoms was consequential upon the reflux of a purulent *sanies* back again into the blood from the part wherein it had been lodged: and thus it appeared, that as this reflux of matter was principally made during the salivation, when the vessels emptied could best attract it; so it was expedient the discharge of it had been made before that was entered upon.

6. The return of the complaints from this cause was very obvious at last, but not so at first: and should not this make us tender and circumspect, when we pass a judgment upon the conduct of others? that the matter should so shift its lodgments after every salivation, is no less remarkable than that it should rather fix on new parts, than those which had been affected before; and that the cause of it, virulent as it was from the first, and attacking the juices flowing in the bones in so particular a manner, should have been no check to the complete cure of the fore-mentioned fracture in the usual time. It were to be wished men of experience were more ready in shewing the errors, failings, and slips in their practice than their successes: that might be of great service to the public, and those warnings prove of singular benefit to those who have not had the like opportunities; it being more eligible to be informed by the failings and misfortunes of others than one's own.

An extraordinary case of a fracture of the arm; communicated by Mr John

Freke, F.R.S. Surgeon to St Bartholomew's hospital. N^o. 494. p. 397. Jan. &c. 1750 Read April 5. 1750.

IV. The following extract of a letter from Mr *John Barde*, Surgeon, in *New York*, having been communicated to me, I thought the case so curious, and to have been treated in so skilful and regular a manner, as to be worthy of being laid before this learned Society.

“ In May 1746. as Colonel *Morris's* Lady was going from this city to *Morrisena*, the Colonel's country seat, she had the misfortune of being thrown out of her chaise by the horses suddenly falling down; by which means she had both the bones of her left arm broke. She was immediately conveyed to the *Hermitage*, a country seat belonging to the honourable *Joseph Murray*, Esq; which is about a mile and $\frac{1}{2}$ from the place where she met with this unlucky accident. Being sent for by the Colonel, I examined the case, and found the bones broken in an oblique direction, a very great contusion of the muscles, and the arm already tumified; which, together with the being 3 months gone with child, obliged me, previous to the reduction, to bleed her in the right arm. I then carefully reduced the broken arm, and applied the proper bandages. The case succeeded, with less inflammation than usually attends more simple accidents of this kind; and, in about 12 days, she ventured to go from the *Hermitage* to *Morrisena*, being about 6 miles. But what I think remarkable in this case is, that tho' the ends of the fractured bones had been exactly
“ applied

“ applied to each other, and tho’ the bandages had been continued 40
 “ days, yet, upon their being removed, the callus remained so soft
 “ and flexible, that her arm could be bent with the greatest ease into
 “ any position, and appeared perfectly strait, to the satisfaction of the
 “ patient and family. But, as I observed the callus to be unconfirmed,
 “ I re-applied the bandage, which remained on five weeks longer.

“ Upon a second examination, I found, to my surprize, and the great
 “ uneasiness of the patient and family, the callus as soft and yielding as
 “ before. I therefore could not help thinking this case to be similar to
 “ 2 cases, of which *Hildanus* gives the history from his own practice :
 “ for, as the patient at this time was about 5 months gone with child,
 “ nature determined those nutritious and agglutinating juices, which
 “ were necessary to form and consolidate the callus, into a different
 “ channel ; viz. to the support and increase of the *fœtus*. As a conse-
 “ quence of this opinion, I contrived a bandage made of velvet, with
 “ four thin blades of steel, $\frac{1}{4}$ of an inch broad, and 7 inches long,
 “ which were covered with velvet, and fixed to the inside of the velvet
 “ bandage ; and to the bandage itself I fixed four small buckles, with
 “ their corresponding straps. The bandage thus contrived, and buck-
 “ led on the arm, resembled a muffitie, and was worn with the great-
 “ est conveniency, keeping the arm in a strait direction, for 4 months
 “ longer. In the mean time, the patient was made easy by the encou-
 “ raging hopes I gave her, that, after her labour, the oeconomy of
 “ nature would be more immediately directed to the recovery of the
 “ use of her arm. And, indeed, so it turned out : for the callus re-
 “ mained unconfirmed, her arm useless, and at particular times painful,
 “ till within 9 days of her delivery ; when, all on a sudden, she ac-
 “ quainted the Colonel, that her arm was quite free from pain, and
 “ had a very different feeling from what it had before. From that time,
 “ in less than a month, the callus was intirely confirmed, and the pa-
 “ tient recovered the use of her arm ; which, considering the oblique
 “ direction of the fracture, and the long-continued softness of the cal-
 “ lus, has it’s natural beauty and straitness wonderfully preserved.

“ I know that a woman’s pregnancy is mentioned in general, by
 “ several authors, as an hindrance of the callus being so soon confirmed
 “ as in other circumstances. But, except the two cases mentioned by
 “ *Turner*, and taken from *Hildanus*, I do not remember to have read
 “ any history, where the usual oeconomy of nature, in restoring a frac-
 “ tured bone, was so intirely, and for so long a time, interrupted ; and
 “ where the cause of such an interruption appears so plain, as in the
 “ case I have been here relating,” &c.

V. Inclosed I send you a case, which, as the learned *Heister* observes, *A letter from*
rarely happens *. If you think it may be worthy the notice of so dis- *Mr D. P.*

* *Os innominatum, raro frangitur.* Heist. Instit. Chirurgic. Tom. 1. pag. 200.

tinguished.

Layard, Sur-
 geon; to C.
 Mortimer;

M. D. Secr. tinguished a body as the *Royal Society*, I beg the favour of you to present it, as a small token of my great respect.
 R. S. inclosing
 an account of
 a fracture of the os ilium, and it's cure. N^o. 477. p. 537. Aug &c. 1745. Read Dec. 5. 1745.

The case of
 John Easdon,
 Coachman to
 the R. Hon.
 the Earl of
 Darnley.

Feb. 8. 1745. John Easdon, Coachman to the R. Hon. the E. of Darnley, about 22 years of age, was jammed between a waggon and a coal-cart, as he was getting up into the waggon; the cart-wheel pressed on the upper part of the left *os ilium*, and, by a sudden jolt, squeezed him against the waggon, so as to raise him from the waggon-wheel on which he stood; then the cart going on, the poor man fell on the ground.

Being carried to my Lord's stables, I examined the part, and found (just below the *contusion* made by the pressure of the cart-wheel) a fracture running quite across the *costa* of the left *os ilium*, about 3 fingers breadth below the *crista* of the said bone; the end of the upper fractured part being forced in towards the cavity of the *abdomen*. The patient being laid on his back, on the edge of the bed, I applied a napkin on the *false ribs*, which was pulled tight by two assistants, in order to press the contents of the *abdomen* downwards: another assistant pressed the *abdomen* on the right side, while, by pressing the *crista* of the fractured *os ilium* gently inwards, I brought forth the edges of the fracture to a mutual contact. The fracture being reduced, the napkin applied on the *false ribs* was tightened, and kept on during the whole cure with the *scapulary*. I applied proper compresses, and a pasteboard cut according to the figure of the bone, over which I applied the * *spica bandage*. The patient was kept in bed for about 3 weeks, lying on his back, the affected side being supported with a soft pillow. By this means, and by observing what is generally recommended in all fractures, the patient was perfectly cured, and walked very well at the month's end.

The case of a
 young child,
 at Houghton
 in Hunting-
 donshire, born
 with all it's
 bones displa-
 ced: commu-
 nicated Feb. 8.
 1746. by Mr
 Edw. Davis,
 Surgeon at
 Huntingdon,
 to Dr Herman
 Heineken in
 London. N^o.
 483 p. 539.
 Mar. &c. 1747.
 Read May 14.
 1747.

VI. I found both the *radius* and *ulna* of the right arm, with the bones of the *carpus* and *metacarpus*, also the fore-finger and little-finger of the same hand, all dislocated. The *radius* and *ulna* of the left arm were dislocated, and receded from each other; likewise the fore-finger and little-finger of the same hand. The *os femoris* of the right leg was dislocated very oddly, and laid downwards, so that one might feel the end of it: the *patella* laid high up the thigh; and the *tibia* and *fibula* at their union with the *os femoris* were also dislocated, and receded very much from each other. The right leg, the *tibia* and *fibula*, at their union with the *os calcis*, also the *os calcis*, and the *tarsal* and *metatarsal* bones, likewise most of the toes. The left leg, the *fibula*, with some of the *metatarsal* bones, and some of the toes. The head, upon examination, likewise is very curious: the *lambdoidal* future is ossified all round, and rises with a prominency half an inch high: the occipital

* Vide Heister. *Institut. Chirurgic. Tom. 2. pag. 1217.*

bone has several risings, which feel like several *exostosis*'s; and the two protuberant sides of the occipital bone are enlarged to a prodigious degree, and unite with each other, but leave a dent between them which feels like a future. They are enlarged, I believe, to six inches long, and three broad: it is all ossified; the midwife and nurse say it was soft at first: the rest of the head appears very well.

This child is 7 days old: I have reduced the dislocated bones, tho' some with great difficulty; for the ends of the bones and cartilages seemed to be all ossifying; and there seems to be an universal *anchylosis* coming on. I could not reduce the right foot well; it was all ossified, with the bones displaced, and the *extensor pedis pollicis longus* was contracted, and had drawn the foot almost round. The jaw-bone was also dislocated, which the midwife could easily put in it's place, and the chin-stay supported it very well; that is, almost well, only apt to slip out on one side. The midwife and nurse say, they could easily, for the 2 first days, put all the bones in their places with ease, but they continually fell out again.

The mother received a fall a fortnight before delivery, and she fancies the bones were displaced with the fall, tho' she did not hurt herself: but whether it is from thence, or from some vice in the fluids, I shall not determine. If it were not for several *exostosis*'s and *anchylosis*'s in several parts, I should have imagined the child (tho' so young) was rickety; but for the above reason it cannot be that. The child seems at present lusty and strong, but I think will soon be otherwise; the woman is lusty, and walks out about her business, tho' but a week ago delivered; and she has 6 children besides, all very healthy.

VII. On *Tuesday Oct. 20.* about noon, Dr Greene's horse, strong, nimble, and vitious, started under him, at the waving of a plowman's whip, and with a quick and violent jerk, turned quite short, first to the left, and then instantly, and with the same impetuosity, to the right. After galloping a few paces, the Doctor fell gently off into a hedge, without receiving any hurt from the fall. A chariot was borrowed, into which he was lifted and brought home; for after this he could never stand. At 4 that evening I first saw him, just after he had been blood-ed. He was very faint, cold all over, and his pulse scarce perceptible, tho' naturally very strong; the *scrotum* so much swelled, that the *penis* was quite absorbed and lost in it, and it's colour a very deep red. I ordered him a glass of wine with a bit of bread, for he had eat nothing all that day. This revived him, and raised his pulse a little. He then told me, in answer to the questions I put, that the testicles were not hurt; that the twisting of the horse gave him at that instant the intolerable sense of being split asunder. I answered, that a violent and sudden stroke, from the pummel of the saddle, upon the *os pubis*, might probably give him that sensation: he replied, that it did not feel like a stroke, and still persisted in his first expression of being split asunder.

A warm M. D. to the

An account of the death of the Rev. Dr Greene, late Rector of St George the Martyr in Queen's Square London, and one of the Prebendaries of Worcester, where he died of an hurt received, as he was riding out in the neighbourhood of that city, contained in a letter from Tho. Cameron, M. D. to the

Rev. Charles
Lyttelton,
LL.D. and
F. R. S. N^o.
484. p. 609.
Oct. &c. 1747.
Read Dec. 10.
1747.

A warm fomentation was ordered to be constantly applied, and ℥iſs. of *Glauber's* salt, quickened with gr. ii. of emetic tartar, to be given in a quart of gruel. At 11 that night an emollient glyſter was given, the ſalts having as yet done nothing. Before morning he had 6 large looſe ſtools; but it gave him exquisite pain to be liſted upon the bed-pan. Next morning the ſwelling was increaſed, and the colour deeper. I preſcribed an electuary of bark and ſalt of amber, to prevent, if poſſible, the approaching mortification. The ſtale beer poultice was applied; and that evening, a fever coming on, ℥x of blood were taken from his arm. Next morning the ſalts were repeated without the emetic tartar, and he had 4 ſtools.

All this while he had made no water, except about a ſpoonful juſt after he was put into the chariot. The lower part of the belly, where a diſtended bladder would certainly point, was not ſwelled, tho' the parts upon the *os pubis* were very much ſo. The *ſcrotum* increaſed in bulk and bad colour every hour; and the inſide of the right thigh grew very tumid, with great pain, and a very perceptible fluctuation in it. Theſe obſervations convinced me that the urine had found a way into the parts laſt mentioned; though indeed I could not account for it, but by ſuppoſing that the *urethra* had been bruised, even to laceration, between the pummel of the ſaddle and the *os pubis*.

The Surgeon, Mr *Ruſſel*, ſoon came into my opinion about the urine, but imagined the bladder muſt be burſt. This I could not comprehend, nor could he explain; for the bladder lies out of the reach of all external injury from the cauſes hitherto aſſigned in this caſe. We agreed however about 3 in the afternoon to make a puncture into the *ſcrotum*; from whence urine, manifeſt to the ſmell, iſſued pretty freely all night.

Next morning a larger opening was made in the right thigh with the ſame effect. The parts ſubſided conſiderably; but the pulse riſing, *decoctum nitroſum* was given with the bark. This evening the hiccup came on, and the *ſcrotum* looked livid.

Next day the common emulſion, with a little nitre, and the extract of bark with muſk were ordered. But the hiccup increaſed, watry bliſters appeared on the *ſcrotum*, the voice faltered, the head failed, and the pulse ſunk. He grew worſe and worſe, till he quietly expired on Sunday morning at 11.

Upon diſſection, we found the *ſcrotum* and *corpora cavernoſa penis* mortified; the *offa pubis* wrenched aſunder to the diſtance of 4 inches, and a rent in the bladder, $\frac{1}{2}$ an inch in length, a little above the neck, and exactly in the middle where the *offa pubis* join. This was a very aſtoniſhing ſight, and gave me quite a new idea of the caſe; which if any Phyſician could have diſcovered without inſpection, I ſhall readily allow him more penetration than I pretend to. We may now however, reaſon about it, with a little more certainty than before; and it ſeems to me, that the body of the horſe in twiſting, acted with the power of a lever, to which the ſuddenneſs of the jerk, added in ſome meaſure the force

force of percussion. But all this leaves us still in wonder at the effect: for Dr *Greene* was a very strong large-boned man, 64 years of age, and the uniting surface of the *ossa pubis* was considerably broader in him, than either the Surgeon, or I had ever seen in any subject.

VIII. *Nicolas Reeks* was born in *Poole*, 1724. with both his feet turned inwards. His mother carried him to a Surgeon, who upon examination gave it as his opinion that he was incurable. The boy, as he grew up, was with great difficulty able to walk, but always on the outward edge of his feet and heels, so that he frequently fell down in walking, one foot striking against the other.

His parents being poor, in 1735, the parish put him apprentice to Mr *Richard Mockett*, of the same town, Taylor, apprehending it the only trade he could be fit for as a cripple. His shoes were made in a peculiar manner to lace on to his legs, the muscles of which were much smaller than those of boys of his age. He lived with his master, and worked at the trade, till about 1741. when they began to perceive a manifest alteration and turn in both of his feet; which was brought on without the assistance of any manner of art, application of plasters, oils, or bandages, till both feet were turned to their right and natural situation. He was able to wear his master's shoes, the muscles of his legs grew larger, his feet and legs like other peoples of his age; if any difference, they turned outwards more than others do. In *March* 1742-3, he ran away from his master, entered on board a ship of war as a marine, and is now living at *Portsmouth*.

Nov. 1, 1749.

“ I *Richard Mockett*, having read over the foregoing case, do hereby
“ certify, that *Nicolas Reeks* was a cripple, as therein described, when
“ I took him an apprentice; and that he was cured, whilst he lived
“ with me, without any advice, assistance, or application of medicine,
“ or bandage whatever: and I am of opinion the cure was performed
“ by his sitting cross-legged on the shop-board; as witness my hand,
“ in *Poole*, 1st *Nov.* 1749.

“ *Rich^d. Mockett.*”

“ We whose names are hereunto subscribed, do well remember, that
“ *Nicolas Reeks*, mentioned in the foregoing certificate, and formerly
“ apprenticed to Mr *Richard Mockett*, was born hurl-footed in both
“ feet, and a cripple; and do know, that he was cured, and well able
“ to walk, before he left his said master; and do believe it was per-
“ formed without any art or assistance whatever, than as expressed above;
“ as witness our hands,

“ *Mabella Glover.* “ *Susannah Jasper*, the
“ *Eliz. Glover.* “ boy's own aunt.”

The foregoing account of the cripple *Nicolas Reeks* was drawn up in such terms as were most agreeable to the apprehensions of the persons who have certified the truth of the fact, and to whom it was first read: and I am well satisfied in the credibility of their testimony, and that many other persons of reputation might be called on, who would sign the same.

Customhouse, Poole,
Nov. 11. 1749.

W. MILNER.

An account of a bristle that was lodged in a Gentleman's foot, and caused a violent inflammation; in a letter from Mr Arderon, F. R. S. to Mr Baker, F. R. S. N^o. 480. p. 192. May & June 1746. Read May 15. 1746. Fig. 124.

IX. *John Wood*, Esq; of the city of *Norwich*, being afflicted with great pain, and a violent inflammation, in one of his feet, applied to *Mr Castil*, an eminent Surgeon, for his assistance; who, upon strict examination, perceived a few short hairs sticking out, not far above the setting on of the little toe. Their thickness, and particular manner of standing out, put him upon taking hold of them with his *forceps*; when, to his great surprize, he extracted a large hog's bristle, of the size shewn in *Fig. 124.* where the pricked line *AB* denotes how far it was buried in the flesh. The Gentleman had immediate ease, and grew well in a few days, without any other assistance. And he can't give the least guess how the bristle got into his foot; unless by some accident it slipped into his stocking, and from thence worked it's way in. Had *Mr Wood* deferred his application to a Surgeon, till the bristle had been entirely buried in his foot, how miserable might he have been? What dreadful operations in Surgery might he not have undergone, without the least probability of having his malady found out, or obtaining a cure for it?

It is not unlikely that many desperate cases in Surgery may arise from such accidents as this.

The Crounian Lectures on Muscular Motion; by James Parsons, M. D. F. R. S. Supp. to the years 1744 & 1745 Read in Jan. 1743-4. LECTURE I.

X. In this essay I have nothing in my view but the consideration of a *muscular fibre*, and that of a *nervous* one, with the manner of muscular motion's being performed; having purposely neglected to touch upon the nature of sensation, or indeed any calculations of the force or powers of muscles; because they are already well treated of by several ingenious authors; and are capable of being handled upon rules of some certainty; being a part of the subject very different from what I take to be the purpose of the *Crounian Lectures*.

Most authors agree, that a fluid, commonly called *Animal Spirits*, flows from the *brain*, by the *nerves* to the *muscles*, in order to move them; but are at a loss to know how it is performed; and also, by what means those spirits are sent, so swiftly, into this or that muscle to be moved.

Some endeavoured to explain it, by supposing certain *valves* placed in the cavities of the nerves, (where they are divided into branches*,

* *Cartes. lib. de Homine*, and several of his followers, were for placing valves only in the divisions of nerves; whereas *Regius, Philosoph. Nat. lib. 4. cap. 16.* thought them to exist elsewhere in the nerves.

Fig. 118.

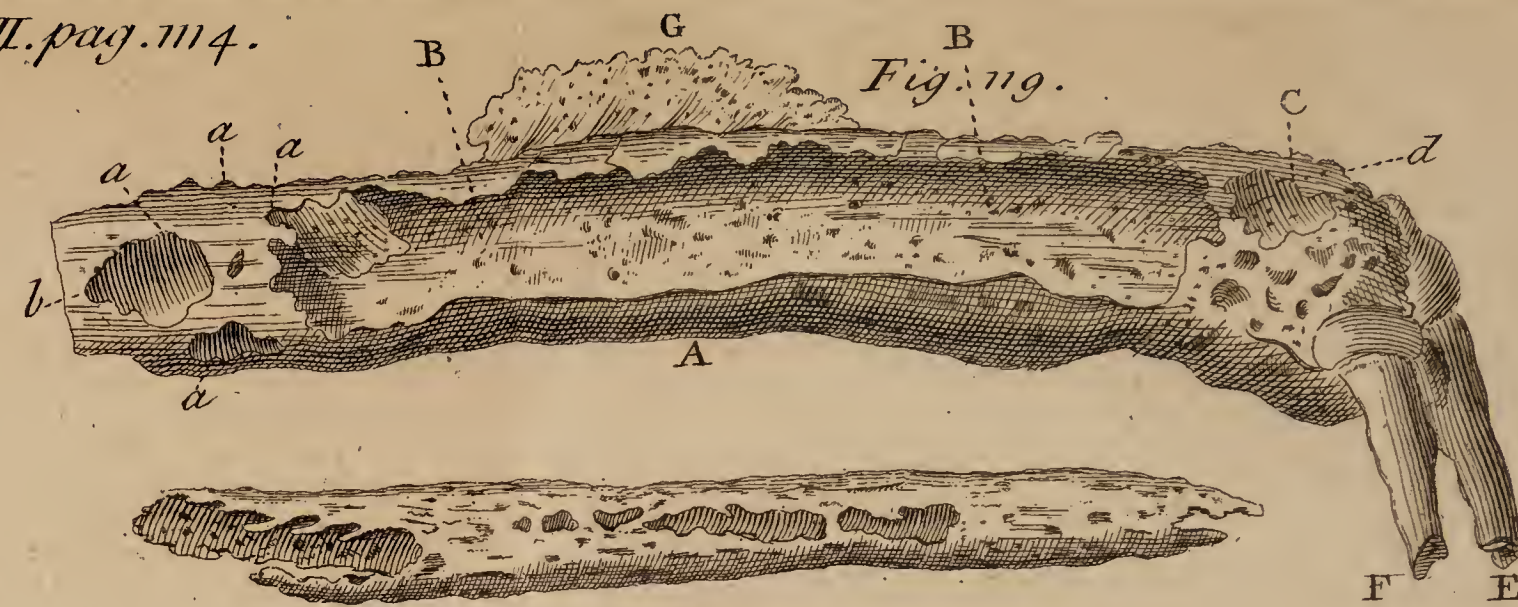
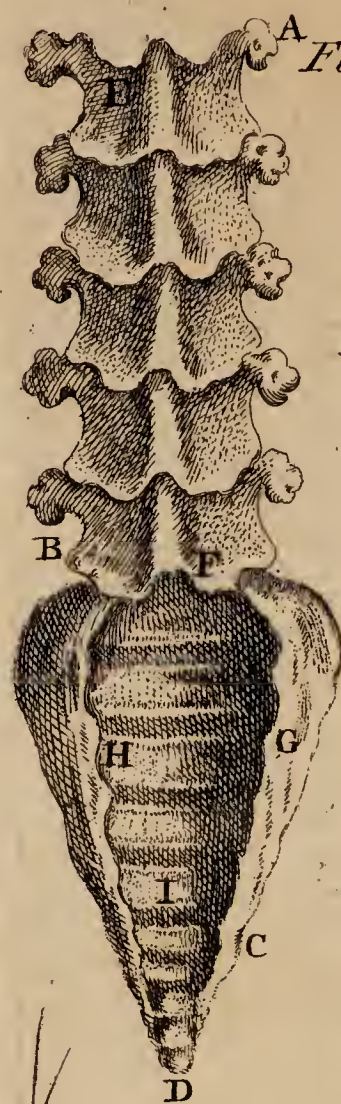


Fig. 120.



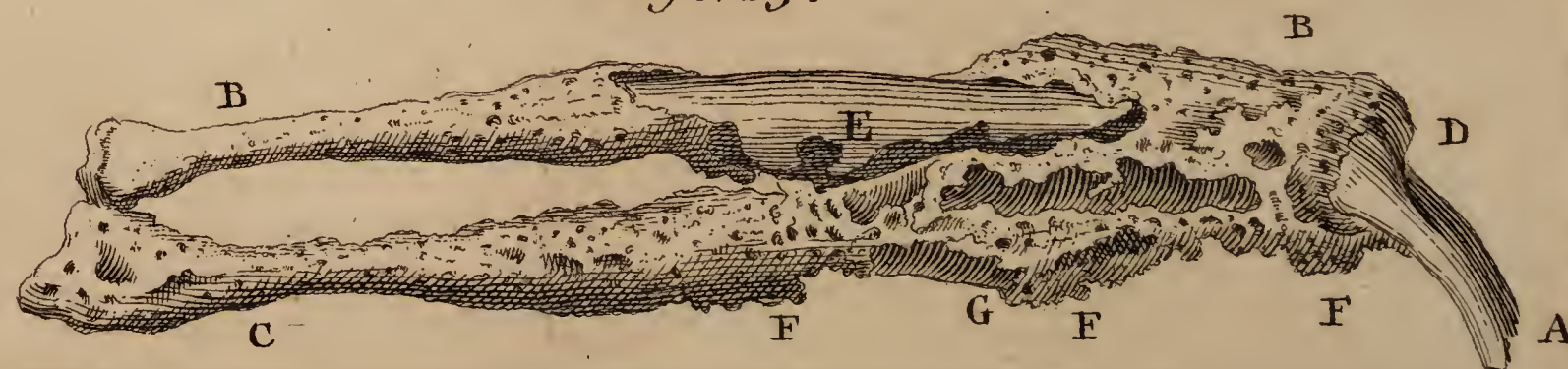
Fig. 124.



Fig. 122.



Fig. 123.



to go to different muscles) in order to stop the reflux of the spirits, and cause them, upon being brought back from one muscle, to be determined to the other, from the valve.

Others, not well satisfied with this scheme, imagined a double tube, passing from one muscle to the other, so placed, as that the orifice of one, in it's contraction, (being furnished with a particular valve) might be opened, and the spirits immediately flow through it, from the muscle to be relaxed into that to be contracted; whilst, at the same instant, the valve of the latter is shut, in order to hinder their flowing-out again, that the muscle may be swelled. By this swelling the situation of the parts being altered, the valve opens again, (the other valve being now shut) and the spirits flow freely back again to the muscle to be contracted.

Cartesius's * opinion was not very different from this: he supposes
 “ Several openings in each muscle, through which the spirits may pass
 “ from one into the other; which are so disposed, that, when the spirits,
 “ which come from the brain towards one muscle, have a little more
 “ force than those which go towards another, they open all the orifices
 “ through which the spirits of the other muscle can pass into this; and,
 “ at the same time, shut up all those by which the spirits of this may
 “ pass into the other; whereby all the spirits, contained before in both
 “ muscles, swiftly pass into one of them, and so swell and contract it,
 “ while the other remains relaxed and extended.”

These tubes, valves, and openings, are merely conjectural; having never been found by any Anatomist, and being only children of the imagination of some Philosophers, produced to serve their uncertain hypotheses. Nor, indeed, would it be a difficult matter to shew, that neither of these systems can account for muscular motion; and that such a structure would produce confusion, instead of that most regular and uniform process we daily see, in every action of life, if our intended brevity would permit it; which must be obvious to every one in the least acquainted with the structure and situation of muscles. Let us, however, consider other more particular opinions in their turns.

The first I shall take notice of is the memorable *Dr Croune*; whose *Dr Croune's* care for propagating the knowledge of this part of Physiology, is no *opinion*. less laudable than evident, in his foundation of these Lectures. He shewed a good example in his own attempt towards an explanation of muscular motion, in a treatise intituled, *De Ratione Motus Musculorum* †:

7 C 2

wherein,

* *De Passione Anim.* Part 1. Artic. II.

† This treatise was published at *London* in 1664. in 4to. and at *Amsterdam* in 1667, in 12mo. and it's being published without his name, occasioned a gross mistake in the Publisher of *Dr Willis's* works at *Geneva*; who printed it among that author's tracts, notwithstanding there is a particular tract in the very same volume, known to be *Willis's* own, intituled, *De Motu Musculari Medico-Physica*. The Editor's apology for so doing is as follows: “ *Traëtatum de ratione motus musculorum, (etsi authoris anonymi) anatomie*
 “ *cerebri, nervorumque descriptioni ab excellentissimo viro D. Thoma Willis, M. D. celeberrimo, institutæ, (ut alias, tum a materiæ affinitate, tum ab operis præstantia petitas,*
 “ *missas*

wherein, after a short recapitulation of some opinions before him, particularly of those of *Cartesius*, *Regius*, and Dr *Scarborough*, he endeavours to found his reasoning upon mechanical laws, in accounting for muscular motion; and lays it down as a principal maxim, That the motion of every muscle is begun by a certain spirituous liquor passing from the nerves; but is accomplished, or finished by two other necessary causes which succeed it. He despairs that any one can ever arrive at the knowledge of the manner in which the soul acts upon the body, and therefore avoids troubling himself about it; but produces many reasons to shew, that whatever it is that gives motion to the muscles, must necessarily pass by the nerves. This leads him to inquire into the structure of a nerve; which, he says, “Is composed of a certain medullary substance full of juice, with a double membrane which involves that substance; and also an infinite number of little cords within these membranes and medullary substance, extended from their beginning to the very extreme capillaments”, which are dispersed and inserted into the parts of the muscle.

In speaking of these spirits, he says, That the alimentary juices abound with very subtle active particles; which, by their frequent circulation with the blood, are gradually freed from the terrestrial parts, wherein they were confined. These are in great plenty in the arterial blood; which, being carried through the arteries of the brain, deposits in it's medullary substance, by a slow kind of distillation, a fluid, which our Author calls a *Mercurial Liquor*, that is (says he) exquisitely impregnated with a *volatile salt* and *sulphur*, which flows from thence into all the nerves of the body, passing every way through them slowly, and at length falling into the veins by a gentle circulation, till they arrive again at the heart: and that, by these spiritous liquors, all the parts of the animal body grow very turgid, and are kept in continual agitation, assisted by the circulation, and the *calor nativus*. And this agitation is what he calls the *very life*.

And although this Author allows the nerves to abound thus with this rich *rectified juice*, yet he denies that they are regularly tubular, as authors affirm, but only as they are defined above; and also that any kind of cavity can exist in a muscle; and, consequently, that there can be no *inflation* of it's parts: but is of opinion, that in every muscle there

“*missas faciam*) quod in prioribus editionibus magno cum doctorum applausu, & sine ulla, quod sciam, clarissimi D. Willis querela, appositus reperiatur.” See the Geneva edition in 4to, 1680.

But Dr *Croune* refers to it himself as his own, in a paper, p. 25, intituled, *An Hypothesis of the Structure of a Muscle, and the Reason of it's Contraction*. [Read in the Surgeon's Theatre, anno 1694, 1695.]. This (says Professor *Ward*) is the substance, or heads only, of the Doctor's Discourses upon that subject, published by Mr *Hook*, in his *Philos. Collect.* Num. 11. Sect. 8. P. 22.; which, being afterwards translated into *Latin*, was inserted in the *Acta Eruditorum*, 1682, p. 194. with the title *De Motu Musculorum*. See that learned Author's *Lives of the Professors of Gresham-College*, p. 323.

are three kinds of spirits ; one peculiar to the *tendons* and their fibres, another to the *muscular flesh*, and another which comes to the muscle by the *nerves*.

These spirituous liquors, (says he) together with the membranes of the body, are the instruments of sensation also. For he concludes, that all the sensible membranes of every part of the body arise from the *meninges* of the brain ; and that they are all kept in a kind of tension, by these spirituous liquors passing constantly thro' them. In this state of tension or tone, he thinks that they may be compared to a glass, or bell *, whose parts have a vibrating motion communicated all over them, by being touched in one part. Thus, (says he) by the intermediation of the membrane of the nerve that belongs to any particular organ of sense, or by means of the one common membrane which involves the whole body, every object of sense is carried, as much as can be, by right lines, to the brain ; wherein the various and distinct motions of objects are perceived by the soul. Hence this ingenious author would endeavour to shew how, in a *paralysis*, sensation should remain when motion is lost, and the contrary : for that if that tone of the membranes should at any time be totally, or in part, destroyed, by either change of situation of their particles, or by the access of too much moisture, or any division of their continuity by an accident, that then, indeed, that vibration or undulation of particles, which causes sensation, would be interrupted ; like a cracked bell or glass, which, instead of it's agreeable sharp sound, exhibits a jarring disagreeable noise.

Our author defines a muscle, as consisting of an infinite number of *tendinous fibres* like cords ; which are so blended together at the extremities, as to resemble a thick cord composed of many others ; but that within the body of the muscle those fibres are at some distance from each other, and the spaces between them filled up with *flesh* ; which, with membranes, blood vessels, nerves, and innumerable lymphæducts, constitutes the entire muscle. This flesh, in the spaces between the fibres, he says, is nothing else than that portion of blood flowing through these interstices ; which, being condensed † by the closeness of the fibres, is detained between them, and constitutes the muscular flesh.

He has given an ingenious scheme for explaining the manner in which a muscle is moved, after having laid down the above *præludia* ; the sum of which is, That a certain power is determined from the brain, by the will, with these animal spirits, through the nerve to the muscle to be moved ; which causes the first tumescence of the muscle : and that the

* There seems but little analogy between such vibrating bodies and membranes. It is, indeed, the property of a bell or glass to vibrate and sound, when struck, provided it be pendulous, or otherwise free : but, if any part be touched, it's vibration and sound are impeded : how much less is a *moist* membrane in the body, capable of vibration ; since it is in close contact with other parts every-where ?

† It is to be feared, if blood could be evacuated, the least condensed particle would be sufficient to form, by degrees, abscesses, or some other mischief.

soul has an *imperium**, through the whole mass of blood also; as is manifest in the various passions of anger, joy, love, bashfulness, &c. whereby she is capable of determining the blood to any part in a greater quantity than ordinary, and, consequently, to the muscle to be moved: for, says he, it is not absurd to imagine, that the same *idea*, which excites the will to move a muscle, and the spirits in the nerve to perform it, in like manner is capable, at the same instant, of determining spirits to the heart, by the nerve which is inserted and dispersed thro' its auricles, and causing it to propel blood more copiously to the muscle.

The use he makes of this is, That a third concurring cause of motion in a muscle should be brought in, in order to render it more complete; and that is, a fermentation produced by the animal spirits of the nerves, and what he calls, the spirits of the blood; which he compares to that of any two chemical liquors mixing together: and that when this agitation is begun in the membranes of the muscles, the fluids will be driven, by their *nifus*, in right lines towards the extremities of the muscle; but that, finding the spaces much narrower in them than in the belly of the muscle, they are driven back to the middle into the muscular flesh, where the pores are larger, and more lax; which makes the muscle swell, by the particles endeavouring to recede from each other, and occupy a larger space; as, says he, happens in all fermentations: from hence, as the spaces are made larger in this muscle, there is room made for the access of more blood from the artery in the moving muscle. Thus muscular motion is performed (according to our ingenious author) by 3 conjunct causes; *viz.* *animal spirits* flowing to the muscle, *arterial blood* determined in greater quantity than ordinary, and a *fermentation* † raised by their admixtion, by which the muscle is swelled and shortened.

* It will appear in the next lecture, that the *soul* can have no *imperium* over the blood, and only presides over the voluntary actions of the body; for the heart drives the blood indiscriminately to all parts of the body, for its welfare, by a propelling force, which is involuntary: nor can the motion of the heart be accelerated immediately by the will, but only by some particular act of the body before; such as an increase of exercise by running, &c. And as to those passions of anger, bashfulness, Joy, &c. they first occasion quick and irregular respiration: this causes a quicker motion of the heart, which warms and rarefies the blood more, whereby it is driven with greater velocity to all parts, and among them to the cheeks. Thus blushing happens, not because the soul determined more blood than ordinary to them, but because its *momentum* is increased involuntarily by the shock or surprize previous to it: for the whole surface of the body is affected in the same manner, tho' most apparent in the cheeks. Hence no increase of blood can be determined to one part more than another; unless some impediment happens to its free circulation in one part, or the resistance becomes less in another; which we shall hereafter shew. But besides, one would be apt to think, if the *soul* had an *imperium* over the blood, she might as well prevent the effects of poison, or any other malignity in it, and correct the mass; as immediately determine it in a more than ordinary quantity to any particular part of the body.

† We shall find this learned author's *fermentation*, as it serves to account for muscular motion, seized on by most of those that followed him, without giving him thanks for it.

Our author has not only manifested (in his treatise) much learning, but also a penetrating *genius*, in many curious remarks and observations dispersed through the whole ; and merits the regard of the learned world no less than some authors we shall mention by-and-by ; who, by pluming themselves with *his* feathers, had monopolized much esteem and attention from mankind, by the exhibition of this system, with very little addition ; and may indeed be justly said to have led several of them, by his hints, into their most favourite notions concerning *muscular motion*.


Steno *, another ingenious author, wrote his *conjectures* on this subject *Steno*. much about the same time with Dr *Croune* ; but had a very different notion from what we have just mentioned concerning the latter ; and which, for it's particularity, shall have a place here ; especially as it will appear hereafter, that he laid the foundation, upon which *Borelli* raised his hypothesis, as to what regards the structure of a muscular fibre ; besides what assistance he has had from Dr *Croune* on other accounts.

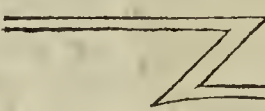
This author, after having expatiated a good deal upon the different structures of the muscles, and made several learned observations thereon, makes his general conclusion to the following purpose : 1. That arteries, veins, nerves, fibres, and membranes are the constituent parts of a muscle : and that, though some authors have pretended to have found lymphatics, he never could find any. 2. That there is no muscle whose fibres do not terminate in a tendon ; which are collected either into firm tendons at one or both extremities, or are diffused into membranes, or degenerate into an osseous rigidity. 3. That those very fibres (which, being closely connected together, compose the tendon,) when joined more loosely, constitute the flesh or carnosus substance ; and that therefore the flesh is not a particular *parenchyma*. In which sense a tendon is a continued body from the beginning of a muscle to the end. 4. That there is scarce any fibre in a muscle which constitutes a right line ; but every one is divided into 3 lines at least, which comprehend 2 alternate angles. Nor are these 3 always right lines ; but often the flesh, as in the *sphincters*, and sometimes the tendons, form curved lines. 5. These 3 lines of every fibre in the same muscle, are not always of the same length ; altho' there is scarce any difference of extension among them when acting all together : that these fibres are in the same plane ; and that, in this angular order, they form *oblique-angular parallelograms*, or *rhomboidal* figures ; whose two opposite parallels, in the place where they comprehend acute angles with the other parallels, are stretched to the opposite parts without the angles. Here he makes a remark, That in every tendon, tho' never so slender, there are as many filaments as there are fleshy fibres in the belly of the muscle ; and the interior filaments are shorter than the exterior. 6. His sixth conclusion is taken

* *De musculis & glandulis observationum specimen.*

up in the division of a muscle into extremities and it's middle, with some animadversions on that head. 7. He says also, That a membrane not only goes round each muscle with a transverse direction of its fibres, but that it also insinuates itself in the same manner between every muscular fibre.

When he comes to speak of the action of a muscle, he confesses ingenuously, that he cannot attempt explaining the cause or manner in which it is performed; yet makes a comparison towards an explanation, which he thinks not improper, but wherein, I must say, I cannot find any satisfaction. Imagine, says he, a machine for driving piles into the ground drawn up by several men, each having his particular cord which is fastened to the main rope: the *cords* imitate the tendons; the *weight* fixed to the cords, the moveable part; and the *men*, the fleshy fibres: for, says he, as men (being rendered shorter while they pull their cords) move the weight, so the fleshy fibres, being contracted, by drawing the tendons, pull the moveable part. There is no more understood by this comparison, than that the contraction of the fleshy fibres causes the tendons to come nearer to each other, which every-body has said; but the grand question is, How they are contracted? And as to this particular structure of the fibres, I believe none but the author himself ever fancied he saw them so. For, supposing this angular form in

every one  to be the real structure, the contraction

would reduce it to this form  But the cause and manner how this is brought about upon this plan is inexplicable; especially since much force is required to draw the weight or moveable part. But enough of this author: let us now proceed to give a short sketch of the opinion of a much more famous author, the great Dr *Willis*.

Willis. He commends the foregoing author very much for his ingenious conjecture about the structure of the muscular fibres; and gives a detail of what that author has said concerning them, with an air of approbation; but attempts accounting for the cause and manner of the performance of muscular motion, of which the following is the sum:

Animal spirits are carried from the *encephalon*, by the nerves, to every muscle; and are received by the membranous *fibrillæ*, and by these forwarded into the tendinous fibres; there to be reserved as in a proper receptacle or store-house. These spirits, as they are of a most active and elastic nature, as often as it is necessary, expand themselves, and fly swiftly into the fleshy fibres; and, having made their *impetus*, recede into the tendons, by turns. But, while these animal spirits pass into the fleshy fibres, at the proper instinct for performing the motion, they meet with very active particles of another kind supplied by the blood, which ferment together; so that, from their strife and agitation, the fleshy fibres which were before lax and porous, are filled up, and are forced

forced into corrugations; from which the contraction of the muscle proceeds. When the contraction is finished, the pure spirits, which remain, recede, for the most part, into the *tendinous* * *fibres*, the other particles remaining among the *flesh*; the blood supplying the expence of these, and the nerves of the others. And as to the instinct or disposition to the ordination of motion, our author thinks that to be produced by *other spirits*, sent out from the brain to the muscles when motion is required, which, by their various impulse, ordain those spirits, already placed in the tendons, to different motions either of expansion or recess. This is the scheme for the performance of muscular motion, according to this learned author; the chief part of which is the agency of fermentation; wherein there appears but very little difference between this and the doctrine laid down by Dr Croune. There seems however some impropriety in imagining, that, when fleshy fibres are filled or puffed up by fermentation, they should be subject to any kind of corrugation; it being rather an effect of emptiness than repletion in flexible bodies.

This hypothesis, hower ingenious in the whole, does not seem very satisfactory; and indeed it was soon taken into consideration by the famous Dr Mayow, who rejects both this and the opinion of Steno; urging many arguments against the sufficiency of either in accounting for *muscular motion*. Let us see how far he excelled them on the same subject himself.

This ingenious author † takes notice of two sorts of fibres; *viz. mus-* Mayow. *cular fibres*, and membranous *fibrillæ*: the former he describes with Steno; being satisfied with what that author has said about them, as to their structure: — the latter, being the *fibrillæ*, he says, are a wonderful series of fibres parallel to each other, and which intersect the fleshy fibres in a transverse direction, yet somewhat obliquely. This observation he has made upon boiled muscular flesh; and says also, that altho' it had been the opinion before him, that contraction was performed by the muscular or fleshy fibres, yet it is his opinion, that the *fibrillæ* are principally concerned in that action: for that, in order to a due contraction, if it was made in the fleshy fibres, they must of necessity be much more shortened than the intire muscle itself; because they are not disposed according to the longitudinal direction of the muscle, but are inserted obliquely into the tendons: and also that the muscle would swell to an immense size; which, he says, does not happen upon muscular motion. Hence he concludes, that the motion is performed by the membranous *fibrillæ*; which, being contracted, draw the fleshy fibres more closely together, and render the whole muscle shorter and more hard.

* Here are clearly Dr Croune's three kinds of spirits; those in the *muscular flesh*, those of the *tendons*, and those sent from the *brain*; besides the fermentation they produce by meeting in the belly of the muscle.

† *Tractatus quinque medico-physici. Vide De motu musculari, cap. 2, 3, 4, &c.*

It is remarkable, that in *Plate 3. Fig. 2.* of this author, the figure he gives to represent the situation of the fleshy fibres, and the *fibrillæ* that intersect them, is copied by Dr *Stuart*, with this difference, that *Mayow* calls the minute intersecting fibres, membranous *fibrillæ*; whereas the former calls them nervous *white fibrillæ* *; and, in his 3d figure, supposes each carnous red fibre to have a chain of *vesicles*, which represent a string of beads or *necklace*. Now, as to these *vesicles*, our author seems to have given the hint to some of those that wrote on the same subject afterwards: for, where he speaks of the manner of the circulation of the blood thro' the muscle, against some opinions before him, "that it is extravasated from the arteries, and absorbed by the veins," he says, That the veins and arteries meet by the intermediation of vesicles, which he calls, *Collectio vesicularum sanguiferarum* †; that no such extravasation can happen, and that this collection of *vesicles* constitutes the chief part of the muscular flesh; and also, that their chief use is, like a strainer, to separate from the mass of blood certain particles, necessary towards the contraction of the muscles. This, if maturely considered, will be found to square pretty much with *Keil* and *Stuart*, as to their *vesicles*; as will hereafter appear, when we speak of these several authors.

He also agrees in other respects with some of his predecessors; particularly about the necessity of an effervescence, being raised in a muscle, necessary to it's motion; occasioned by the admixtion of particles of different natures, according to the power of the will; which proceed from the brain and mass of blood. Those from the brain, he says, are *nitro-aërial* particles, and the true animal spirits; and those from the blood he calls *salino-sulphureous*; and that the former, being sent by the nerves, meet the latter in the muscle wherein they are secreted, as was said before, and make the emotion and fermentation, which is the cause of muscular motion.

As to his manner of muscular motion's being brought about, it seems to be intirely his own invention: he denies that it can be performed by inflation, either of the muscular fibres, or *fibrillæ*; the latter of which, according to him, are solid bodies, and can undergo contraction no other way than by twisting or *contortion*; and that those *nitro-aërial* particles are very fit to affect the *fibrillæ* in that manner. To prove

* These are no more than the fibres of the membrane that invests the muscular fibres mentioned by *Steno*, and which really exist; for, in tearing asunder, with one's fingers, the fibres of a boiled muscle, they are very apparent; and seem to be what *Bernoulli* imagines to bind his fleshy fibres at equal internodes, of which more hereafter, being first thought of by this author, (*Mayow*) as agents in muscular motion.

† Which indeed do not exist, but are however first thought of by this learned author; who also finds it necessary to bring to his assistance the fermentation of the founder of these lectures; yet differs from him as to the nature of animal spirits; the latter giving them the name of a liquor exquisitely impregnated with a volatile salt and oil; and the former calling them *nitro-aërial particles*, which ferment by mixing with the *salino-sulphureous* particles of the blood: both which terms seem to be the invention of Dr *Mayow*.
this,

this, he brings the following experiment : let a small string of a musical instrument be held between the fingers of each hand, at a considerable distance from each other, over a lighted candle, so as that it may become sufficiently heated without burning : when thoroughly hot, it will be perceived to contract with a considerable force, by twisting itself ; and, moved from the candle, will be easily distended again, by untwisting. Thus, says he, the *nitro-aërial* particles issuing from the candle are the cause of the contraction of the string ; as they are of the *fibrillæ* being writhed and shortened about the muscular fibres in the body ; which being by that means drawn closer together, the whole muscle is shortened. This experiment, he says, is the more to be relied on, because, by microscopical observation, he pretends to have found these *fibrillæ* exactly like a fine string of a musical instrument. From this system he concludes, that, as some force is necessary to distend the string to its former dimensions after contraction, if no force is applied, it will always remain contracted ; and that therefore, when there happens a *paralysis* of a muscle, it's antagonist is convulsed, or is spasmodically affected. Thus much is sufficient to shew what our author's notions were concerning this difficult subject. The next we shall consider is the famous *Borelli* ; a man famous indeed for his calculations of the powers of moving bodies, but much less so for his account of the cause and manner of the motion of muscles. The following is the substance of his opinion concerning that particular.

He supposes *, that, within the membrane which invests a muscle, *Borelli* the *fasciculi* of muscular fibres have a prismatical † form ; which is sometimes triangular, sometimes square, and sometimes hexagonal ; each of which is composed of many filaments, or tendinous fibres, which are parallel to one another in every little bundle or fascicule, and adhere together by a tenacious *gluten*, if they are not continued to the extremities of tendons, or membranes ; and sometimes are immediately connected to bones, or carnous fibres.

That besides, these bundles are every-where invested and bound together by innumerable transverse fibres ‖, as it appears in a muscle boiled, and immediately dried ; which nervous fibres seem to compose certain reticular membranes, together with the capillary vessels § that

* Johan. Alph. Borelli *De motu animalium*, pars prima, c. 2. Prop. 1. & c. 17. Prop. 114, 115, 116. Pars altera, c. 3. Prop. 22, 23, 24, &c.

† Our author thought so, because he made his observations upon a dried ham ; which, having it's moisture exhaled, and it's fibres being collected, by the salt used in preserving it, into bundles, will flake off in parcels when boiled. But it must be obvious to every one, that salting, or boiling, will alter the contexture of fibres so minute ; and, consequently, that no great truth can be drawn from such observations.

‖ These are no other than the fibres of the investing membrane mentioned under *Mayow*.

§ This reticular structure is made use of by Dr *Stuart* in his explanation of muscular motion ; which see in his turn.

bring blood to them, and carry it back again ; and that these fibres are nervous, he conjectures from their being very hard and tough.

His description of a muscular fibre, which he has observed after being boiled, is, that it seems, by the help of a microscope, to be a *cylinder* like the twig of a tree, not hollow, as a reed is, but is observed to be full of a medullary substance, which ought to be spongy like the pith of elder ; because every soft twig, which is filled with any adventitious moisture, grows turgid, and is necessarily porous, since it is filled with particles of water as with wedges, as it appears in a wet rope. He seems to be further confirmed in this notion, because he observed, in the fibres of a piece of *dried ham*, certain sanguineous particles, or strait and transverse filaments, dispersed like *porphyry* or marble ; which, says he, seems not possible to be so, if the internal substance of the fibres were not spongy.

From his notion of this spongy contexture of the internal substance of a muscular fibre, he is led further to *imagine*, that it consists of pores of a rhomboidal figure, so as to resemble a chain of *rhombus's*, which are capable of contraction like so many bows, by the help of the *moving faculty* ; and that each of these *machinulae*, or rhomboidal pores of the fleshy fibres, are so minute, that their length does not exceed $\frac{1}{10}$ of an inch. Hence his definition of a muscle is, that it's texture is like a reticular bundle, composed of rhomboidal chains, contiguous to each other.

A nerve, he says, is a bundle or capillament formed of a number of fibrous threads, connected together by a membranous binding ; and that every fibre may be hollow like a blood-vessel, altho', from the imperfection of our sight, they may seem solid : yet, if it be not impossible, that they may be tubes, he had rather believe them little tubes filled with a moist spongy substance analogous to green elder, or the like ; because the nervous fibres are not only soft, flexible, and moist, but also because they admit humid nourishment, and a fluid drops from them ; all which properties require spongy porosities bedewed with some liquor.

As to the manner and cause of muscular motion, he allows, that a spirituous liquor, which in some places he calls *substance*, or *faculty*, passes by the nerves from the brain to the muscle ; and that some fluid, proper to the muscle, meeting it, something like a fermentation or ebullition is excited, which causes that sudden swelling of the muscle. The examples he gives to render this familiar to the reader, are the spirit of vitriol poured upon oil of tartar, or any acid spirits mixed with fixed salts. Hence we see from whom this author has been furnished with his opinions concerning muscular motion ; *viz.* the *rhombus's* from *Steno*, with a very little difference ; and the fermentation from the memorable *Croune* ; notwithstanding his being frequently quoted as the real inventor of these notions.

The celebrated *Bernouilli* *, tho' confessedly a follower of *Borelli* on *Bernouilli*, this subject, as it appears in his own words, where he says, "In which (account of muscular motion) I shall tread in the track of the incomparable *Job. Alphonfus Borelli*, embracing his hypothesis:" Yet blames him for imagining that the *machinulae* in the muscular fibres were of a rhomboidal figure; and will appear to have made no other alteration, even in what he finds fault with in that author's *rhombus's*, than to cut off their lateral angles; and by that means reduce them to elliptical forms bound together, at their extremities, very regularly by transverse fibres.

The account he gives of the structure of muscular fibres is the same with that of the foregoing author, so needs not be repeated here: we shall therefore only give the reader an abstract of some sections of his *Dissertation on Muscular Motion*, which regard the part of this subject we are at present concerned in.

In sect. 2. after he has mentioned *Borelli's* inspection of a boiled muscle, he says, The fibres of the little bundles are collected together by transverse fibres, which are parallel to each other, and form with the former a reticular texture; to which he ascribes no other use, than to confine the moving fibres, lest, in performing their proper action, they should be forced too far asunder † from their natural situation. Thus every moving cylindrical fibre, by the help of this ligature, is divided into equal *internodes*, forming *vesicles*, which are flaccid when the muscle is inactive, but when acting, are distended, acquiring an oval figure like the rings of a chain, which, says our author, *Borelli* falsely calls *Rhomboidal Machinulae*: and also adds, that these transverse ligatures are loose enough to admit a free communication for the moving matter to fill the vesicles.

Sect. 3. is chiefly taken up in the confutation of *Steno's* opinion, That muscular motion is performed without the access of any thing whatsoever, but by the sole disposition in the fibres to change their figure, from an oblique-angular parallelogram into a more strait one; and opposes to it that common physical axiom, "*Omne quod movetur, movetur ab alio.*" And, in sect. 4. assents to Dr Croune, with *Mayow*, *Willis*, and *Borelli*, that motion must be caused by a fermentation raised in the muscle; believing also with the latter, that the nerves are a congeries of tubes filled with a spongy substance, which are always full of a very spirituous juice, supplied by the brain, of such a nature, as (when mixed with the blood) to be capable of raising a fermentation: this is the animal spirit. So that, when the soul performs an act of volition, there

* *Dissertatio de Motu Musculorum. Vide Proëm.*

† We must here assent to *Bernouilli*, that the membranous fibres, which not only inclose every muscle, but proceed to invest every fibre, serve only to keep them in their natural situation; but that they divide them thus into bladders at equal internodes, we must deny; inasmuch as we have been so happy as to separate distinct muscular fibres, and demonstrate them before the R. S. and to many Anatomists since these Lectures were read.

must of necessity happen a certain local agitation of animal spirits in the brain, from the great union between the soul and body ; whereby the beginning of some nerve receives an impulse, which is continued to the juices thro' it's whole length : and so, from an irritation at the beginning of the nerve, the last drop of the nervous juice is, by a gentle vibration, thrown out at the other extremity, and also from the little mouths of every other nerve dispersed through the whole muscle in the same manner, according to the power of the will. And as a sponge filled with liquor suffers not a drop to fall out, so, says our author, altho' those little mouths of the nerves (*in the muscle*) are always open ; yet the drops of the nervous juice never fall from them, without an actual impulse or concussion ; because the spungy substance of the nerves serves them instead of valves *.

When therefore, says he in sect. 5. by the command of the will, or from nature's custom (in involuntary motion), innumerable drops are thrown out together from the orifices of the nerves, through the entire bulk of the muscle, which is always thoroughly moistened with blood ; then these spirituous particles or drops, by striking their sharp *spiculæ* into the more fine particles of the blood, break them, and give the confined condensed air room to expand itself, and cause the subsequent ebullition and inflation of the muscle.

But, because an objection might lie against this system, as, how it comes to pass, that the muscle should so suddenly grow slender, and reassume it's former state ; since, according to our author's doctrine of fermentation, it should seem, that, after the first ebullition, the muscle ought to remain constantly swelled ; he proceeds to remove that obstacle in sect. 6. by having recourse to the following hypothesis : let us suppose, says he, besides the thick air we breathe, another more subtile air ; which, however elastic, can by no means be perceived, as being capable of penetrating freely all the pores of the body. He thinks this supposition not at all absurd, as believing that there is other matter of different degrees of subtility, between this grosser air of the *atmosphere* and the *materia subtilis*, least there should be a *chasm* in any part of nature ; and thinks it must be elastic, from the same cause that that of the atmosphere is ; to wit, from the continual motion of the *ethereal matter*, which always endeavours to drive the less agitated and gross particles from each other, and thereby obtain for itself a free passage.

This being pre-supposed, says he, the particles of the spirituous juice of the nerves are so very subtile, delicate, and tender, that their *spiculæ*, which at the most gentle touch are blunted, are only capable of opening the most minute pores of the particles of the blood, from which immediately that more subtile elastic *aura*, that was condensed before, rushes forth, and expands itself, thereby swelling the whole at once ; but, because of the exceeding minuteness of it's particles, it freely breaks

* This is also taken from *Borelli*, where he speaks of a nerve.

out through the open pores of the muscle, and flies off into the ambient air. Hence the muscle must of necessity grow lax in a moment after its intumescence ; unless new drops of the nervous juice continually fall into it, creating a succession of fermentations, and so keep it in a constant state of inflation *.

This is the sum of the opinion of that great genius the famous *Bernouilli*, touching the manner in which muscular motion is performed : the next is that of one as eminent, the learned *Keill*, but whose sentiments of the matter agree well with those before him. He defines a muscle to be “ a bundle of parallel plates of fleshy fibres, which are
“ composed of other smaller fibres, and each smaller fibre to be a string
“ of bladders or vesicles, into which, he supposes, the nerves, veins,
“ and arteries to *open*. That the contraction, or swelling of the mus- *Keill.*
“ cles, is performed by the blood and animal spirits distending these
“ vesicles ;” but endeavours to prove, by many ingenious arguments, that neither the spirits alone, nor the quantity of both together, distend the vesicles ; but that both mixing and rarefying † together, cause them to swell : for, “ That the globules of blood continually circulating
“ through these vesicles of the fibres, which are, probably, capable of
“ containing only one globule at a time, in which globule (he supposes
“ a globule of air) meet with the animal spirits which drop from the
“ nerves : that the spirits surrounding the globule of blood must attract
“ the particles of it, of which they are composed, more strongly than
“ the others of the globule of blood ; and, consequently, their *nifus* to
“ one another ceasing, the condensed globule of air will expand itself
“ with a very considerable force ; whereby each vesicle of the fibre will
“ be distended, and, consequently the fibre shortened, or the whole
“ muscle will be contracted.
“ But, when the particles of the globule of blood are mixed with the
“ nervous fluid, they will both together inclose the globule of air again,
“ and compress it into as small a space as it was in before : and thus
“ the contraction of a muscle must immediately cease, unless fresh blood
“ and spirits, succeeding one another, continue the swelling of the
“ vesicles ||.”

This

* We shall find the following author says no more than *Bernouilli*, explaining it only a different way.

† What the foregoing authors call fermentation, effervescence, &c. this author calls rarefaction. *Bernouilli* endeavours to explain it by the *spiculæ* of the nervous juice striking against the finer particles of the blood ; and this author, by the attraction and *nifus* between the animal spirits and the drop of blood when they meet in the vesicle : so that, in the whole, they may be considered to speak the same thing (and even not to differ much from their predecessors) ; for it is no great matter whether the globule of air is freed from it's confinement, by *spiculæ* opening the pores of the blood, or by the attraction of particles to each other.

|| The only difference that seems to be between this and the foregoing author is, that the *aura*, after being let loose, and swelling the muscle, flies off into the open air, according

This system, however ingenious, as it admits of so many bare suppositions, upon which these authors found their arguments, it will be difficult to think it the true explanation of muscular motion: but admitting every supposition to be true, yet the time that this kind of *nifus*, *attraction*, *rarefaction*, *fermentation*, &c. must necessarily take up in the performance, can no way be accountable for the quick motions performed by the muscles of the organs of speech, the twinkling of eyelids, or any others that are done as quick as thought, no more than it can be reconciled to the nature of involuntary motion, in those muscles that are not subject to the impulse of the will.

His explanation of rarefaction, admitting we expected no use of it in muscular motion, seems to be very particular too; for we can scarce have any other cause in view for rarefaction or condensation, than *heat* or *cold*; which are sufficient of themselves to produce those effects in the most *homogeneous* fluid, without the admixture of any other. Whether rarefaction can be effected by any other cause in the body, will be hard to determine, if we do not suppose those spirits to be an acrimonious fluid, capable of causing a commotion with the globules of blood in the vesicles, which would amount to a *fermentation*; but our author says all is done without fermentation, by this methodical *nifus*, and attraction of the particles of the spirits to each other.

Quincy.

I must here insert another very remarkable notion propagated by Quincy, from a hint of *Bellini*, as it appears in his explanation of sect. 2. aph. 8. of *Sanctorius*, where he owns his being led by * *Bellini*, in his notions of the structure and power of a distractile fibre. He supposes the parts of a distractile fibre to be made up of certain *machinulae*, like *syringes* and their *embolus*'s, and their motion to be analogous to that of the instrument mentioned. “ The observations and experiments (says “ he) which have been made of late, but more particularly by Mr *Boyle*, “ about the spring of the air, have explained to us the contrivances and “ properties of a *syringe*; and the reasons upon which that phænomenon of the difficulty of drawing back the *embolus*, when the pipe is “ stopped, depends; and the necessity of any liquor's following it, “ wherein the pipe is immersed: the reason of which being well considered, it will be found, that all which is necessary for this contrivance is, that the *embolus* be so exactly adapted to the inner surface “ of the barrel, as to prevent any air passing between them when it is “ drawn up; and that it matters not what figure the barrel is of, so that

according to *Bernouilli*, and the muscle ceases to swell: whereas our present author says, his globule of air is again condensed into as small a space as before, and the swelling and contraction of the muscle ceases: however, their conclusion is the same for the continuance of the motion or swelling; this author requiring fresh blood and spirits succeeding one another; and the foregoing author the same, in these words:—*Musculus iterum detumescat, nisi jugiter novæ instillentur guttulæ succi nervosi, quæ novam & novam pariendo ebullitionem, musculum in continua inflatione conservent.*

* *Opuscula de Villo contractili.*

“ the

“ the *embolus* is well fitted to it. It easily therefore might be contrived
 “ to make a case of syringes, wherein every barrel may also serve as an
 “ *embolus* to it's exterior, which immediately includes it. And, with
 “ this view, it is not at all difficult to imagine a continued series of
 “ particles so put together, that the inner may be moved and drawn
 “ upon one another, without suffering the air immediately to enter into
 “ the interstices made by their distraction: whereupon, as soon as that
 “ force which drew them is removed, they will, for the very same rea-
 “ son as the *embolus* of a *syringe*, rush up again into their former con-
 “ tacts.” This is his notion of the arrangement of the particles com-
 posing the main substance of an animal fibre, and of it's construction.
 Now it is no difficult matter to overturn this system, since there are 2
 principal objections to be made to it, which prevent the trouble of any
 further consideration. The first is, That this absolutely is not the struc-
 ture of a muscular fibre, as we shall make it appear in the following
 Lecture: and, secondly, There is a great absurdity in the comparison
 he makes between the action of a *syringe* and it's *embolus*, and that of a
flexile fibrous thread: for, as it is necessary, in the action of that instru-
 ment, that it's sides should be inflexible, hard, and able to bear the ex-
 cessive force of suction in the retraction of the *embolus*, and the force
 of the air in it's repulsion, with how little propriety can this minute
 supple fibre of a muscle be said to be capable of a like action, or such
 a wedge-like construction as this author thinks applicable to it. When,
 in mechanical reasoning, we find it necessary to illustrate an opinion by
 a comparison, there ought to be a true similarity of structure in the ob-
 jects whose actions are to be compared; otherwise it would be altoge-
 ther as prudent to liken a human blood-vessel to a blunderbuss.

The ingenious professor * *Monro*, of *Edinburgh*, supposes the ner- *Monro.*
 vous fluid to be “ an extreme fluid saponaceous water, flowing in a
 “ constant, equal, *slow stream*, from the *encephalon* and *medulla spinalis*,
 “ in each of the proper nervous fibres, &c. That it is fit for nourish-
 “ ing and restoring the particles that are constantly carried away from
 “ the solids by the *vis vitæ*, circulation of the liquors, and necessary
 “ actions of life; and that the constant flow of the liquor of the nerves
 “ into the cavities of the muscular *fibrillæ* occasions the natural contrac-
 “ tion of muscles, by the constant *nisus* it makes to increase the trans-
 “ verse diameter, and to shorten the longitudinal diameter of each fi-
 “ bre; and that it is only to allow the mind a power of pouring a
 “ greater quantity of this fluid, with greater velocity, into what mus-
 “ cular fibres it pleases, to account for the voluntary strong action of
 “ muscles.”

To this opinion we will here subjoin that of Dr *Bryan Robinson* †; *Robinson.*
 who, after taking much pains to prove, that a hair or fibre decreases in

* Anatomy of human bones and nerves, p. 29 of the latter.

† Dissertation on the *Æther* of Sir *Isaac Newton*.

breadth as it increases in length, and *vice versa*, concludes that, “ if a
 “ vibrating motion be raised in the *æther* contained in the fibres of
 “ muscles, the diameters of all the fibres will be increased, and their
 “ lengths lessened by that motion : all therefore that is necessary to give
 “ animals a power of moving their own bodies, is to give them a
 “ power of raising a vibrating motion in the *æther* contained within
 “ the fibres of the muscles. And this power they have by the media-
 “ tion of the nerves, which are solid uniform threads, arising from that
 “ part of the brain to which the soul is present, and terminating in the
 “ muscles : for, a vibrating motion, raised, by the power of the will,
 “ in the *æther* contained in that end of a nerve which terminates in the
 “ *sensorium*, or place in the brain to which the soul is present, will, in
 “ an instant, be propagated to the muscles supplied by that nerve, and
 “ raise a like vibrating motion in all it's fibres, from the very great
 “ communication there is between the nerves and the fleshy fibres of the
 “ muscles.”

As to what Prof. *Monro* says of the component parts of the nervous fluid, it would be fit for the purposes he assigns, by a good analogy drawn from plants, *viz.* to nourish and supply wasted particles, as well as to propagate muscular motion ; provided it was of a consistence subtile enough to pass through those extremely minute tubes, and that the mass of blood was not the only *pabulum* designed by the Creator for affording nutrition. There is great need, in whatever fluid the nerves contain, of a capacity of acting with the greatest celerity imaginable ; and one would think a mixture of a saline and oleaginous matter not the most fit for such active performances as the will sometimes determines, and some of which are as swift as mere explosions. However this be, our ingenious author has not so much made it his business to account for the manner in which this fluid causes the muscular fibres to increase and swell, in order to the contraction of the muscle, as to give the true description and situation of the nervous system ; than whom none has done it better.

And as to the opinion of Dr *Robinson* concerning the *æther*, it can hardly give satisfaction : for supposing this *æther* to be the cause of muscular motion, there must be a *modus actionis* to produce the necessary effect ; for it is not enough to say a vibrating motion causes the muscle to contract, without making some attempt to shew in what manner it does so. We can understand as much, by saying at once, The animal spirits cause the muscle to move ; which was said many years ago.

But, from a very natural and obvious argument, one may venture to doubt, whether the *æther* can have any share in muscular motion ; for there can be no motion, whatever, mechanically performed in one body, without first receiving a certain propulsion from something else ; and the body making that propulsion ought to be endowed with qualities necessary for such a resistance, proportioned to the body acted upon : now, if the *æther* (as Philosophers have defined it) be a *subtile matter*
 pervading

pervading all substantial bodies, of whatsoever solidity, without lett or hindrance, it cannot be said to cause muscular * motion, since it can meet nothing to oppose it's passage; and, consequently, can make no resistance.

The last opinion I shall trouble you with is that of my predecessor in *Stuart*. these Lectures, the late learned Dr *Stuart*; who (in his explanation of the experiment upon the frog, to which I refer the reader) concludes, "That voluntary muscular motion is begun by the impulse of the mind or will on the animal spirits, thro' the nerves into the muscles."

He supposes (with *Keill*) the structure of a muscular fibre to be vesicular, with a reticular *plexus* of blood-vessels investing each vesicle: his particular definition of it is this: "It is a nervous fibre, produced from it's entrance into the muscle, along, or in the axis of each carnosus fibre, in the form of a chain of distensible vesicles, whose sides are covered with a net-work of elastic longitudinal and transverse blood-vessels, &c." Here he makes a difference between the nervous fibre in the form of a chain of vesicles, and the carnosus fibre, along whose axis it is produced; whereas Dr *Keill* says, each fibre is a string of vesicles. Our author endeavours to explain his system by these little longitudinal and transverse blood-vessels on the surface of each of these vesicles, which he calls the reticular *plexus*. I must confess I do not

* Whatever necessity there is for the existence of the *æther* in the oeconomy of the world, we can hardly suppose it either the *cause* or *instrument* of muscular motion; for the *soul*, or *mind*, seems to be the very cause of voluntary motion: and as to it's being instrumental, it will be extremely difficult to reconcile the qualities of the *æther* to those of the parts we shall, in our next Lecture, endeavour to prove are the instruments of that motion.

The excessive distance between the subtilty of this *element*, and the grossness of the *nervous* and *muscular system*, upon which they are, by this author, supposed to act, will admit of no proportion: wherefore, to suppose the nerves can be impressed by the *æther*, we must pre-suppose millions of gradations of grosser particles from the extreme fineness and subtilty of this, down to the visible grossness and solidity of the organs to be moved, in order, as it were, to hand down the impulse from the extreme subtile to those extreme gross particles: all which raises in me so complex an idea of that impulse, that I cannot find it compatible with the great quickness of both the resolution and impulse we daily see in the performance of animal motions.

There is another argument, which seems very powerful against the *æther's* being instrumental in muscular motion, drawn from some considerations on the *fire* produced in the *electrical experiments*, now verified by Mr *Watson*, provided this *electrical fire* be analagous to the *æther*; which is, That it is certain, this fire pervades animal or other bodies, from my own experience; as I was one of several persons through whom it passed, without having any sensible effect on me, in it's passage to the point where it was collected into a body: and also, because silver *laminæ* were moved by the excited tube, even thro' the sides of a strong flint-glass vessel well stopped.

It seems, indeed, a very great care in the All-wise Author of Nature, that this *electrical fire* should not find resistance in animal bodies; because, we see, when all that passed from the excited tube, through several persons, was collected in one column at the end of a Gentleman's finger, it fired rectified spirits of wine, and oil of orange peels; and, consequently, might produce as direful effects as the fire of *lightning*, when collected and excited to violent motion, and is resisted.

well comprehend his meaning; however, the sum of his account is as follows:

“ In the utmost state of extension (of a muscle), the longitudinal capillary blood-vessels on the surface of each vesicle in the fibres must be extended, and therefore their transverse diameters must be lessened; that is, these vessels thereby become straiter, and the circulation in them therefore more difficult; and in this state also the transverse blood-vessels of each vesicle will be forced into serpentine flexures, which must render the passage of the blood through them still more difficult.

“ If the mind impels but a little more of the nervous fluid than usual thro’ the slender tubes of the nerves into these extended vesicles, they will be uniformly dilated.

“ By this distension of the vesicles, their *axes* being shortened, and their diameters lengthened, the longitudinal capillary vessels in their surface must be shortened, and thereby their diameters enlarged, and the serpentine flexures of the transverse vessels will be extended; which, in both kinds, will lessen the resistance they gave to the *transit* of the blood; which, by the *diastole* and *systole* of the arteries, is continually urged on to its passage through them; and, being thus facilitated, every globule of blood in its progress, by endeavouring to fly off by the tangents of these vessels and vesicles, tends to expand them more, and thereby opens the way for the further and easier influx of the nervous fluid, to which the blood-vessels contribute, as so many elastic levers acted upon by the blood in its progress. Thus, by the assistance of these three powers, the nervous fluid, blood, and blood-vessels, the progress from extension to the *diastole* of the vesicles is made, by which the muscle becomes tumid and enlarged in bulk, &c.

“ But, if the mind desists sending this recruit, or suspends it, then these circular arched elastic vessels, now turgid with elastic blood, whose *area’s* have been thus forcibly enlarged, endeavour to contract themselves every way towards the centres of their *area’s*, which are the centres of the vesicles; and the mind giving no resistance, this *nifus* takes place, to the complete contraction of each fibre; by which means the limb affixed is brought into complete flexion, or extension, according as this or the other antagonist has been acted upon.

“ In this state the whole muscle becomes shorter and less in all its dimensions; harder and paler by expulsion of a great part of its fluids thro’ the veins towards the heart, and through the extremities of the nerves into the tendon and *periosteum*.”

Here he supposes the spirits to fly off to the tendons and *periosteum*.

None of all these authors have considered the state of a muscle when at rest; which has contributed not a little to assist my explanation of muscular motion. Our author has nothing in his view, but the bare progress of the motion from the utmost extension to the utmost contraction

traction of a muscle ; and, in the explanation, the whole progress seems intricate and tedious. Again he supposes those longitudinal and transverse blood-vessels to be on the surface of the vesicles, which do not appear by any assistance of the microscope we can apply.

He begins this explanation with the utmost extension of a muscle ; and endeavours to prove, that, in it's progress to contraction, the whole muscle must grow larger in bulk every way, by shortening the *axes*, and lengthening the diameters of the vesicles. This he calls their *dias-tole*, which happens from the mind's impelling more than ordinary of the nervous fluid ; but says, That, when the will ceases that recruit, the complete contraction follows by the *nifus* of the circular blood-vessels towards the centre of their vesicles.

Now one would be apt to conclude, from this way of reasoning, that the vesicles ought to lengthen again by the restriction of the circular blood-vessels ; for, as their diameters were lengthened by the contraction of the longitudinal ones, so, by the contraction of their transverse or circular fibres, the length of the vesicle ought to be increased again ; and, consequently, the muscle ought to return to it's state of extension.

Here is one short particular more, that seems unintelligible, with which we shall close this introduction : he applies his experiment on the frog, to prove, that the complete contraction of the muscle is excited from the *impulse* of the animal spirits into it ; whereas here he says, " If the mind *ceases* her impulse, the *nifus* of the circular fibres of the vesicles takes place, to the complete contraction of the muscle." I submit the consideration of these different obscure accounts to understandings more penetrating than mine ; and shall take an opportunity of offering my own attempt towards an explanation of muscular motion.

There is not the least motion performed in any part of an animal body, which does not depend on a muscular structure for it's progress, whether in the fluids or solids, voluntary or involuntary ; and therefore whatsoever explanation of muscular motion is not reconcileable to, and accountable for, every motion performed in any part, cannot be the true explanation.

LECTURE II.
Containing the
Author's
scheme of
Muscular Mo-
tion. Read in
February,

The most minute muscular fibre *, that I was able to separate, seems to be tubular, but unequal ; that is, having some parts of it more protuberant than others, so as to resemble as many sailors *hammocks* one after another, and much in the same proportion in general : we shall call these bellies or hammocks, *cells*, for the better explanation of the subject. Now though it is a received opinion, that fibres are divisible *in infinitum*, that is, that each fibre is composed of others, and those

1743-4.
Of a muscular
fibre.

* A muscular fibre of a middle size is about equal to the hair of a child's head newly born, at the strictures or smallest parts ; the *cells* being thicker according to the proportion at Fig. 125, 126, 127, &c. Yet some are larger, and some smaller ; the *cells* however are not to be discerned with a glass of a less magnifying power than the fifth of a double reflecting microscope.

again of others, and so on ; yet this *muscular fibre*, as it is a *tube*, and serves to perform an office peculiar to itself, it cannot be divided, without destroying it's necessary form ; as a trumpet, or any other hollow vessel, cannot be divided or cut into other trumpets, &c. (let it's size be what it will) and is therefore, properly, an *ultimate tube* or *trumpet*, as this tubular fibre is an ultimate *muscular fibre* ; and this is the case with the *nervous tubuli*. Indeed the *parietes* of every *muscular* and *nervous* fibre may be composed of fibres divisible, for what I know, *in infinitum*.

These cells are not regularly alike, some appearing three times longer than others, nor at the same distances from each other ; nor do the fibres themselves seem all of a size, and consequently each fibre contains some more, some less of these cells.

The cells communicate with each other by a passage through the whole length of the muscular fibre, till both extremities terminate in a tendon or otherwise ; which appeared upon many experiments both while the muscle was moist, and after drying a little, having separated them in both cases ; but after being boiled or roasted, every fibre, I examined, seemed to have lost it's cells, and become uniform ; whence one would be apt to think the cells had burst by the rarefaction of the *inflating matter* they contained, and their sides become uniform with the parts that were, before, more slender.

And as these cells communicate one with another, it may be reasonable to suppose, that there is no necessity for any more than one nervous tube to each muscular fibre ; so that the number of nervous fibres in that bundle that goes to a muscle need only to be equal to the number of muscular fibres that compose it. From this structure it is easy to conceive, how the muscular *cells* are capable of being shortened, by their being inflated * and increased in diameter : and it is to be further observed, that the bellies or *cells* of neighbouring fibres do not lie regularly by the sides of each other, but promiscuously ; that is, the cells of some lie close to the slender parts of others, and sometimes two slender parts lie together, and sometimes two cells.

Of a nervous
fibre.

The smallest nervous fibre I was able to separate seems to be a tube ; therefore a nerve may be defined a bundle of uniform tubes, whose sides are parallel to, and in contact with each other, beginning in the *encephalon* and spinal marrow, and terminating in muscles, membranes, *cutis*, &c. for the propagation of motion and sensation †.

But

* The authors, mentioned in my first Lecture, make use of the word *inflatio*, and it's verb, to signify a repletion of the fibres, or their supposed *bladders*, *rhombus's*, &c. with blood and spirits, fermented together. On the contrary, I apply it to signify a blowing up of the muscular *cells* with an elastic *aura* only, denying that any blood or spirits can get into them.

† Altho' I think it not the business of this Lecture to touch upon sensation, yet I am induced in this place to make the following reflection : if the *soul* dwells, or exists in every

But because they have not been thought tubular by some ingenious men, it will be necessary to give some reasons for our thinking them so. If they are not tubes, but solid strings, there is no way of accounting for the beginning of muscular motion, but by their vibration: now nothing can be said to vibrate that is not elastic, and first in a state of tension; but, from known experiments, there is no elasticity in the nerves; nor can any Anatomist say he ever found a nerve in a state of tension.

The GREAT CREATOR seems to have wisely avoided any tension in a nerve, for several probable reasons: 1. The origins of the nerves in the *encephalon* and spinal marrow are a mere pulp, very soft and tender, before they unite to form the nerve, and very loose in their contexture; so that a vellication in any one, produced by tension, would pull it out by the roots, and would disorder the parts of it's insertion too: 2. Supposing the places of the origin and insertion of a nerve to be firmly fixed, and in no danger from vellication, there could still be no tension in the nerve, because no nerve could be brought into right lines; the very structure of the parts, through which they pass, would hinder it; for the course of the nervous trunks of the limbs, &c. being along the interstices of muscles, &c. if a tension was produced in them, the sides of the muscles, by which they run, would be preternaturally pressed, and become so many angles or centres of that vellication, to the great detriment of their several functions: 3. If a vellication was produced in the *intercostal nerve* or *par vagum*, which communicate with several of the principal nerves in the body, would there not be great confusion brought about, in all the parts to which such communicating nerves lead; and would not their proper actions be much impeded? From these and such-like reasonings, we must conclude the nervous fibres to be tubes, capable of transmitting something to the parts into which they are inserted, from the brain and spinal marrow.

These nervous tubes contain a fluid * whose nature and property is not so much to nourish as to inflate; and consequently whenever the nerves are impressed by the soul, there is an immediate *inflation* of all the cells, in the fibres of that muscle which is to perform a voluntary motion.

And

every individual solid part of the body (which I am inclined to believe, rather than confine her to any one particular place), sure she may instantly be apprised of every *contact* producing the different sensations, that can happen, wheresoever there is a *nervous expansion*; without our being under a necessity (by placing her only in the brain) of bringing to her assistance an *undulation* of animal spirits to and from the brain; a vibration of the nerves; or, with some authors, contrary motions of those animal spirits in the same tube, serving to motion and sensation; all which are productive of many absurdities, which we have not room here to enumerate.

* This opinion leads me to the following queries:

1. Is it not well known, that confined air is capable, by a proper impulse, of performing more violent and swift motions, than any *succus* or *liquor* than can be conceived?
2. Are not animal spirits a *Quid occultum*?

3. Is

And these muscular cells, together with the nervous tube that opens into each muscular fibre, are constantly full of this *inflating matter* to a certain degree; that is, filled to a *medium* between their utmost compression or emptiness, and their utmost capacity of repletion. By this means, the muscular fibres, when at rest, are in a medium between distension and contraction; but are distensible to near $\frac{1}{2}$ longer, and contractible to near $\frac{1}{3}$ shorter, than when in a state of rest; the former, by the *retraction* of part of the *inflating matter* back into the nerves; and the latter, by its *impulsion* or inflation into the *cells* from the nerves.

There is no necessity for imagining, with some authors, that the soul makes her impulse in the head rather than any other part, in order to impel from thence the inflating matter of the nerves to this or that muscle: because, by supposing the nerves always thus full, the smallest impulse on the part of a nerve leading to this or that muscle will be sufficient to perform what is necessary; whereas, if we confine the power of the will to the brain, may there not be danger of irregularities like explosions, from thence into the different divisions of a nerve, and so of causing confused motions in several parts at a time, like an epilepsy? Or if an impulse be made on the trunk of a nerve, the divisions ought all to be subject to the effect of that impulse. It will therefore be better for our purpose to think the *mind* makes her impulse where she pleases, and chuses that part of a nerve only that leads to the particular muscle to be moved.

3. Is not the blood the sole mass from which every liquor of the body is secreted? If so, may we not as rationally suppose, that any exalted or refined liquors, for whatsoever purposes designed, may be sent off through exquisitely minute canals, to answer their several ends, immediately from the mass, as to *imagine* such are only secreted in the brain? For, by the assistance of the solar microscope, such exquisitely minute canals, as could not be discerned even by the double microscope in a full view of the circulation in the mesentery of a frog, appeared to be filled with a clear liquor passing rapidly from the vessels wherein I saw the blood circulate, and which seemed to me some hundreds of times smaller than those vessels from which they sprung.

4. Is not the blood full of air, and is it not daily supplied by inspiration in the lungs? If so, why may we not as well suppose, that the blood carries to the brain, and there deposits, a sufficient quantity of an *aura* to supply the nerves and muscular cells, in order to their motion, as we shall explain it by-and-by, as that animal spirits are secreted there? any liquor, as we have said before, being less likely to serve to swift motions than air.

Now till the negative to these queries can be proved, I cannot help thinking, that nothing but this *inflating aura* can enter into the cavities of the muscular *cells*; and consequently must consider the brain as destined only for treasuring up this *inflating aura*, and the nervous fibres as pipes to convey it to the muscular fibres, into whose cells they alone can open; that no nutrition can proceed any other way than immediately from the blood-vessels to the parts that require it; and that therefore it may, with some probability, be concluded, that the *encephalon* and *spinal marrow*, *nerves*, and *muscular fibres*, are a set of mechanical organs, exempt from any other office, than to cause motion and sensation by *inflation* only, so that every muscle that moves may justly be said to be blown up into motion.

This seems to be corroborated greatly by the motions we see in insects after being cut to pieces, as worms, flies, &c. and in more perfect animals, as poultry, frogs, &c. which move, and shew signs of pain, a considerable time after their heads are cut off; which could not happen, if the impulse was only made in the brain. We can carry this argument yet further, even to human nature, if what *Diemerbroeck* * relates be true: he says he saw a man executed at *Leyden*, who, after his head was struck off, rose upon his feet, and stood for a little time. And Dr *Stuart*'s experiment upon the frog (which is, after the head is cut off, and the limbs hanging loose, to compress the spinal marrow with the end of a probe made flat, whereby the limbs are immediately contracted, and with some violence) shews, that if the impulse was made in the brain, the motion would be confused and general; since all the lower parts were moved by the single impulse upon the spinal marrow of the frog.

Another reason for supposing the soul capable of chusing any part upon which her impulse may be made, is, that if it was made in the brain alone, there might be some impediment or interruption to her intention, produced in the *ganglions* of the nerves; which, some authors think, do the office of so many brains, but which, more probably, serve as so many *fulcra* or *stays* to keep the nerves firm, and to favour their further directions to the different parts of the body.

The air-pump shews us, that, in all flexible bodies, such as flesh, *Of interstitial* small animals, and the like, there is a sufficient quantity of air, lodged *air.* in the interstices between the muscular fibres and the blood-vessels, &c. to resist the pressure of the ambient air; which pressure being taken off in the air-pump, this interstitial air will expand itself, and swell the flesh, animal, &c. to an incredible size. This may be considered as a general *æquilibrium* kept up between the ambient air, and that in an animal body, for the safety and preservation of the animal: and these air globules, which we shall throughout this essay call *interstitial air*, are constantly in a middle state between their utmost *contraction* or *condensation*, and their utmost elastic *expansion*, except changed into either state by some adventitious cause.

The blood appears, by the microscope, to flow from arteries into *Of the circu-* veins immediately †, which is easily seen in the webs of frogs, tails of *lation of the* fishes, *mytuli*, and the like; and, I doubt not, would appear so in mus- *blood in the* cles, if they were thin enough to become transparent for viewing with *minute vessels.* glasses. Therefore the arteries and veins may be considered as continued tubes, terminating in nothing; but as the arteries arise immediately from the heart, so they run to the extremities of the lungs and body, ramifying and decreasing in diameter, till they become invisible to the naked eye, and gradually become veins, which unite into trunks, in-

* *Anat. corp. hum. lib. viii. cap. i. de nerv.*

† Without the intervention of any vesicles, such as *Mayow* suggested. See Fig. 127.

creasing in diameter till they arrive at, and open into, the heart again. So that nutrition and the secretions are carried on by minute twigs, from these continued *capillary canals* * sent off to the glands, and to the parts to be nourished.

And it further appears, by microscopical observations, that these capillary canals run parallel to, and by the sides of, the muscular fibres in general, or to fibres of whatsoever nature, where these capillary canals exist. This seems a very wise contrivance in every degree; for if their longitudinal direction was cross the other fibres, the circulation could not be so smoothly nor securely carried on; and therefore would be liable to great impediments, from a transverse pressure of the muscular fibres upon them; whereas, in this parallel direction, they are secured from any impediment, but what proceeds from the pressure of the sides of the muscular cells upon their sides, in the performance of muscular motion: we are here to take notice, that the muscular fibres receive twigs from the arteries to nourish them, and from the veins to carry back the *residuum* into the parallel canals, as we have hinted before.

Hence we must conclude, that no *vessel*, of any kind whatsoever, opens into the cavities of the *cells* of the muscular fibres, but nerves.

Of the æquilibration and motion of muscles.

From what we have premised, let us endeavour to shew how muscular motion is performed, and how the several principles just laid down are applicable to it.

The force of the *inflating matter* contained in the *cells*, is only equal to the force of the *interstitial air*, while the muscle is at rest. This may be called the peculiar *æquilibration* of a muscle in itself; but, by the impulse of the will on the nerve, an additional *inflation* is made to the *cells*, and then their force becomes superior to the *interstitial air* globules; and again becomes equal when that impulse ceases, and the muscle is at rest. But if, upon the cessation of that impulse in one muscle, there is an impulse made on the antagonist at the same time, in order to move it, then the force of this *interstitial air* in the ceasing muscle, exceeds that of the *inflating matter* in the *cells*, compressing their sides, whereby they are lengthened beyond their tone of rest; as we shall more fully explain it, when we speak of the æquilibration between antagonistic muscles.

From this *inflation*, which is performed as quick as thought, the *cells* instantly increase in diameter, and grow shorter, compress the venal canals, and obstruct them. Hence the blood stops, and the arteries, by propulsion from the heart, increase in their diameters, while the veins are squeezed quite empty, and the whole muscle is shortened, and its action performed.

The degree of contraction in a muscle is always as the force necessary to perform such or such an action; and this is determined by the degree of the *inflation* of the *cells*; that is, if the force required be but inconsiderable,

* Where the arteries degenerate into veins.

considerable, then the impulse of the mind will be but inconsiderable, and consequently the *cells* will have but an inconsiderable *inflation*; therefore the pressure of the *cells* on the minute veins will be in proportion: so that of the arterial blood, tho' it flows always in the same quantity into the muscle; yet, because the pressure of the *cells* on the veins is but small, a proportional part will go on, and return by the veins; and therefore the repletion of the muscle, and it's contraction, can be but inconsiderable: whereas, when a violent motion is to be performed, then the degree of *inflation* of the *cells* will be very great, the blood totally stopped in the veins, the arteries increased in diameter, and the muscle shortened even to it's ultimate state of contraction, if requisite.

While the circulation is thus hindered in the acting muscle, there can be no disorder occasioned, either in the oeconomy of the circulation in other parts of the body, nor in the particular constitution of the muscle itself. The former case is certain from our daily experience of amputations; wherein, tho' a limb be cut off, and the space in it immediately wanting, and although the same quantity of blood, supplied by digestion every day, is carried into the vessels, yet their distensibility is such, that the only change they can suffer is further repletion. And as to the muscle itself, if, during a violent action, there should happen any danger of injury, it would be perceived immediately by the mind, and remedied by ceasing her impulse on the nerve and *cells*, and thereby granting a free circulation instantly.

As to the æquilibration between two antagonistic muscles, Dr *Stuart*, in his Lectures, thinks, that “As each antagonist has it's distinct nerve
“ or nerves without communication, and the antagonist muscles communicate one with another by one common trunk of an artery, and
“ one common trunk of a vein; they are like two antagonist scales in
“ *æquilibrio*, over which the mind has a distinct power, by distinct nerves,
“ for determining the animal spirits, and thereby the blood, to either
“ side at pleasure, without affecting the other: and concludes from
“ thence, that if what is taken from one be added to the other, the
“ *momentum* of the motion will be doubled.”

Now there does not seem to be occasion for taking away any blood from one muscle to be determined to the other; nor has the Doctor explained how it may be brought about by the mind; nor, indeed, can I conceive it possible to hinder the motion of the blood in the arterial trunk, leading to one muscle, while it is sent to the other; except we could suppose the mind capable of making a ligature, or other stricture, on the ramification leading to it, leaving the other open at the same time. But no nerve can have such a power from the mind to act upon the main trunk of an artery; and therefore the *effect* of the mind's impulse upon the nerve can only be produced, as I have said, on the most minute venal canals; where they are capable of being pressed by the bellies or *cells* of the muscular fibres that lie by their sides, and where alone the mechanic structure of the parts admits of it: besides, the

arterial pulsation must of necessity go on, to carry blood to every part of the body, being propelled by the same constant force always, in healthy bodies. Hence the convenience of this our system of muscular motion is apparent, since it is carried on at the same time that the heart and arteries do their offices without interruption to either.

Again, if it was absolutely necessary to muscular motion, that blood should be taken from an antagonist, in order to be sent to the acting muscle to break the *æquilibrium*, some muscles, that serve to involuntary motion, could not have any motion at all, having no antagonist. Therefore, in general, motion must be carried on more simply, and in the manner explained above.

Yet an *æquilibrium* between a muscle and it's antagonist is absolutely necessary ; but it is only so in order to preserve the equipoize between them, while both are in a state of rest. Now there are two sorts of *æquilibriums* proper to muscles, the one which is peculiar to each muscle in itself, explained before, and the other that which is between two antagonists. Let us see how the latter is broken, in order to motion.

We are to consider two antagonists exactly equilibrated, before any action is begun in either. The mind, now resolving to act with one muscle, makes an impulse upon the nerve leading to it. The *cells* in that muscle are instantly inflated beyond their tone of rest, to the degree necessary for the particular action intended : the blood is stopped, as mentioned before, in the little *venal parallel canals* ; and the muscle is contracted beyond it's tone of rest. And at the same instant that the mind impels the *inflating matter* into the *cells* of the muscle in motion, she remits to determine any to the antagonist, which causes a *retraction* of it into the nerves : by this means the interstitial air, in this, exerts it's elastic force, compresses the sides of the *cells*, whereby they are reduced to oblong forms, and the whole muscle is drawn beyond it's tone of rest.

It is thus alone the *æquilibrium* is altered between two antagonists in an instant, without a necessity of taking one drop of blood from the one to add to the other : and it is even so far from it, that, during all this action, the quantity of blood in both muscles, while one is contracted, and the other dilated, is nearly equal ; for altho' the blood is stopped in the acting muscle, by the mechanism above explained, and the veins are all compressed, so as to contain none during a violent contraction ; yet the arteries may be said to contain a double quantity, propelled into them by the constant pulse, as they are elastic, while the circulation is carried on pretty equally in the antagonist ; and consequently both arteries and veins in this can contain no more than the very arteries in that : therefore a muscle is neither larger nor less, in general, by contraction or extension, than when in a state of rest, the proportion being constantly kept up ; that is, when shorter, a little thicker ; when longer, a little more slender.

Our explanation of the *æquilibrium* between the interstitial air, and the muscular *cells*, will enable us to attempt accounting for the swiftest *gentle* muscular motions that can be performed; such as are necessary in an *allegro* part on an instrument of music. Thus: the mind intends moving a finger to perform a swift shake; the first motion is a pressure of the finger upon the string, by the contraction of the *flexor* of that finger: the successive motion then will be only a remission of that pressure, without any necessity for a re-action in the antagonist muscle, because the motion required is but inconsiderable: and this pressure and remission, by being nimbly repeated, will become a kind of alternate *tremor* of the finger upon the string; because, after the first inflation of the muscular cells, which causes the contraction of the finger, the instant remission of the inflation becomes a kind of retraction of it, as I have said before: then the interstitial air follows it with an elastic expansion beyond it's tone of rest, and compresses the cells; then a new inflation of the cells succeeds; and, in fine, an action and re-action between the interstitial air and cells is carried on as long as the shake is required to continue: this amounts to an alternate *systole* and *diastole* between them, like two springs acting by alternate repulsion.

The performance of the common actions of life depending upon the *Of involuntary* voluntary motion of the muscles alone, authors have almost neglected *tary motion.* to give any explanation of involuntary motion; and therefore have chiefly attended to the former: whereas the latter as well deserves the attention of the learned, and depends as much upon the same mechanical principles and structure, differing only in this, that voluntary motion is urged by the will, and the other is carried on without her impulse.

It appears, by what we have already said, that the disposition of muscular fibres to contract depends on an additional inflation of their cells, but that this cannot be done without an impelling power upon the nerve to cause it. Now, in the performance of voluntary motion, the will must be supposed to make this impulse; but we must endeavour to find some other impelling cause of motion in the muscles of involuntary motion to set them to work: for the mechanical structure of all muscles is alike.

We will produce two examples; the *heart*, and *intestinal canal*.

By a very natural analogy between vegetables and animals, I am apt to believe, the parts of the animal are perfectly formed in the *ovum*, before it's egress from the *ovarium*, only wanting *explication* and *expansion*; but that these two effects cannot be produced, till after it has changed it's place, and is deposited in such a receptacle as may favour the propagation of these effects. Just so it is by the seeds of vegetables. This being the case, in order to come at our purpose concerning involuntary motion, we must now consider the *fœtus* in the *uterus*, tho' very small, as having all it's parts complete, but motionless, or in a state of rest.

Now

Now the heart is the first part that can have any motion at all ; and being in a state of rest, must be supposed in a *medium* between it's dilatation and contraction. The first motion in the heart then will be a compulsory motion, which is it's *diastole* ; and the first motion performed by itself will be it's restitutive motion or *systole*. Till this is done, there probably can be no accretion of parts, because nothing can be propelled to the extremities but by the contraction of the heart. And this is much earlier brought about than many authors have imagined, and in the manner following.

It is certain, that fluids pass from the mother to the child very early, by the veins of the *funis umbilicalis*, and at length arrive at the *vena cava*. The auricles and ventricles are now in a state of rest ; but the fluids falling into the right auricle, with a gravity and force it was not liable to before, it is filled and dilated beyond it's former tone ; whereby the muscular cells are laterally compressed, and become longer by the diametrical pressure, and the interstitial air is also compressed into narrower limits than before, and the whole auricle is upon the stretch : thus it's *diastole* is compulsory.

Now this sudden repletion, at the same time that it dilates the auricle, makes such an impulse upon it's nervous system, that an inflation succeeds in the cells, whereby they increase in diameter (the interstitial air now concurring by it's *nifus* to restore itself) ; and these two powers together, which, in performing voluntary motions, are antagonists to each other, are now forced to become joint antagonists to the dilating force of the intruding fluid from the *vena cava*, whereby the auricle is contracted, and the blood driven into the neighbouring ventricle. Thus is the motion propagated from auricle to ventricle, and from the ventricles to the arteries thro' the whole body, which is carried on during life.

And as to the intestines, as there can be no peristaltic motion, till deglutition is first performed, it is carried on by the same mechanism : for, when the first food is swallowed, being driven into the *oesophagus*, it forces it open, and dilates the circular muscular fibres beyond their tone of rest, compressing both the interstitial air, and the cells of the fibres ; which, restoring themselves gradually, again contract themselves successively downwards, till the food is conveyed to the stomach, and thence to the intestines : and this is also carried on till death, when once begun.

Of depraved
motions.

Palsies, cramps, and epilepsies, may be accounted for by this system. May not the muscular cells become rigid and hard, and not be capable of being dilated to the degree necessary to carry on the action and reaction between them and the interstitial air, in an equal manner ; and therefore produce such irregular attempts to motion, as would amount to a paralytic *tremor* ? Frequent drinking spirituous liquors will render the cells of the muscular fibres rigid, and cause the same effects ; and in fevers, with what is called a *subfultus tendinum*, a common symptom arises,

arises, which is an imperfect attempt to pull the bed-cloaths, attended with a *tremor*, and proceeds from the same cause; for the violent heat of the *fever* has rendered all the fibres and their cells rigid, and, therefore, incapable of due inflation; for that motion is not made in the tendon which we feel, but is produced by the irregular twitchings of the muscular fibres of that tendon.

Or may not these cells be too lax and weak, and so lose much of their elasticity, and yield to the entrance of too great a quantity of the *inflating matter* of the nerves, whereby the interstitial air may be constantly compressed, and the peculiar *æquilibrium* depraved, so as to produce spasms, if partial; if total, an epilepsy? Painful cramps are occasioned by the inflation of some of the muscular fibres, while others are not affected in the same muscle; and an unequal *drag* or contraction is painful, when a total one is more tolerable.

And as to the decay of a limb, it does not seem to me to happen from the want of a nervous juice, by way of nourishment, as has been the common opinion; but because, whether they be muscles of voluntary or involuntary motion, the *inflating matter* not being able to reach the *cells*, from an obstruction in the roots or any other parts of the nerves, so as to fill them up to an *æquilibrium* with the *interstitial air*, this air will expand itself beyond its *medium*, and compress the little twigs of the *parallel arterial canals*, that go off to nourish the *cells* of the muscular fibres, and lie constantly upon them, and so stop their nutrition from the blood; while the *parallel canals* themselves, being too considerable to be affected by the pressure of the interstitial air, continue on their stream: thus a decay of the limb, and loss of motion, is effected, while there is a circulation continued thro' the body of every muscle in the limb.

The hearts of certain animals, taken out of the body, will continue to move a considerable time, because the action and re-action cannot cease at once between the *muscular cells* and the interstitial air, no more than a pendulous body can cease at once, after having been swung beyond its point of rest. The reason is, that as long as the heart remains warm, the elastic force of the interstitial air, and that of the inflated *cells*, will have the same alternate effect upon each other, till the external cold, and want of the gradual supply of the inflating matter, cause the action and re-action to become unequal, and so by degrees fix both: but, when the motion ceases, it may be again excited for a little time by pricking. This seems to make a new impulse on the matter yet remaining in the muscular cells, which, acting by their expansion, will compress the interstitial air, and receive a repulse from it for a few times faintly: but this experiment will not answer, when the heart is quite cold; but then, if warm water be poured on it, there will be motion again excited. This happens, because the interstitial air, before condensed by the cold, is now instantly rarefied and expanded, and therefore presses the *muscular cells*; which also being warmed, the remaining
inflating

inflating matter will exert itself in it's turn, and so continue an alternate motion for a few times, till the heart grows cold again. But this experiment will not often answer; because, altho' the same interstitial air remains, it is a doubt whether the matter of the *cells* does not exhaust by degrees; in which case there can be no more re-action. I have two or three times observed, when the skin of an ox has been taken off, the muscular fibres continued to move in spasms, and ceased at intervals; then re-assumed their motion, going on thus till the whole surface grew cold, and the inflating matter of the cells, for want of a supply, was quite overcome by the pressure of it's antagonist, the interstitial air.

LECT. III.

Read in October, 1745.

As I have already produced every thing I could say upon the subject, touching the *cause* and *manner* of a muscle's being moved, I must confine this Lecture to the description of certain particular organs, which, tho' not *commonly* thought so, I shall endeavour to prove to be muscles: and these are, 1. The *uterus* itself; 2. The *fallopian tubes*; and, 3. The parts commonly call'd *ligamenta rotunda*.

Of the uterus.

Whoever considers the offices and use of the *uterus*, will hardly hesitate to pronounce it a *muscle* with me. Besides which, the very structure of it's substance, from many observations, prove it sufficiently, having discerned it's interlaced muscular fibres, as plainly as those of any other muscle in the body: however, it's definition and use, as we shall now mention them, will serve further to illustrate and confirm our assertion.

The *uterus* is an organ destined, by the wise AUTHOR of nature, not only to receive, cherish, and increase the *ovum*, which contains the *fœtus*, till it arrives at due maturity, but also to promote it's *expulsion*, when the time of gestation is fulfilled.

In a *virgin* or *empty* state, it is very small, as it appears at *Fig. 130.* and being laid open, it's substance and cavity are no more considerable than *Fig. 131.* represents; yet, in proportion to it's size, it's *parietes* are thicker and more compact, than at any time during it's gestation, whatsoever may vulgarly be thought of it by those who have only read of, and not observed it. This substance, then, consists of fleshy fibres and blood-vessels, both arteries and veins; and is cloathed internally by a membrane, whose whole surface is set thick with *valvulous holes*, and externally by the *peritonæum*: These, no doubt, serve to strengthen it's substance, and hinder it's muscular fibres from being driven too far asunder, by the distension or enlargement of the blood-vessels (which are very great and numerous thro' it's whole substance), while it is distending gradually, by the increase of the lymphatic fluid *, and growth of the *fœtus*, *placenta*, &c. in it.

* *Liquor Amnii.* The manner of the secretion of this fluid, which is much controverted, shall be soon accounted for in a treatise on the *Nutrition* of the *Fœtus*, which shall be published in *Some Essays on the Diseases of Women*.

In this virgin or empty state, the muscular part is so squeezed and compressed together, that the arteries and veins are quite closed up, and utterly incapable of receiving a drop of blood during that state; except some few small vessels, which only serve for the continuation of nutrition: but as soon as the *ovum* is lodged in the *uterus*, and begins to increase in bulk by the intrusion of the *liquor amnii*, it forces the *uterus* to distend by degrees, till it arrives at the bulk we see in the latter months of pregnancy. By this gradual distension, the muscular fibres are forced further asunder, and consequently the blood-vessels are gradually freed from the pressure, till at length they increase to their full dimensions.

All this time the muscular part, being compelled to dilate, can perform no function proper to a muscle, until the *cervix uteri* is so shortened, as to become thinner than the rest of the *uterus*; which does not happen till about the completion of the ninth month (a little sooner, or a little later). Now a word or two of the *cervix* here, since we are obliged to mention it, will render our explanation more clear, and will also shew the infinite wisdom of the Great CREATOR in the formation of this organ, for the purposes it is designed for.

The *cervix* of the *uterus* is a compact part, narrower than the body, and about an inch long (more or less), as at *Fig. 131*. It has two orifices proper to it while in this state; the one called the *internal*, which opens into the cavity of the *uterus*; and the external orifice, commonly called the *os tinæ*, which opens into the *vagina*. It's substance is very hard and solid, and it has a very small passage from the one orifice to the other, having on it's surface many glands, which secrete a dense glutinous substance, and many *striæ*, to which this *gluten* adheres; so careful is nature to keep the *uterus* close, in order to prevent the ingress of even the air.

Now this *cervix* is made thus long and compact, that it's resistance to the intruding waters of the *amnium* * should be greater than that of the *parietes* of the *uterus*, while it is dilating: this gives time enough and room for the *fœtus* to grow to perfection; and near the time in which this is brought about, then this, which was before a *cervix* with an orifice at each end, is now become so much shortened, as to acquire a greater thinness than the *parietes* of the *uterus*, and consequently to bring both orifices into one.

By this time the *fœtus* being come to perfection, and the resistance being less at this orifice than at the other parts: the *uterus* now begins to act in it's muscular capacity, beginning by degrees to contract itself alternately, till the waters and *fœtus* are determined downwards to the orifice; which, the more it dilates, the more power the *uterus* still acquires, by it's natural disposition to contract; until the muscles of the *abdomen* are at length drawn into consent, and the *conatus*, or pains, as

* Ἀμνίον. ἢ τὸ. The name of one of the membranes that contain the child, &c.

women call them, which begin by the involuntary motion of the *uterus*, are assisted by the voluntary contraction of the abdominal and other muscles towards the extrusion of what it contains *.

Nor does the *uterus* rest, after it is rid of every thing, but still continues alternately to contract; until, in some days sooner or later, it has squeezed out the fluids from the blood-vessels, called *Lochia*, compressed them close, and at length acquired it's former size and compactness, or very near it. This is what causes those pains, which are commonly called after-pains.

Thus is this wonderful *phænomenon* brought about by the muscular structure and mechanism of the *uterus*; and it may securely be affirmed, that if it was not a very muscle, this effect could not come to pass, no more than the blood could be driven from the ventricles of the *heart*, if it were not a *muscle*, or the urine from the bladder (without a muscular coat, and a *detrusor* muscle), which are forced to distend by the intrusion of urine from the ureters, till, by it's repletion, the resistance becomes less at the orifice than in the *detrusor*. This is more fully explained in my description of the bladder; which see.

Of the Fallo-
pian tubes.

These organs were known to, and well described by, *Hierophilus* and *Ruffus Ephesus*, as the late learned Dr *Douglas* has very justly observed †; altho' they are called by *Fallopian's* name by some not well versed in the History of Anatomy. They are soft pliable bodies, and are properly enough called tubes or trumpets, because they arise small on each side from the angles of the *uterus*, and run larger by degrees, till they approach the extremity, and growing again a little more narrow, terminate in ragged *fimbriæ*.

The cavity of each is also so small at their origin in the angles of the womb, that a small hog's bristle can hardly be introduced; but it enlarges gradually, till, at the other extremity, it is capable of admitting the end of a goose-quill.

These tubes are in a strait direction for a little way from the *uterus*; but as they enlarge, they grow into vermicular curves or flexures, terminating downwards, and a little backward, with their mouths opening upon the *ovaria*, altho' not at all attached to them; and are sustained and strengthened by their situation in the duplicatures of the *peritonæum* on each side, which serves to keep them in their curved state (as the mesentery sustains the guts) during the unimpregnated state of the *uterus*: and these duplicatures (which also strengthen, and in some measure clothe, the *ligamenta rotunda*), are what are commonly called the *Alæ Vespertilionis*.

These tubes are made up of muscular fibres, which are partly longitudinal, and partly are situated obliquely, and somewhat circular; from which structure they have a motion which may be called a compound

* This regards the natural delivery of a woman in due time. As to what relates to abortions at different times, it is a subject reserved for another place.

† *Bibliographiæ Anatom. Specimen*, p. 126.

motion, and which amounts to what we count the vermicular motion in the guts ; but this motion is not begun, till there is a necessity for it ; which is as soon as an *ovum* is impregnated.

At the instant that this impregnation happens, the orifice of the *fallopian* tube clasps itself close upon the part of the *ovarium* next to it, by contracting ; and receives into it the *ovum* ; which, as soon as it has entered, those oblique circular fibres, or, as we may term the extremity, the *sphincter* of the tube, incloses and pushes it farther in towards the *uterus*, which is by degrees forwarded by the peristaltic motion of the tube, till at length it is pushed into it's receptacle the *uterus*, in the manner explained in my last Lecture, where I endeavoured to account for involuntary motion : and when the *ovum* is thus settled, then the cause of this motion ceasing, the tube is at rest ; and in proportion as the *uterus* grows more tumid afterwards, both tubes lose their vermicular flexures, and at last grow quite strait and pendulous, as at *Fig. 132. bb*, by the general distension of the *peritonæum* : but when the *uterus* is emptied, and again is contracted to it's former dimensions, then the duplicature of the *peritonæum* is contracted in proportion, and these tubes are consequently restored to their former vermicular flexures, and therefore rendered capable of receiving an impregnated *ovum*, as before.

From hence it is easy to conclude, that what has been thought, by many old authors, and indeed by some moderns, concerning superfetation (any considerable time after the *uterus* has received one (or more) impregnated *ovum*) is altogether groundless : for, when the *tubes* are grown flaccid, after having conveyed the *ovum* to it's receptacle, and lost their vermicular flexures, they can no more embrace the *ovaria* during that pregnancy ; and also because the *ovarium* on each side is driven by the distension of the *uterus* to a greater distance (out of it's former situation) from the *vagina*, and consequently out of the reach of impregnation.

Therefore, whensoever it happens that two *fætus's* are brought forth at different times, they both come within the common stated time of gestation ; that is within nine months, or thereabout, from the *coit* that produced the fecundation : in which case, tho' one may be perfect, and come at the full time, the other is imperfect, and sometimes wasted, and comes before the due time, being both begotten at the same instant, or within a very little time of one another. But because one shall have more nutrition determined to it, the other less, the latter, which is always situated nearest the orifice, will of necessity suffer abortion. Thus, different emissions of children happen, not because they are begotten at different times, but because, nutrition being unequally distributed, the defrauded *fætus* is extruded by the other, who often keeps his place till he arrives at due perfection.

Before we quit this part of our subject, it will not be unseasonable to mention a case here, which was in some measure, objected to what I

had laid down against a possibility of superfetation, when I read this Lecture.

A Gentlewoman in *Charles-Town* in *South Carolina*, about 1714, was brought to bed of twins, one immediately after the other, in the same labour. The one proved to be a *negro* child, and the other a white one; which very much surprized those that attended about her. So flagrant a testimony of her infidelity to her husband, made her confess before them, that a *negro* servant, immediately after her husband had left her one morning, came to her; but she pleaded, as an excuse, that he threatened to kill her, if she did not comply with his desire; and that accordingly she was forced to admit him into bed.

Now, in order to remove this objection, it must be observed, that what I have asserted is, that when the *fallopian* tubes are grown flaccid, and have lost their vermicular flexures, and the *ovaria* are driven to a greater distance from the *vagina* by the enlargement of the *uterus*, that then they are out of the reach of impregnation.

Whereas, in the case before us, there could be no such change brought about in the *uterus*; for, altho' one tube had received an impregnated *ovum* by the *coit* of the husband, and conveyed it to the *uterus*, whereby it may have lost it's flexures, yet the other tube had not; nor could the *uterus* be enlarged by the one in so short a time; for it appears the *negro* cohabited with her immediately after her husband; so that the *ovum* impregnated by him was from the other *ovarium* thro' it's neighbouring *tube*; which might have happened in a second *coit* with her husband, as well as with the *negro*.

From this structure and office in these *tubes*, we may venture to be of opinion, that they are truly *muscles*.

Of the ligaments
menta rotunda.

There have been various conjectures concerning these slender bodies amongst authors; but the greater part accounted them as ligaments. *Spigelius*, and after him *Diemerbroeck*, had a notion, that they were *vasa deferentia*, which they thought carried seminal matter from the *ovarium* to the *clitoris* in females: but this cannot be the case; first, because they have no cavity, that I could find, and are therefore impervious to any matter: again, their situation would not admit of it, supposing they were tubular, because they arise from the angles of the *uterus* a little below, and forward of the ligaments that suspend the *ovaria*, as *Fig. 129. f*; and, passing along thro' the duplicature of the *peritonæum* on each side, rise over the edges of the *os pubis* nearly in a line above the fore-parts of the *acetabula*; and then running thro' the rings in the *peritonæum*, and tendons of the oblique muscles, &c. turn downwards and forwards, towards each other, on the *ossa pubis*, till they are lost in the fat of the *mons veneris*, and sometimes in the groins; and consequently could not serve to any such use, having no communication with either *ovarium* or *clitoris*.

The celebrated *Winslow* thinks them vascular cords, arising from the communication of the spermatic and hypogastric vessels; and says, they ought

Ought to be esteemed as a particular continuation of the spermatics ; and that he suspects they furnish the matter which is secreted from the *lacunæ*. Now these cords are made up of muscular fibres alone ; for, by the most strict examination I could make, the fibres appear to me fleshy, and have a longitudinal direction from one end to the other ; and as to their being vascular, it is impossible, from their situation, they should be so, because they have not the least communication with these blood-vessels he mentions, and have only minute twigs, as every other muscle has, for their nourishment : and as to the *lacunæ*, which are glands on the surface of the *vagina* in several parts, they are remote enough from any communication with the fibres of these muscular cords, where they are lost in the fat of the *groin*, and *mons veneris* ; and therefore must be designed for some other use.

From what I have said of these cords, I must concur in the opinion *Vesalius* had of them, concerning their being *muscles*, who is quoted by *Santorini* on that account ; and these, with *Dr Nichols*, are the only authors that I know of, that have mentioned them as muscles ; yet none, I believe, have ascribed the same use to them that I think they are employed in ; and that only respects the distended *uterus*, as follows :

While the *uterus* is in it's contracted state, these *muscular cords* are also in a state of rest, forming the curved direction we have mentioned, and as the *uterus* grows larger by it's pregnancy, they are pulled upward and outward over the *ossa ilia*, being more and more distended, in proportion with the *uterus*, till they form nearly right lines from their origin to their insertion, and are in many women in the last month 15 or 16 inches long, more or less. Whereas, when they are in their state of rest, they seldom exceed from 7 to 9 inches. From hence it may be rationally concluded, that when delivery is over, they assist the *uterus*, by their equally contracting on each side, the more regularly to contract itself, because many evils might be produced by an unequal contraction of it ; but, as they are part of it's diseases, they can have no place in this Lecture. And as the muscular cords can in no wise be said to suspend the *uterus*, we cannot consent at all to call them *ligaments* ; nor can we conceive any use of them while the *uterus* is unimpregnated at any time. Therefore we must think they are *muscles*, reserved for the purpose just mentioned ; and neither *vasa deferentia*, blood-vessels of any kind, nor ligaments.

Fig. 125, 126, 127, 128. shew the forms of several muscular fibres, considerably magnified by the double microscope, where they appear to differ in size, as well as in the number and distance of their cells, as it is mentioned in their description before.

Explanation
of the figures.
Fig. 125, 126,
127, 128.

Fig. 126. is a view of several muscular fibres, with those minute blood-vessels which I have called the *parallel* or *capillary canals*, lying in their parallel directions, in order to answer the ends of nature.

Fig.

- Fig. 127. *Fig. 127.* is a representation of the circulation of the blood in a little piece of the *mytilus*, not exceeding $\frac{1}{4}$ of an inch every way in it's natural dimensions; wherein it appears, that those minute vessels are in a direction parallel to other fibres of whatsoever nature.
- Fig. 128. *Fig. 128.* shews part of a *capillary canal*, with part of a *muscular fibre* on each side; to demonstrate how the minute twigs are sent off from the canal to the muscular fibres to nourish them, as is hinted in the last section above-mentioned. This is viewed by the greatest magnifier of the double reflecting microscope.
- Fig. 129. *Fig. 129.* is a view of the *pelvis*, with the internal feminine parts of generation *in situ*. *a*, the body of the *uterus*. *b*, the *rectum*, turned over the upper *vertebra* of the *os sacrum*. *c*, the bladder. *d*, the *fallopian tube*. *e*, the *ovarium*. *f*, the slender *muscular cord*, commonly called *ligamentum rotundum*, rising from the angle of the *uterus* on each side, and ending by several *fimbriæ* at *g* in the fat of the *mons veneris*. *h*, the *meatus urinarius*. *i*, the orifice of the *vagina* deprived of the integuments.
- Fig. 130. *Fig. 130.* is a view of the shape of an unimpregnated *uterus*, all the appendices being cut off. *a*, the body of the *uterus*. *b*, the *cervix*. *c*, part of the *vagina*.
- Fig. 131. *Fig. 131.* shews $\frac{1}{2}$ of the same *uterus*, the other being cut off laterally and longitudinally, whereby it's cavity (*a*), the inner surface of it's *cervix* (*b*), one lip of it's *os tincæ* (*c*), and part of the inner surface of the *vagina* (*d*), come into view. *e*, the small passage from the angle of the *uterus* into the *fallopian tubes*.
- Fig. 132. *Fig. 132.* shews a view of a pregnant *uterus* of 7 months. *a*, the body of the *uterus*. *b b*, the *fallopian tubes* relaxed and pendulous. *c c*, the *ovaria* also pendulous. *d d*, the *muscular cord*, called *ligamentum rotundum*, upon the stretch. *e*, the bladder. *f f*, the *ossa innominata*.

Human Phy-
siognomy ex-
plained, in
the Crounian
Lectures on
Muscular Mo-
tion; by the
same. Supp.
to the year
1746. Read
Nov. 20.
1746.

LECTURE I.

XI. I shall now attempt to give you a description of the muscles of the face; with some observations and remarks, which I hope will appear curious to you, relating to their separate as well as conjunct actions, and the appearances of the countenance that are the natural effects of such actions; together with accurate drawings, which will serve to their better demonstration. Let us consider those muscles in their order, which chiefly serve to form and move the skin of the face, or change the countenance; which are those of the *forehead*, *eyelids*, *eyes*, *nose*, *lips*, and *cheeks*.

To these I shall confine this Lecture, and to certain remarks and observations upon their actions; having regard all along to those authors, that were most exact and accurate in their researches and discoveries of the true structure of muscles. But, before I begin my description, I cannot but particularly mention, in this place, with the greatest reverence, that indefatigable Anatomist the late Dr *James Douglas*, whose treatise of the muscles, I find, was not known to the famous anatomical critic

Fig. 125.

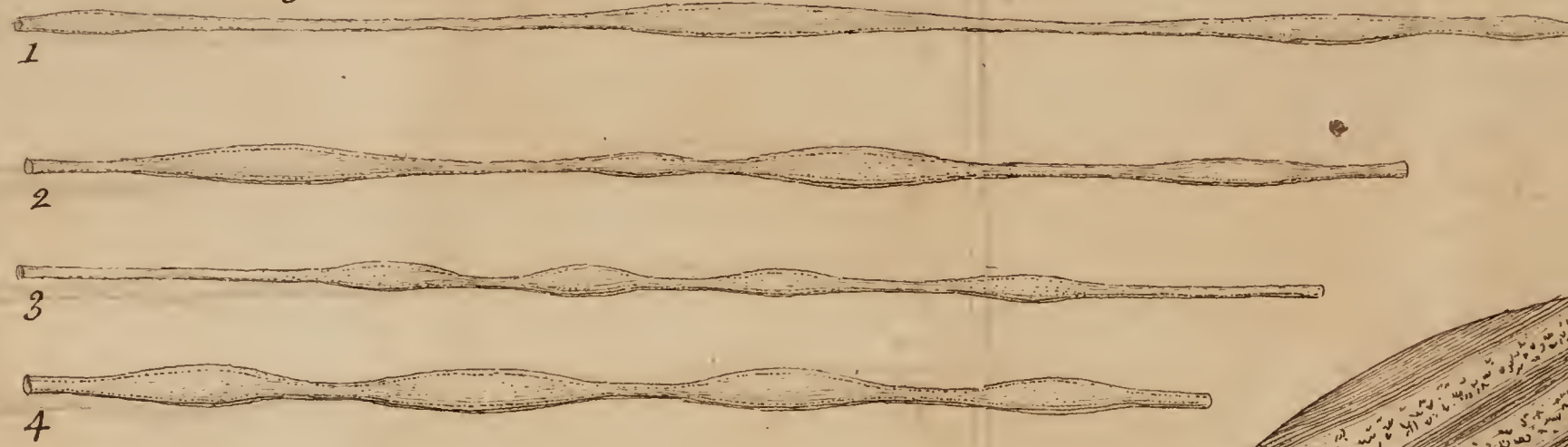


Fig. 126.



Fig. 128.

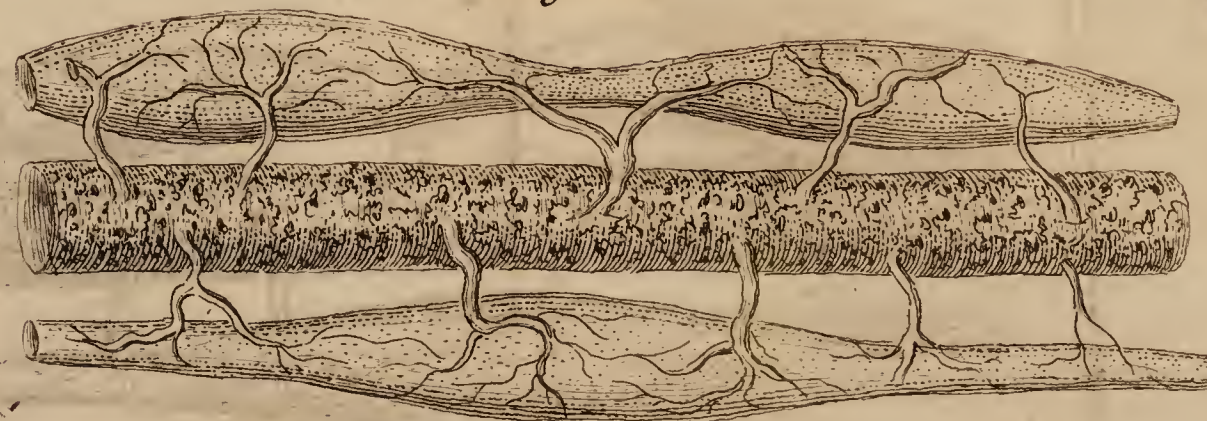


Fig. 127.



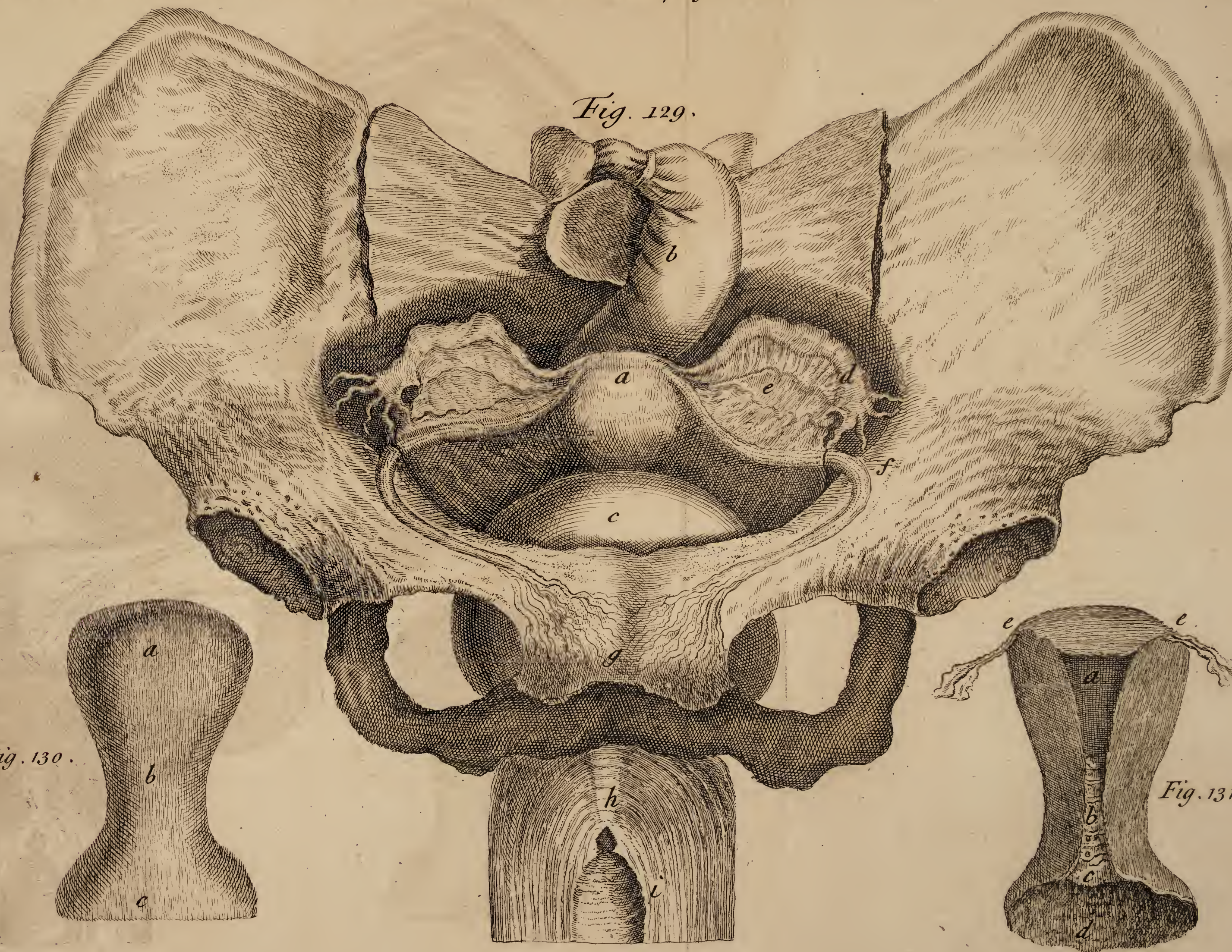


Fig. 130.

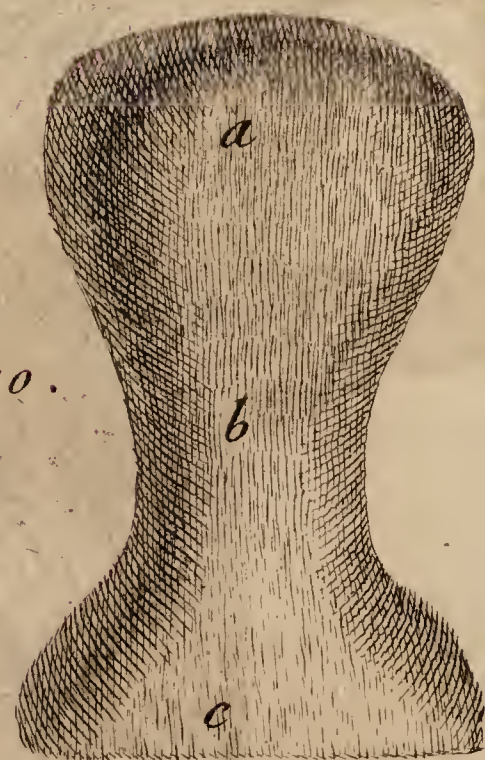


Fig. 131.



Fig. 132.



critic * *Santorini*, or he could not have quoted, besides *Eustachius*, two more modern authors for the discovery of the muscle called the *coccygæus*; neither of whom could ever have known this very name, if *Dr Douglas* had not invented it, when he discovered the muscle which he called so, some considerable time before he published his *Myographiæ comparatæ Specimen*, which the world was favoured with in 1707.

Now, although the first discovery of this muscle has been given to *Eustachius*, because something like it is seen upon a back view of one of his figures; yet, supposing that author's figures right as to this point, our *Dr Douglas* is absolutely as certain an original discoverer of this muscle as *Eustachius*; because the plates of the latter were concealed from the world till about 5 years after *Dr Douglas's* book came out; and even when they were found, they had no explanation, till *Lancisus* first explained them in the year 1712, who, notwithstanding, never took any notice of the *coccygæus* muscle in his explanations; although we may affirm *Morgagni* assisted in them: for *Lancisus*, after he had been ordered by the Pope to publish his explanations, wrote letters to one *Fantonus*, and to *Morgagni*, both Physicians and Professors of Anatomy, setting forth the difficulty of the task, and requiring their opinions and assistance in altering whatever they might see amiss in what he had done. He received answers from both; and in that of *Morgagni* a particular recapitulation of some part or other of the figure of every table, except that of the xxxvi. plate, which alone contains this muscle in question. But, afterwards he tells us, in his *Adversaria*, (which were printed as late as the year 1719.) that he discovered it himself, and calls it *Musculus Coccygis Levator*. See the end of his 45th *Animadversion*; for before this, neither *Lancisus*, *Fantonus*, nor *Morgagni*, took notice of it: whereas *Dr Douglas* not only discovered the muscle, and demonstrated it to the Students who attended his Lectures, but also invented the name for it, so many years as I have mentioned before *Eustachius's* plates were found. It would have been an ungenerous omission, to have neglected setting this part of anatomical history right, since I was so happy to be well acquainted with his great abilities in this and other branches of learning in his life-time; and particularly, since I find a foreign author, of so much credit as *Santorini*, very much mistaken in the matter; and a domestic author of no less note, the ingenious Professor *Monro*, calling it the *Coccygæus* of *Douglas*, or rather of *Eustachius*, who, though he might have seen the muscle, we might

* *Santorini*, in his *Observationes anatomicæ*, has these words; “*Quamvis postremæ levatoris fibræ triangulari illi musculo adstent, qui jamdiu ab Eustachio exhibitus, deinde ab clar. Cheseldeno, & Morgagno, tum retrahendo & levando coccygi, tum iis partibus firmandis tribuitur, &c.*” He also seems not to know what *Lancisus* mentions in his letter to *Dr Fantonus*, that *Morgagni* was invited to assist in the explanation of *Eustachius's* tables; and, consequently, that he was the less likely at that time to have any share in the discovery of that muscle, which no doubt appears on a back view of a muscular body in that author's 36th table, at the time of *Lancisus's* being employed to explain them; of which more hereafter.

be sure never dreamed of the name ; and, consequently, we have as much reason to ascribe the discovery of the *coccygæus* to *Douglas* as to *Eustachius*, if not more.

Of the frontal
muscle.

There are some differences amongst Anatomists concerning this muscle and the subsequent ; but, from the accuracy of the dissections of *Dr Douglas*, and my own observations, there is no better description can be exhibited than his, whose book I was intirely led by, during my dissections in Myology ; which was always strongly recommended by the Anatomists I followed, as the surest guide, during my studies, and which I shall now chiefly follow, as to the descriptions and uses of the muscles in question ; except where any new observation may perhaps make an alteration.

The true *frontal muscle* arises fleshy from the process of the *os frontis* next the inner or great angle of the orbit, above the joining of the *os nasi* and superior process of the *os maxillare* with this bone ; from thence it runs obliquely outwards and upwards, and is inserted into the fleshy part of the subsequent muscle, some of it's *fibrillæ* passing through into the skin a little higher than the middle region of the eye-brows. It's use is to smooth the forehead, by pulling it down after it has been wrinkled by the action of the *occipito-frontalis* ; and when it acts more forcibly, it serves to wrinkle the skin of the forehead between the eye-brows ; as it happens when we frown, or knit them.

Of the occi-
pito-frontalis.

This muscle arises fleshy from the transverse line of the *occiput*, opposite to part of the superior termination of the *mastoidæus*, and part of the beginning of the *trapezius* next it ; and then tendinous from the rest of that line backwards, arising after the same manner on the other side ; from thence it goes strait up ; and soon becoming all tendinous, it covers the parietal bones, and the *ossa squamosa*, above the temporal muscles ; it's outer edge being fastened to the *os jugale* on each side. This broad tendon near the coronal future grows fleshy, and descends with streight fibres as low as the *musculi orbiculares*, and ends in the skin at the eye-brows.

This muscle sends off, from it's lower part between the eyes, a narrow muscle (which *Dr Douglas* calls an elongation), that passes over the bones of the nose, as far downward as the end of the cartilaginous part, where it's fibres run off on each side, and end in the skin above the *musculus nasi proprius* : the office of the *occipito-frontalis* is to pull the skin of the head backward, drawing up the eye-brows, whereby the skin of the forehead is wrinkled. The power of moving this muscle is much greater in some than in others, so as to be able to shake off their caps ; and the same may be said of the muscles of the outer ear ; for nature designed strong motions to all these parts, if the manner of binding up infants heads did not deprive them of their natural actions, so as to leave them in an almost paralytic state.

OBSERVA-
TION.

Although the *occipital* and *frontal* portions of the last described muscle are by *Mr Cowper* accounted a continuation of each other, yet he confesses

confesses he follows the custom of other authors, by calling the fore-part the *frontal* muscle, and giving the name *occipito-frontalis* to the posterior portion only ; continuing the two names, in order as much as possible to avoid innovation, whereby he makes some confusion of names. However, although they are really but one muscle, only having one of it's fleshy parts forwards, and the other backwards, I see no reason why they might not have their appellations continued from their situations, with this difference only, that, instead of accounting them separate muscles, they should be only properly speaking, the *frontal* and *occipital* portions of the same muscle. And indeed Dr *Douglas* has justly called them the *occipito-frontalis*, as one *digastric* muscle, having a belly of fleshy fibres before and behind ; and separately described the true *frontal* muscle having it's origination from the process of the *os frontis* mentioned before, and terminating in the anterior fleshy part of the former. And although this *frontal* muscle (or *corrugator* of *Coiter*, who first discovered it) is the same that *Cowper* calls the internal fleshy production only of the *frontal*, yet *Cowper* declares he sees no reason to make them distinct muscles ; which I own surprizes me, since their fibres have a different direction and office, from the anterior fleshy part of the *occipito-frontalis*.

As to the actions of the true *frontal*, or *corrugator Coiteri*, I must dissent from all authors in one particular ; it is said, that this muscle serves to pull down the skin of the forehead, after it has been wrinkled. Now, let any one, by moving the *occipito-frontalis*, pull up the eye-brows, and wrinkle the forehead, the restitution of it will appear to him, as it does to me, to be spontaneous immediately upon the remission only of the action of the *occipito-frontalis* : whereas the true *frontal* muscle of *Coiter* and *Douglas* can act no otherwise, than in drawing together the eye-brows downwards and inwards, as in frowning ; for the fibres of this muscle are directed from their originations obliquely upwards and outwards ; and, consequently, when they are contracted, they can have no other motion towards their origination than in the oblique direction just mentioned ; for their action is successive to the spontaneous restitution of the skin of the forehead, after being wrinkled.

The eye-lids have only two pair of muscles amongst authors ; the *aperiens palpebram rectus*, and the *orbicularis palpebrarum* : the former ^{Of the muscles of the eye-lids.} of which being proper to the upper lid, and the latter being common to both upper and under, was the reason of these distinct names. But to these I shall now take the liberty to add a third pair, and call each the *occludens palpebram*, which consists of those fibres that cover the upper lid, distinguished from those of the *orbicularis* both in situation and office, as explained in the following observation. And as to fibres of the under lids, they are inconsiderable, serving for no other use, that I know of, but to render them of a sufficient thickness to match the edges of the upper eye-lids, when they pass over the eye, and come into contact with them. *Santorini* distinguished indeed the fibres upon the lids

from the circular fibres of the *orbicularis*; but says, their motion or action is at the same instant: which mistake we shall rectify in the following observation. He also makes the *occipito-frontalis* run downwards over part of the *orbicularis*, and sink into it about the *supercilia*; blending with each other between the inner *canthus* and the nose.

The *aperiens palpebram rectus* arises narrow from the upper part of the hole of the *sphænoidal bone*, through which the *optic nerve* passes between the originations of two of the muscles of the eyes, viz. the *attollens* and the *obliquus major*, and ends broad and tendinous in the edge of the upper lid, and serves to draw it up; whereby the eye is opened. This, with a little portion, by some called the *ciliaris*, are hid under the *orbicularis*; and therefore I have represented it in this drawing of a preparation, made on purpose, of all these muscles together in two different views, which will give a just idea of their structures and actions. See Fig. 134. and 135.

The *orbicularis* * arises tendinous and fleshy from the edge of the *os maxillare*, that makes the lower part of the orbit at the inner angle of the eye. Its fibres are spread upon the under lid, and a great part of the *os mali*, and, running round the little *canthus*, they are continued, authors say, over the upper lid, and upper part of the orbit at the great angle adhering to part of the *os frontis*, and superior process of the *os maxillare*. This muscle, say they, draws the two ciliary edges together, and shuts up the eye.

OBSERVATION.

It is no difficult matter to conceive how the eye-lid is opened; because there is a fixed point to which it is drawn back by the contraction of the *aperiens palpebram*: for, altho' the origination of this muscle is rather behind the eye than above it, as being at the bottom of the orbit, yet the end of pulling up the lid is answered, since the ball of the eye serves it's tendinous expansion as a *lever* or *pulley*, whilst the fleshy fibres behind are contracted, to favour it's ascent, as well as if it's origination was in a right line over it from the frontal bone: but the motion of the *orbicularis*, which is often compared to a *sphincter*, is more difficult to understand, and yet slightly passed by without any notice. Mr Cowper only says, This muscle, acting like the *sphincters* of other parts, constricts the eye-lids; and this seems the general notion of all Anatomists. Let us, however, attempt to explain this most wise contrivance for the motion of the eye-lids, in the following manner:

We may very securely affirm, that there is no analogy between a *sphincter* and the *palpebram occludens* (for so we shall venture to call it for the future). For, altho' there is an appearance of a circular direction of the fibres, above and below the eye-lids, in the expanded part or *orbicularis*, yet the fibres upon the eye-lids themselves have no such

* Most authors, and particularly Cowper and Douglas, make this muscle but one circular muscle; but Spigel. and Riolan. divide it into 2, the upper and under, and give them attachments at either angle of the orbit, to which Diemerbroeck subscribes.

appearance : they have attachments to the *cantbus* on each side, and the direction of their muscular fibres is, in some measure, parallel to the lids, though upwards somewhat curved ; otherwise the upper lid would not be capable of shutting down upon the under ; for it is this lid alone that is lifted up from the under, the latter having no need of a muscle to draw it down, but is naturally confined below the convexity of the globe of the eye, and is never capable of passing up even to the equatorial point with it's ciliary edge ; whereas any one may observe, that the ciliary edge of the upper lid (in shutting the eye) is carried down far below that point ; and, straitening itself below the convexity, forms a perfect concave cap over the globe when it meets the under lid ; which in full-eyed people is very conspicuous, and well worth observing. The manner of the upper lid's being drawn down is this :

The two fixed points of the ciliary edges are a considerable way below, and parallel to, the equator of the globe of the eye ; consequently, when the muscular fibres of the upper lid (which, whilst open, form curves of a certain dimension) contract themselves, they will endeavour to form strait lines by being shortened : this tendency to become strait will force each ciliary edge over, and beyond, the *equator*, even to form an inverted arch below it ; for, when once it is forced over the globe thus far, it will naturally pass a little further, since the convexity diminishes before it all along, to it's utmost contraction. See Fig. 136.

I have been thus willing to explain the manner of the occlusion of the eye ; 1. Because *Santorini*, in his figure of the face, makes the eye-lids meet upon the very *equator* of the eye-ball ; and others seem to think, that the upper eye-lid moves not much more than the under : and, 2. To shew how widely different the muscular fibres of the lids are, in their situation and action, from those of a *sphincter* (which is *Cowper's* opinion) whose office is to purse up any part round which they are placed, as the mouth, bladder, &c. when they act without restraint ; and therefore, since those circular fibres, which alone are to be called the *orbicularis*, can only act by purring themselves up round, they have no share in shutting the eye, which is done by the bringing down of the upper lid alone, as I have said already ; and, consequently, only serve to draw together the skin of the face all round, closing up the eye with more violence, and at the same time distorting the face ; for the eye-lids are naturally shut down, without the least discomposure of the countenance ; whereas this motion of the *orbicularis* is one of the expressions of pain and anguish ; or used to defend the eyes from dust, or the glaring rays of the sun when the eyes are weak.

Another short observation, worthy of note, is, that no one can lift up the eye-lids to their utmost height, without looking upwards, that is, without raising up the *pupil* of the eye at the same time, except a particular passion urges the contrary ; because the muscle which lifts up the lid, when there is an intent to raise it very high, draws the *musculus*

lus attollens, or *elevator* of the eye, into consent, and makes it act too; for they both arise from points that are very near each other; and the *aperiens palpebram* lies upon the *attollens*, or *elevator* of the eye, in it's whole length. And this consent between them is reciprocal; the sole reason for it being, that, when there is a necessity of looking up at an object, the lid should be pulled up out of the way.

Now, notwithstanding this wise contrivance that appears in the consent of these muscles upon opening the eye, there is a circumstance, wherein the *elevator* of the eye acts in pulling the *pupil* upwards, when the *aperiens palpebram* does not act at all, but continues shut; and that is, when one shuts the lids, in order to sleep; for, in that case, the *pupil* is turned up, as if nature intended, that, whilst the upper lid is drawn down, the *pupil* should be intimately covered, by being turned up under it, lest any rays of light might stimulate the eyes, and impede the sweet refreshment, which is so necessary after business and fatigue. This is the case with all who sleep with their lids close; and as to those who sleep with them open, it is very apparent in them; for, whosoever takes notice of such while they sleep, will find, that nothing is seen but the white of the eye; and, upon awaking them, will easily see the *pupil* turning down, as the lids open.

Muscles of
the eyes.

The eyes have 6 pair of muscles, 3 pair to each, which are, the *elevator*, *depressor*, *adductor*, *abductor*, *obliquus superior*, *obliquus inferior*, which, although they cannot be said to contribute aught to the formation or gesture of the face, yet they have no small share in the expression of the passions of the mind upon the countenance, by the different motions they give the eyes, in consent with the muscles of the face, upon particular occasions.

The *elevator* arises tendinous and fleshy from the edge of the *foramen lacerum* near the *abductor*, terminates into the upper and fore part of the *tunica sclerotis* of the eye, by a thin tendon, and serves to raise up the globe of the eye.

The *depressor* arises tendinous and fleshy from the lower edge of the hole that gives passage to the optic nerve, is inserted by a thin tendon into the same coat opposite to the former, and serves to draw down the globe of the eye.

The *adductor* arises tendinous and fleshy from the edge of the hole of the optic nerve, between the *obliquus major* and the *humilis*, ends in the same coat towards the nose by a thin tendon, and pulls the globe of the eye that way.

The *abductor* springs from the *foramen lacerum* without the orbit of the eye, and is inserted into the same coat, called *sclerotis*, against the former, whose office is to draw the eye from the nose.

The *obliquus superior* has it's rise from the edge of the hole through which the optic nerve passes, between the *elevator* and *adductor*, runs along the *os planum* upwards, passing through the *trochlea*, a little pulley fixed to the frontal bone, and, doubling downwards again, is
fixed

fixed by a thin tendon into the same coat, behind the tendinous expansion of the *elevator*, whose office is to incline the eye forwards, with it's *pupil* downwards.

The *obliquus inferior* arises tendinous from the edge of the orbit, where the *os maxillare* joins the *os mali*, and ends backwards and inwards between the optic nerve and the tendon of the *abductor*, drawing the eye forwards, and inclining the *pupil* upwards.

Although the eye is said, in our description, to be drawn this or that way (and it is indeed the language of all Anatomists) according as the muscle acts; yet, strictly speaking, it is only turned or rolled this or that way, as a moveable ball in a socket; by which motion it is the *pupil* alone that can be said to change it's place, as a spot upon a globe, whose situation is altered by the motion of such a body round it's *axis*: so that, since the eye is moved only for the sake of vision, when the *elevator*, for example, is contracted, by drawing the place of the globe, to which it is attached, backwards; it's spot or *pupil* turns upwards; and again, when the *depressor* is contracted, by drawing it's fixed point backwards, the *pupil* is turned downwards. The same may be said of the other muscles that move the eye; for, as the 4 first have their rise very near one another, the eye with these 4 muscles in their natural situation, make the figure of a cone, whose *basis* is forwards, and the *apex* backwards; and therefore each, when it is respectively contracted, must necessarily pull the place of it's attachment backwards, and so move the *pupil* upwards, downwards, to the nose, or from it; and the business of the oblique muscles is the same, except what regards the points of their vellication, whereby they are pulled in an oblique direction to the 4 strait motions mentioned above.

The eyes are capable of a rotatory motion, which authors have ascribed to the power of the oblique muscles; but every other muscle of the eye contributes to it's rotation as well as these, which of themselves can do no more than the office assigned them in the above description; but that motion is thus performed: let any one look upwards at the centre of an horizontal line, on a wall, of several feet long; he will have it in his power to carry his view along that line to the right or left, to either extremity, without moving his head. Now the *elevator* of each eye elevates the *pupil* to the central point of vision mentioned: but the question is, how the *pupils* are carried to the extremities of the line? To answer this, let us consider the state of the muscles: the tendinous attachments of the 4 strait muscles are broad and thin, where they are fixed to the *sclerotis*; so that the edge of one expansion is very near that of the other all round the globe; and the fleshy fibres from them also are disposed in a thin flat order backwards, until they approach the bones from whence they arise; where, for the convenience of their attachment, they are squeezed closer together than when

OBSERVATION.

when they first quit the globe: from this position they may be said to form a hollow cone, having the optic nerve as their *axis*. See *Fig. 137. a.*

Now, suppose the sight fixed to the middle of that line above-mentioned, by the *elevator* of each eye, and it was resolved to carry it along the line to the right; then the fibres on the left side of the *elevator* of each eye would remit their actions, and those of the right side of each muscle contract more strongly, until they are succeeded and assisted by as many of the neighbouring fibres of the *abductor* of the right eye, and the *adductor* of the left, as will serve to maintain the view all along the line to the extremity on that side, and *vice versa*.

But if a circle was described in a vertical plane, and it was resolved to carry the sight all round it, to the right or left; then supposing one, for example, begins at the top, and proceeds to the right, every individual fibre succeeds the other on that side in both eyes, till the last of the one muscle, as it were, delivers over it's office to the next fibre of it's neighbouring muscle, until the rotation is carryed all round as oft as the person pleaseth: now in this rotatory motion, the oblique muscles seem to me to have very little share.

Hence it appears, that, in moving the *pupil* upwards, downwards, or to either side, all the fibres of each muscle serving to these motions act together; but if any other motion is required between these, part of the fibres of one, may join and assist part of those of it's neighbouring muscle, in order to perform it: and this will further appear, if we do but consider, that when, for example, the *obliquus superior* of either eye acts, it serves to incline the eye forwards towards the nose, with it's *pupil* downwards; and, of consequence, the *pupil* of the other eye must be carried from the nose, and downwards too; which can be done no other way than by the combination of part of the fibres of the *abductor*, with some of those of the *depressor* next them.

If, without moving the head, one would endeavour to describe a circular line with the *pupils* of the eyes, it can scarce be done truly circular, because one is apt to fix upon different points successively, from each of which the eye makes a right line, and therefore would rather describe a polygon than a true circle, from the intermissions we are apt to make in pursuing such a view; but if one looks at the images of his eyes in a mirror, without moving his head, and the mirror be moved to describe a circle, then the eyes will have a true rotation, without the least intermission whatsoever; or if the mirror be fixed, and one describes a circle with one's head, keeping the eyes fixed upon their image, the same will happen.

Authors have given other names to the muscles of the eyes, besides these mentioned above: the *elevator* is called the *superbus*; the *depressor*, the *humilis*; the *adductor*, the *bibitorius*; the *abductor*, the *indignatorius*; and the oblique muscles, *rotatores* and *amatorii*. How far these names are justly or improperly applied, will appear in our explanation of

of the figures which represent the passions; however, we shall here make an observation relating to short-sighted eyes, and such as squint.

When the eyes are very convex, their focal distance is very short; and therefore, when they view an object, it is put to the eyes, and generally held on one side; for, by holding it thus, they can have a clearer sight of the object, than if held right before them at equal distances from both eyes; because the *adductor* of each eye is violently strained and contracted, in order to draw both *pupils* to the object which is troublesome; and it is only in this case that these 2 muscles act together as antagonists to the 2 *abductors*, and bring them to squinting; whereas, when the object is held a little sideways; though the *pupil* is brought near the nose on one side, that of the other will be carried in some measure from it on that side towards which the object is held, and thereby the proper muscles will act together; that is, the *adductor* on one side will act in concert with the *abductor* on the other, and *vice versa*, although not so perfectly as when the convexity of the eyes is more natural. The same squinting distortion will happen to any one who places an object too near his nose. Now the reason why the *pupils* of the eyes must follow one another in viewing objects, is, because the mind can attend to no more than one object at a time; and since one eye cannot discern so well as both together, let the object be placed where it will, those muscles, whose contractions serve to conduct both *pupils* towards it, are only employed by the will: and hence it is, that the *abductors* can never both act together according to the will; for one eye in that case would be intirely useless, and the aspect of the person unnatural and unseemly besides. *Aristotle*, Sect. 3. Prob. 1. makes a query, why the eyes act together? and answers, That it is because the motion of both has one and the same principle; which is, says he, the conjunction of the optic nerves. *Galen*, and many authors since, had the same way of thinking: but how strange it is, to find authors seeking for the cause of the uniform motions of the eyes in the optic nerves, which, every one must allow, serve not to promote any motion at all, nor have they any other office than to promote vision alone; whereas the eyes are moved by the third pair of nerves, which go to the muscles, and serve to move them, and are therefore called the *motores oculorum*; which move them uniformly, for the plain natural reason just given; and howsoever the union or conjunction of optics may be, or any other constitution of them, there can be nothing intended by such circumstances, but the welfare and security of those organs, which in every oher part of nature we daily see has been the benevolent care of our GREAT CREATOR. But further, many are found to have no conjunction of the optic nerves, and yet have neither seen objects double, nor were their eyes irregular in their motions.

There are several of the muscles of the lips, and other parts, which Muscles of the are common to the nose, and but few that are proper to it: the latter nose.
are,

The slip, or
elongation of
the occipito-
frontalis.

are, the *rinæus* or *nasalis* of Douglas, or *pinnarum dilatator proprius* of Santorini, which arises fleshy from the extremity of the *os nasi*, and terminates in the intire cartilage of the *ala nasi*, spreading itself obliquely backwards: it's office is to draw the *ala* upwards and backwards, in order to dilate the nostrils, in which it is assisted by the slip, or elongation of the *occipito-frontalis*, which, as it is continued from thence down the nose, and dividing at the middle, terminates on each side, partly in the aforefaid muscle, and partly in the skin above it. This slip I take to be proper to the nose, and serves to assist the former in it's office, and also to wrinkle the skin of the nose, by drawing it's lower part upward; which happens in the act of *derision*, and in many when they laugh much. Besides this, Santorini has mentioned another new muscle, which he calls *Musculus novus transversus*, which, like a saddle, rides cross the lower end of the bones of the nose, and, running over the insertions of the *rinæus*, terminates in the *pyramidalis* on each side. This muscle is so exceeding thin as not to be distinguished in every subject; and as it's use, from it's situation, can be only to wrinkle the skin of the nose, it may well be spared, since that office is well enough performed by the above-mentioned muscle, and by another *slip*, which is a companion to the *pyramidalis*, whose fibres run parallel to the nose, from the great *canthus* of the eye, and is inserted into the upper lip, and in the *ala nasi* on each side.

The muscles
of the lips.

As the cheeks have no motion of their own, being only moved in common with some one or more of the muscles of the lips or jaws, we shall give our description of the *buccinator* a place with the muscles of the lips; and as to the formation of the cheeks, they cannot be said to be formed of the *buccinator* or *quadratus genæ* alone, for all those that move the lips contribute to it also; and whatever motion the *buccinator* may be said to have, it is proper to the lips and mouth alone, and not to the cheeks. Now the mouth being the central point, from which all the muscles that move the lips, as it were, radiate, the first that ought to be described is the *orbicularis*, by some called the *osculatorius*, and by others the *sphincter labiorum*; because whatsoever muscle moves of those round about it, the spontaneous form of this is inevitably altered; we shall therefore first describe the *orbicularis*.

The orbicu-
laris.

Notwithstanding that the natural position of the lips makes not a circular form, yet the fibres of this muscle go round it; however, they cannot properly be said to be orbicular, or be likened to a ring, on any account; altho' their action is only to purse up the mouth, as in whistling and blowing.

The *Elevator labii inferioris proprius* arises from the lower jaw, near the gums of the fore teeth, and ends in the skin of the chin, which, with the lower lip, it draws upwards.

The *Elevator labii superioris proprius* arises from all that portion of the *os maxillare* that makes the lower part of the orbit, above the hole that transmits the nerves and arteries to the cheeks, and ends in the upper

upper lip : this is what *Santorini*, and other authors, call the *pyramidalis*, which serves to pull the upper lip upwards on each side.

The *Depressor labii inferioris proprius* arises fleshy from the lower part of the chin, and is inserted into the under lip, and serves to pull it downwards and outwards.

The *Depressor labii superioris proprius* arises thin and fleshy from the *os maxillare*, immediately above the gums of the *dentes incisivi*, and ends in the upper lip, pretty high under the nose ; which it serves to draw downwards.

The *Elevator labiorum communis* arises thin and fleshy from the hollow of the *os maxillare*, under the great hole, and ends in the angle of the mouth. *Santorini* calls this, *Seu caninus extra oris rictum terminatus* ; it's use is to bring the corner of the mouth upwards on each side.

The *Depressor labiorum communis*, or *triangularis*, arises broad and fleshy from the lower edge of the lower jaw, between the *latissimus colli* and the *masseter* ; it is also inserted into the angle of the mouth, decussating with some of the fibres of the following muscle, and serves to pull down the corners of the mouth.

The *zigomaticus major* arises fleshy from the *os mali*, near it's conjunction with the process of the temporal bone, runs down obliquely towards the angle of the mouth, over the insertion of the salival duct into the *buccinator*, and, spreading, joins the end of the former muscle, about the *rictus oris*. It's office is to pull the angle of the mouth obliquely upwards.

The *zigomaticus minor* arises from the same bone, forwards of the former ; and ends under the *pyramidalis*, and serves to assist in the elevation of the upper lip.

The *risorius* is a flat bundle of fibres, which arises near the angle of the jaw from the skin over the parotid gland, and is inserted into the upper part of the *triangularis* near the *rictus oris* : it lies before the upper part of the *quadratus genæ* ; and serves to draw the angle of the mouth gently backwards.

The *buccinator* arises from 2 different places, that is, from the most remote part of the lower jaw, between the last tooth and the coronoid process, and from between the last jaw-tooth of the *maxilla superior*, and the *pterigoid* process : and, running forwards broad, is inserted at the angle of the lips into the *orbicularis*, behind the *triangularis* and *zigomaticus major*. It serves to several purposes ; first, to squeeze against the gums, in order to direct the victuals while we chew, both between the teeth, and thence into the cavity of the mouth ; and, 2dly, to assist in forming the face for laughter. Now in smiling, the *risorius* is sufficient of itself ; but, when great laughter is promoted, the *buccinator* takes the whole action to itself, which is begun by the former, and pulls the corner of the mouth on each side, and stretches the lips prodigiously, according to the degree of laughter required.

Of the qua-
dratus genæ,
or latissimus
colli.

Notwithstanding this muscle is not among the muscles of the lips, we shall mention it here as a muscle which belongs partly to the face. It arises thin, with some membranous, and some fleshy fibres, from the *sternum* or breast-bone, the *acromion* of the *scapula*, and from the intire space between them occupied by the pectoral and deltoid muscles, and is inserted partly into the lower jaw, partly into the *buccinator* muscle, and partly by a thin membrane under most part of the skin of the face. It serves, according to the opinion of Dr *Douglas*, to draw the skin of the face downwards, and to assist the digastric in opening the jaws. This last office is denied by *Heister*, and some others; but, as there is a strong insertion into the lower jaw of the most considerable part of this muscle, I cannot but join with the former, in the opinion that it's chief office is to open the jaws: for the fibres which go to the skin of the face are very slender and weak; insomuch that, in many subjects, they are not to be discerned at all. As to the muscles proper to the lower jaw, viz. the *temporalis* and *masseter*, which pull it upwards, and the *digastricus*, *pterygoidæus internus*, and *pterygoidæus externus*, which serve to draw it downwards, we shall omit their descriptions here, since they do not come under the subject we are at present confined to; which is only to treat of those muscles alone, whose motions are subject to the influences of the passions of the mind, in altering the countenance, or which serve to it's formation.

LECTURE II. In my last Lecture I had the honour of demonstrating before you the muscles of the face, and, at the same time, of explaining their sensible motions: it now remains to shew you which of these muscles act, in the several motions of the face that express the different passions of the mind; for they serve two principal ends, first, (altogether) to form the symmetry of the countenance, by supporting the skin of the face, in the manner we see it when a general *composure* appears thro' the whole; and, secondly, to express, as we have said, those passions of *joy*, *grief*, *fury*, *ill-nature*, and such-like, as the mind is often prone to suggest; and may indeed be said to become the glory or disgrace of the man, according as they obey the dictates of the mind in those cases; or, in other words, as they are most predominant.

It is certain (whatever stress may be laid on that trite phrase "*Fronti nulla fides*"), that, if we but duly consider, what is a great truth, that the countenance is the *nuncio* of the mind, and only become well versed in the proper actions and predominancy of it's several muscles, we shall have a great deal of reason to reverse that sentence; since also we must, from every day's experience, see a face promise what we afterwards find to be the real disposition of the person who wears it.

PROVIDENCE is wise, in causing this to be so; and it has it's considerable uses in nature. Does it not *cheer* and *please* society, to see an open cheerful face among them? Does not a sorrowful aspect move mankind, who are naturally prone to compassion? Does not a countenance expressing fear give warning to others to prepare against the danger?

ger? does not a sneering scornful face warn us to beware of it's owner? And does not a surly, morose, or dogged aspect give men distrust, forbid friendship, and fill societies with uneasy apprehensions?

It was because the means of self-preservation should be generously distributed to us, that the prevailing characteristics of tempers should be thus conspicuous in us; innumerable instances of which are to be observed in every other part of the animal world besides: and even from hence we might naturally conclude it absolutely necessary; but the structures of these parts, their sensible actions, and the great consent between one part of the animal and another (from their nervous communications), yet more plainly confirm this conclusion.

Whatsoever sovereignty the diaphragm is known to have in respiration, which concerns the circulation of the blood in the greatest measure, since the seat of the heart is formed upon that organ in human bodies, I am now almost confident, from many scrutinies and observations made upon it for several years, that it has no small share in being a principal *instrument* of receiving, and communicating the impulses of the *will* to the several parts which are destined for the expression or publication of the intention of that *will* or *mind*: now, in order to prove this, let us only consider, that, when an object of mirth offers to us, the laughter, or quick concussion of the lungs, is begun by the diaphragm; which is no sooner set in motion, to express the pleasure conceived at it, than the muscles serving to form a smile upon the countenance, are immediately influenced, and a gentler, or more forcible drawing of the corners of the mouth backwards is excited, according to the degree of mirth before us. Now, from the first influence of the diaphragm, when grief is the subject also, we plainly see, that the muscles, which are proper to express that passion, are alone actuated; and when it increases to an immoderate weeping, there are not wanting concussions of the diaphragm here too, as well as in immoderate laughter; so that there may be said to be a reciprocal commerce between the diaphragm and the muscles of the face, not only to demonstrate these passions, but indeed all others we see imprinted on the countenance, by means of the nervous sympathy between them.

From the *Phil. Trans.* N^o. 153. p. 395, being an extract of the 24th dissertation of *Spon's Recherches curieuses*, &c. printed at Lyons 1683. in 4to. it is easy to see what a slight foundation they formerly had for their notions of the matter: “ We learn, says he, the use of ancient
“ medals, pictures, and statues (of which *Varro*, *J. Cæsar*, and *Alex-*
“ *ander Severus*, were great collectors) as relating to other studies, so
“ especially to Physiognomy, nature having imprinted in the counte-
“ nance certain airs and conformations, which discover the grand incli-
“ nations of the mind. In this art the famous *Campanella* was a great
“ master, as Mr *Choner* relates in the life of *Boisot*. Hence *Nisius*
“ *Erythraeus* tells us, that *B. Stephanus* the Poet had the same fea-
“ tures with the statues of *Virgil*. Others observe, that *Numa Pom-*

“ *pilius* and *Antoninus Pius* resembled each other in face and manners ;
 “ and that the Chancellor *Hospitalius*, a great Philosopher, was like
 “ the figure of *Aristotle*. The face of *Alexander Magnus* upon his coins,
 “ his eyes set high and great, with his chin thrust out, speak him
 “ haughty, earnest, and courageous, as *Plutarch* remarked from the
 “ Physiognomists. The frizzled hair of *Pompey*, and his forward coun-
 “ tenance, shew his stoutness and ambition. The temperament and
 “ disposition of *Julius Cæsar* is read in his coins, as divers have obser-
 “ ved, and at length Dr *Andreas*, out of *Argoli*: *Marcus Antoninus*’s
 “ double chin shews his love of pleasure. The air of King *Juba* argues
 “ him cruel and arrogant. The good features of *Augustus* declare an
 “ excellent mind, a mixture of sweetness, and prudence, and courage.
 “ The little eyes of *Nero*, his thick neck, his throat and chin conjoined,
 “ were no good signs to the *Romans* ; and the stature of *Maximinus*,
 “ and narrow chin, bespake his cruelty.”

But all this kind of doctrine of Physiognomy must very often deceive, because it was taken chiefly from incidents that happened during the reigns and authority of great personages, who were very conspicuous in their stations, and who, consequently, drew the attention of these observers upon themselves. Thus when the character of the person was known, an account of his face was immediately taken, and a standard laid down for every person, who in any wise had a resemblance to it : as if the length or shortness of nose or chin could be an indication of the disposition of the mind : but such observers should have considered, that the general form of the face and head is chiefly owing to the structure of the bones that compose them ; and that a person with a long chin or nose, &c. may be either of a good or bad turn of mind ; and, on the contrary, those with the best proportioned faces may be possessed of unhappy as well as happy tempers : so that, let this be as it will, it is the alteration of the muscles alone that is capable of demonstrating the reigning passion of the mind upon every kind of face.

Some observers took their indications from the tints of the countenance, and judged according to them ; that is, from the pale, red, livid, brown, yellowish, or olive. But, however these may serve as indications whereby to lead to the knowledge of diseases, every day’s experience shews they can give no information of the disposition of the mind ; for there are, amongst men of all hues, both good and bad, cheerful and dejected, and the like.

Aristotle, who has among his works a particular treatise upon Physiognomy, and which one would imagine has been the ground-work on which the writers upon that subject have raised all they have said about it, has not omitted one circumstance that might in any wise help him in his ingenious researches, but those which alone could give the true tokens of the dispositions of the mind, that is, the actions of the muscles of the face. He has drawn some injudicious suppositions from the native countries of men ; others, from the strength or weakness of their limbs ;

limbs ; others, from the nature or colour of the hair ; others, from the shortness, length, hardness, or softness, of the flesh and limbs ; and has laid great stress upon the likeness of the human voice to that of other animals : thus, if the voice was loud and sonorous, he compared the person to a lion, bull, or barking dog, according to their differences ; and if, on the contrary, the voice was feeble and mild, he concluded the person pusillanimous and fearful. If women were strong, healthy, and of good constitutions, he declared them of a masculine nature ; and if men were weakly, he esteemed them as having more of the female dispositions than otherwise, and also drew some remarks from the size, roundness, length, flatness, or protuberance of the face, without ever dreaming of the use of the muscles belonging to it.

Lancisius has written a particular treatise, to his friend *Fantonus*, upon this subject, which he calls *Dissertatio Physiognomonica*, and divides it into 2 parts ; the first he calls *Chiroscopia*, which treats of several things relating to the lines on the palm of the hand ; as their origin ; why they are more in the hands than the soles of the feet ; 3 kinds in the palms ; the causes of the best and worst kinds, and of their defects ; with prognostics drawn from them, and the like : the second part he calls *Metoposcopia*, which relates only to the countenance, and is the least considerable ; wherein he chiefly considers the *rugæ* of the forehead, which he divides into 3 kinds : 1. Such as are equal and strait : 2. Such as are unequal and interrupted : 3. When they are but few and superficial : and the only consequences he draws from these wrinkles are, that the equal ones signify strength and *æquilibrium* in the frontal muscles ; the unequal ones the contrary ; and very truly says, that if the skin be thin and lean, there are more wrinkles ; if thick, a smaller number ; and, lastly, when they are but few, and are superficial, it shews, says he, great weakness of the muscle. Now, from these conditions of the muscles of the forehead, he judges of the condition of the brain : if that be strong, so is the brain ; if weak, the latter must be so too, and the like.

This author places the seat of the soul somewhere in the fore part of the brain ; and thinks he has found a new part, which was never taken notice of before, and promises the publication of it. He has nothing that particularly relates to a demonstration of the passions of the mind on the countenance, but this general notion : “ It is not a difficult matter (says he) to discern an angry man by his flaming eyes ; nor an envious man by their distortion and frowning ; nor a fearful man by his paleness and trembling ; nor a melancholy man, by his dark and dejected countenance ; nor, in fine, a happy temper, by a florid and cheerful aspect.” Indeed there is no great art in seeing these indications, which are obvious to the meanest capacities ; but how they are so, and the reasons for them, this great man has not thought worth while to explain. The same may be said of that great master *Le Brun*, who (in his *Abregé d’une Conférence sur la Physiognomie*, at the end of his book of the passions) lays great stress upon following the traces
of

of the lineaments in the faces of brutes, in order to account for the appetites and passions of men.

J. Bapt. Porta, who is well known to the men of science of all nations, built his system of Physiognomy upon that of *Aristotle*; which he attempted to explain, in a book intituled, *De humana Physiognomia*, &c. wherein he lays it down, as a certain truth, that whosoever has a likeness in his face to that of any other animal (though never so remote), his frame of mind and passions must be the same with those of the animal whose resemblance he bears: and accordingly, he makes exaggerated figures of mens countenances like lions, tigers, lambs, and other creatures, with remarks upon them, in order to give weight to the system he lays down: but had he only studied the parts which constitute the face, and their obedience to the impulses of the mind, he could not have persisted in a method of accounting for it's passions, by comparing the faces of men to those of creatures, which can have no more analogy to each other, than the forced imaginations of his brain could produce.

Many of the Ancients were strongly of opinion, that mens faces discovered their tempers: and this must have been founded chiefly upon a long observation and experience of the tempers of men with whom they had frequently conversed; for their penetration in that part of Anatomy had not gone so far, as at present it does with us.

The scholars of *Socrates* brought a noted Physiognomist, *Zopyrus*, to their master, in order to try his art; who viewing his face for some time, having had no previous knowledge of him, and after an examination of his aspect, he soon pronounced him the most lewd, drunken old fellow he had ever met with: the disciples mocked and laughed at him, as believing his art of no effect; but *Socrates* told them, he believed his art might be true, notwithstanding his present mistake, for that he himself was naturally inclined to those particular vices the Physiognomist had discovered in his countenance, but that he had conquered the strong dispositions he was born with by the dictates of Philosophy.

Ovid was of this opinion, as appears by this beautiful passage;

Heu, quam difficile est, crimen non prodere vultu!

which the great *Addison* chose for a motto to the 86th *Spectator*, wherein he has the following refined sentiments upon this subject: “ Every
“ one (says he) is in some degree a master of that art, which is gene-
“ rally distinguished by the name of Physiognomy; and naturally
“ forms to himself the character or fortune of a stranger from the
“ features and lineaments of his face. We are no sooner presented to
“ any one we never saw before, but we are immediately struck with
“ the idea of a proud, a reserved, an affable, or a good-natured man;
“ and,

“ and, upon our first going into a company of strangers, our benevo-
 “ lence or aversion, awe or contempt, rises naturally toward several
 “ particular persons, before we have heard them speak a single
 “ word, or so much as know who they are.

“ Every passion gives a particular cast to the countenance, and is
 “ apt to discover itself in some feature or other: I have seen an eye
 “ curse for half an hour together, and an eye-brow call a man scoun-
 “ drel.—For my part (says he) I am so apt to frame a notion of
 “ every man’s humour or circumstances by his looks, that I have
 “ sometimes employed myself from *Charing-Cross* to the *Royal Ex-*
 “ *change*, in drawing the characters of those who have passed by me:
 “ when I see a man with a sour ravelled face, I cannot forbear pitying
 “ his wife; and when I meet with an open ingenuous countenance,
 “ think on the happiness of his friends, his family, and relations.—
 “ I think we may be better known by our looks than by our words;
 “ and that a man’s speech is much more easily disguised than his
 “ countenance. In this case however, I think the air of the whole face
 “ is much more expressive than the lines of it: the truth of it is,
 “ the air is generally nothing else but the inward disposition of the mind
 “ made visible.”

This maxim I am now about to prove to you in a few words, by
 answering an objection made to me on that sentiment; “ A Gentleman
 agreed with me, that the muscles of the face obeyed and expressed the
 different passions of the mind occasionally, but that, when the cause of
 that passion ceases, the face resumes it’s natural position, and that
 passion appears no more for that time.”

To this I answer, 1st, That every person has a particular *bent* or
disposition of mind, which oftener reigns in him than any other; 2^{dly},
 That this habitual disposition, causing the muscles of the face, that are
 destined to express it, frequently to act in obedience to that bent of
 mind, brings on at length an habitual appearance of that passion in the
 face, and moulds it into a constant consent with the mind. In the
 course of my own acquaintance, I know some persons who wear on
 their countenances a continual cheerfulness, complacency, and openness;
 and, by experience, I know it to be their continual disposition of mind:
 and, on the other hand, I also know some, on whose faces a settled
 moroseness always strikes the beholder; and know it to be their own
 constant plague, and that of those among whom they come.

This need not be wondered at, nor indeed can it be denied; for
 that such a habitude of countenance is easily acquired will appear
 certain, if we do but observe what happens every day: we may take
 notice that among school-boys, if there be one who stammers in his speech,
 it will speedily be acquired by the others; and I have known two boys,
 whose fathers were my acquaintance, catch that habit of speaking at
 school, and have never yet been able to shake it off: I have also
 known a youth catch a habit of winking the upper lid of his eyes, quicker
 than

than the seconds of a clock, by having sometimes seen an *epileptic*, who frequented the quarter where he lived; and I personally knew a Gentleman eminent in the Law, who incessantly winked with one eye; and told me, to satisfy my curiosity, that his father had a servant, when he was a child, who had an involuntary winking, which he had acquired, and which, notwithstanding his having been sent away to a distant school, when his parents had discovered it, he never was able to shake off. Add to this, that there are many who acquire squinting distortions of their very eyes, by seeing others too often who are troubled with that malady. Now what but a habit, acquired by some muscle or other, could have produced these effects?

However, in order to be as fair and clear as I can, in the proof of my assertion, I will answer another objection of the same Gentleman, which is all I shall at present trouble you with upon this head.

Says my friend, "I have known one of these crabbed four-faced men look as agreeably as one could wish; and, on the other hand (says he), one of those merry-faced Gentlemen put on a countenance as full of fury as possible." It is very true, I agree with him in this; and it is right it should be so; because every person, of whatsoever temper of mind, ought to have a power of altering it upon a necessary occasion, and of shewing his approbation, or dislike, of any affair that may chance to offer: but is the morose man long pleased, or the happy man long displeased? And does not each return soon to his former habitual mind and countenance, when the occasion of their alteration ceases?

My friend might have started other objections, and which indeed are, in some measure, exceptions to the general system I have laid down; but cannot invalidate the least part of it, when the reasons for them are explained. 1. He may argue, that there are many *maniacs*, who are in a constant state of madness, and yet the muscles of the face are in no wise distorted, nor any other appearance of wildness in the countenance than what the eyes produce: but this happens only because the morbid madness is involuntary; whereas all the passions of the mind, which correspond with, and actuate, the muscles of the face, are voluntary, and, consequently, draw the muscles subservient to each particular one into consent. The player, acting his part with judgment, is capable of producing these effects; he can artfully put on the grimace which best expresses or accompanies the character he is to represent; as the man, whose natural temper also leads him to wear the aspect suited to his state of mind, whatsoever it be; and as such a one can occasionally change his aspect, though but for a little time, from its customary state, it is plain, that the customary passions of the mind are first voluntary, and the actions of their several subservient muscles are so too: and, 2. There can be no reigning gesture of the face discernable in *idiots* expressive of any passion; because they can have no settled intention to produce one, or render it customary in their general conduct; whence it

it may be very justly said, such poor creatures have unmeaning countenances.

From what I have hitherto laid down, and from the following explanation of the figures, I hope it will appear, that no analogy can be drawn from brutes, no signs from the voice, nor general shape of the face, or any of it's parts ; in a word, nothing but the actions of the muscles, become habitual in obedience to the reigning tempers of the mind, can in any wise account for them ; and the art of Physiognomy, especially the *metoposcopy*, or what relates to the face, must prove very uncertain without this foundation.

Fig. 133. shews a view of the muscles of the face in profil.

Fig. 133.

a, the anterior portion of the *occipito-frontalis*. *b*, the *orbicularis* of the orbit. ***, the *occludens palpebram*. *c*, the *temporal* muscle. *d*, the *masseter*. *e*, the muscle *attollens auris*. *f*, the *zygomaticus major*. *g*, the *zygomaticus minor*. *h*, *Pyramidalis*, seu *elevator labii superioris proprius*. *i*, *Pyramidalis socius* Santorini. *k*, *Elevator labii superioris proprius*, seu *incisorius* of Cowper. *l*, *Elevator communis labiorum*. *m*, the *osculatorius*. *n*, a bundle of fibres running down with the *incisorius* from the bottom of the *orbicularis*. *o*, the *ductus parotidis*, or salival duct. *p*, *Glandula parotis*. *q*, the *buccinator* muscle. *r*, the *risorius*. *s*, the *triangularis*, or common *depressor labiorum*. 1. the *corrugator* of the chin. 2. the *depressor labii inferioris proprius*. 3. the new transverse muscle of the nose, of Santorini. 4. the *rinæus* of Douglas. 5. the *elongatio occipito-frontalis* of Douglas. 6. the *narium contractores*. 7. the *quadratus genæ*, *latissimus colli*, or *platisma myoides*. 8. the skin turned back.

Fig. 134. is a view of the orbicular and palpebral muscles, taken off from the head.

A, the orbicular muscle. *B*, the *occludens palpebram*. *c*, the lacrymal ducts. *d*, the lacrymal gland. *e*, the ciliary ligaments. *f*, the ciliary portions.

Fig. 135. shews the surface of the same muscles which lie next the bones.

A, the *orbicularis*. *b*, the *palpebram attollens*. *c*, the lacrymal ducts. *d*, the lacrymal gland. *e*, the ciliary ligaments. *f*, the portions arising from the tendon of the *attollens palpebram*, called by some the ciliary portions.

Fig. 136. explains the motion of the eye-lid.

Fig. 136.

A, the *attollens palpebram*. *B*, the upper lid. *C*, the under lid. *D*, the globe of the eye. *E*, the *equator* of the eye. *FF*, the fixed points of the muscular fibres of the lids, or *occludens palpebram*. *G*, the line of the upper lid, when it passes towards the under lid, over the *equator*.

Fig. 137. represents a back view of the ball of the eye.

Fig. 137.

a, the optic nerve. *b*, the *musculus attollens*, or *elevator*. *c*, the *depressor*.

pressor. *d*, the adductor. *e*, the abductor. *f*, the obliquus superior, or trochlearis. *g*, the obliquus inferior.

Fig. 138.

Fig. 138. shews a view of both eyes, with their muscles disposed nearly as in the life.

a, the optic nerve. *b*, the *musculus attollens*. *c*, the depressor. *d*, the adductor. *e*, the abductor. *f*, the trochlearis. *g*, the obliquus inferior.

Fig. 139.

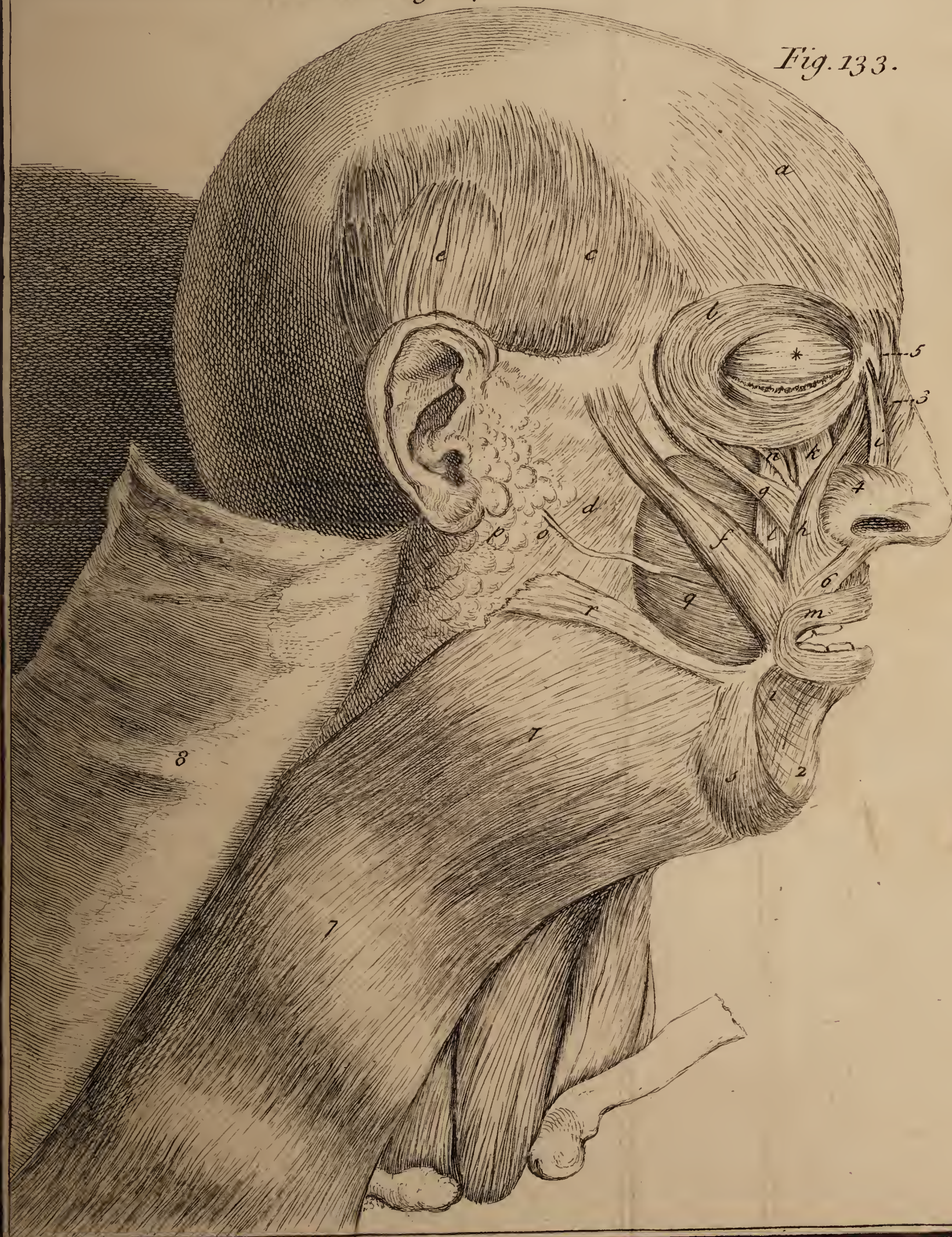
Fig. 139. is a view of a countenance in an easy composure, wherein no muscle can be said to have any particular action, and wherein every muscle in it's respective place only conspires to form the pleasing symmetry and proportion that appears thro' the whole. An appearance like this on the face must inform every spectator, that, although the mind of the person who wears it, does not seem to be exalted into mirth, or veneration, yet it must at the same time shew, that neither is it debased by envy, malice, jealousy, or a tyrannic spirit; nor degraded into desponding grief and care, the true characteristic of those who are not happy enough to rely on the AUTHOR OF PROVIDENCE for His Protection; nor filled with fear and terror, either at external dangers, or the more excruciating attendance of the guilt of having injured others; nor disfigured by the deforming grins or sneers, which are the dictates of pride, haughtiness, ignorance, and scorn; nor, in fine, chained by a narrow stricture of soul, which would circumscribe the benevolence of God towards mankind, deny happiness to fellow-creatures, and monopolize it all to it's own despicable self; whereas such a face is undoubtedly more ready to alter into cheerfulness and complacency, and to demonstrate to the world that benevolent compassion, which is the natural bent of the generous mind that owns it.

Fig. 140.

Fig. 140. represents a face whereon *veneration* or *reverence* is apparent, notwithstanding the acting muscles are very few, which obey the dictates of the mind in this case; and which may be increased to the most profound veneration by a little augmenting their actions. The *aperiens palpebram* draws up the upper lid of each eye, at the same time that the *elevator*, by it's contraction, turns up the *pupil*; for in this case they act in perfect harmony; and these actions are but moderate, yet are sufficient to shew the meaning of the mind, in the several passions we shall bring under this head. The mouth also is but moderately opened; not by the action of the muscles that serve to pull down the lower jaw (because when they act they open the mouth pretty wide), but by the remission of the action of those muscles which serve to pull it up; for this maxim is necessarily true, that, between the actions of any muscle and it's antagonist, there must be a remission of the former; so that, as in the case before us, the remission is sometimes sufficient, without any necessity for the subsequent action of the antagonist. The same is observed before, where we mention the *Occipito-frontalis* and the *Corrugator Coiteri*.

But,

Fig. 133.



J. P. MD. del.

Fig. 135.

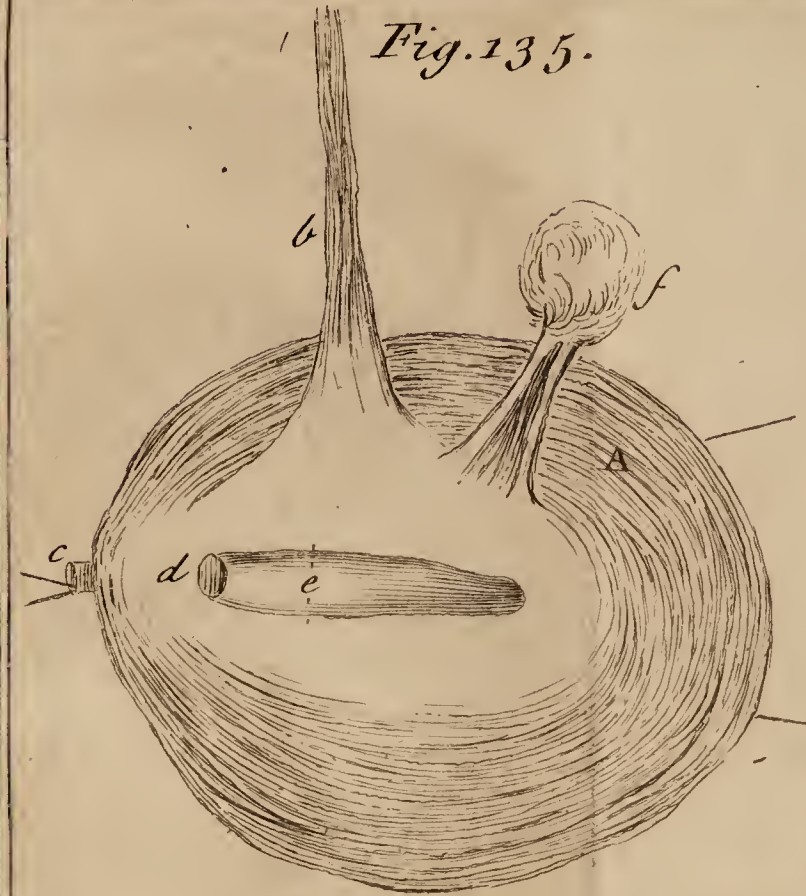


Fig. 136.

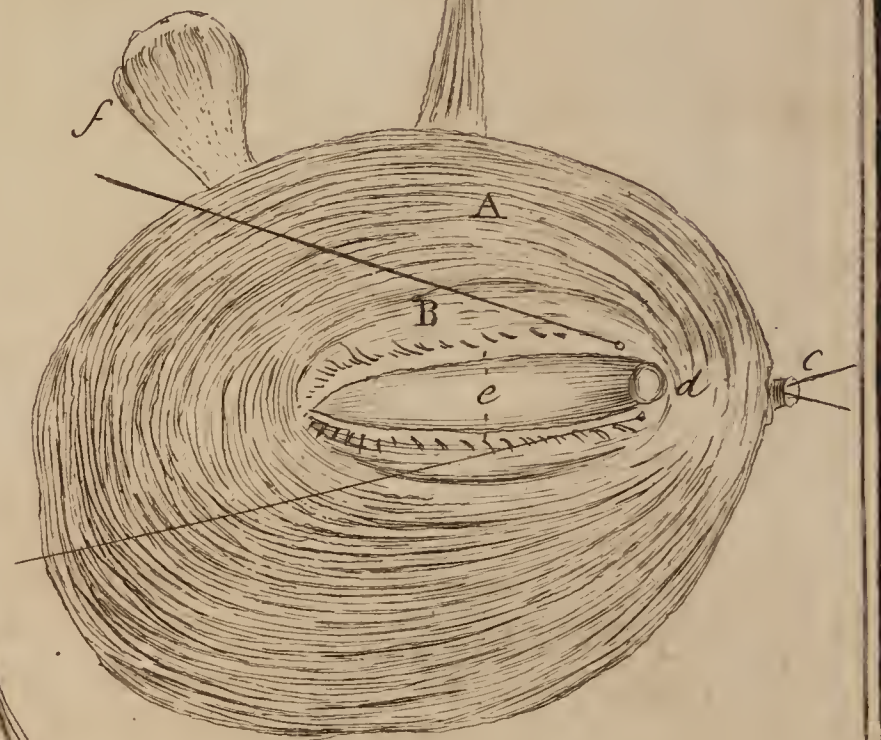
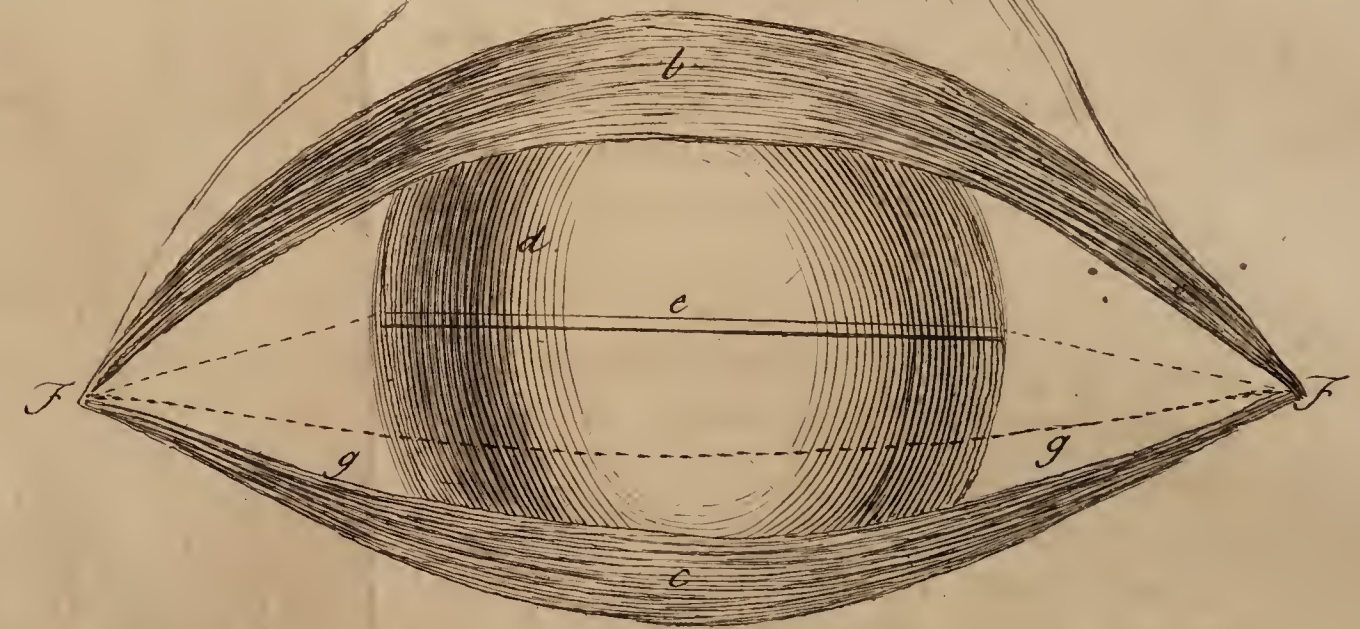


Fig. 134.

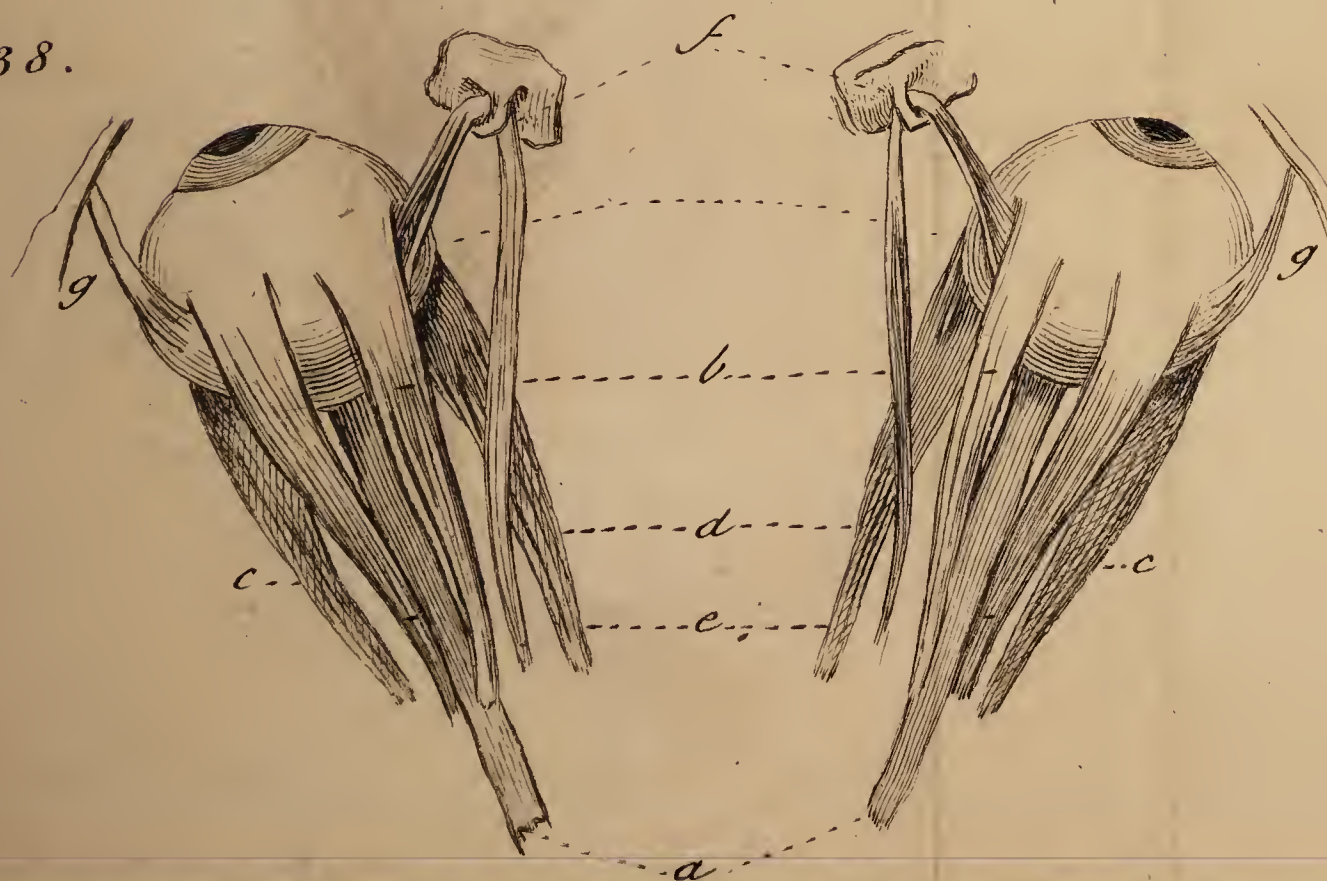


J. Mynde sc.

Fig. 137.



Fig. 138.



I. P. MD. del.

Fig. 139.



J. Mynde sc.

But, in order to render this remission of the *temporalis* and *masseter* (which are the muscles serving to draw up the jaw) more easily understood, let us only observe a person sleeping in a chair, with his head leaning directly backward, or upon his back in bed, at which time the mind is quite undetermined to any design; we shall see the lower jaw fall by it's own gravity; which cannot happen but by the remission alone of those muscles mentioned; because the *digastricus*, *pterygoideus externus*, and *pterygoideus internus*, which serve to open the jaw, are as much at rest as the former. This any one may prove, by leaning his head back, and letting his jaw fall spontaneously as far as it will go; and then, by bringing those muscles just mentioned into action, he will be able to pull it down yet lower. But if a man's head leans forward on his breast in a chair, or if in bed his bolster and pillow be high while he sleeps, then the pressure of his chin upon his breast keeps his mouth shut.

The passions of the mind that are generally expressed by the actions and remissions of the muscles mentioned, with but very little variation, are,

1. *Esteem* for any object. If the object be of divine, or otherwise of a superior nature, the present system of the face serves to express the esteem with a graceful humility, such as is becoming in adoring the Divine Being; or in professing a grateful respect for superiors, who have conferred any favours. And if this countenance be accompanied with other actions of the body that are generally concomitant with it, as the head bowed or inclined forwards; the hands spread forwards, or lifted up; the knees bent, and the body inclined forwards; then the appearance of the mind's dispositions is yet more conspicuous. If the object of esteem be extended to any friend, who can claim no superiority, or be any matter of curiosity, then it is sufficiently expressed by this settled countenance, only bringing the *pupils* of the eyes to view the object, having the lids but moderately opened, instead of being elevated, whether the person sits or stands.

2. *Love* is a passion which is a natural follower of esteem, according to the different circumstances that attend it, and can only be conceived for persons; the expression of simple friendship is just accounted for, and also the regard for objects of curiosity. This passion then, as it is a little more earnest, requires some small alteration in it's expression upon the countenance. Thus, if a lover is making an address in conversation, or if there be a reciprocal affection in the person to whom the address is made, it will be expressed by this system of the face, with this difference, that in the man, the head inclined downwards, and to one side, will cause the eyes to appear more languishing; and in the woman, the lids but moderately open, and the *pupils* rather carried a little downwards, will be very expressive; for in the man, this love amounts to veneration; in the woman, to an esteem, adorned with a modest deportment.

If it be attended with *desire*, the same aspect will serve to express it, with the addition only of a more florid and vivid colour of the lips and cheeks, and the *pupils* of the eyes directed to the object of love.

But if the passion of desire be prompted and accompanied by any more engaging circumstances, then the *elevator* of the eye will act strongly, causing the *pupil* to turn up, at the same time that the action of the *aperiens palpebram* is more remitted, whereby all the *pupil*, except a little of the lower edge, will be hid, and the lids come nearer each other; the mouth being a little more open, the end of the tongue will lie carelessly to the edge of the teeth, and the colour of the lips and cheeks be increased.

Hope has an undoubted relation to every passion we have now mentioned, and therefore cannot be better imprinted on the countenance than by this very system; these are the muscles which act, and are remitted, to express it in obedience to the mind, for, as it consists only of a seeming likelihood or expectation of obtaining what we earnestly desire, love, esteem, or venerate; and as it must be something pleasing and agreeable we hope for; the countenance must be the same as in this figure, with this difference, that the corners of the mouth must be drawn a very little more backwards, and turned upwards, which increases the appearance of the satisfaction upon the countenance that demonstrates such an expectation; for we may love or desire an object, that we may never have the least hope of obtaining; which is the cause of this difference in the aspect. The harmony of this countenance shews also, that it partakes of all the amiable qualities mentioned in our explanation of the last figure, which is capable of being altered only into the class of these noble passions of the mind of it's owner, that this figure represents, whereby it is rendered yet more lovely.

Fig. 141.

Fig. 141. demonstrates a face full of fear and terror.

1. Here the *occipito-frontalis* drags up the eye-brows, and wrinkles the forehead transversely.
2. The *aperiens palpebram* on each side pulls open the lids with violence a great way above the *pupils*, which are as it were suspended below the *equator*, by the remission of the *elevators*.
3. The *digastricus* and *latissimus colli* pull down the lower jaw.

The reason why the eyes and mouth are suddenly opened in frights, seems to be, that the object of danger may be the better perceived and avoided; as if nature intended to lay open all the inlets to the senses for the safety of the animal; the eyes, that they may see their danger; and the mouth, which is in this case an assistant to the ears, that they may hear it. This may perhaps surprize some, that the mouth should be necessary to hear by; but it is a common thing, to see men, whose hearing is not very good, open their mouths with attention when they listen, and it is some help to them: the reason is, that there is a passage from the *meatus auditorius*, which opens into the mouth. Thus

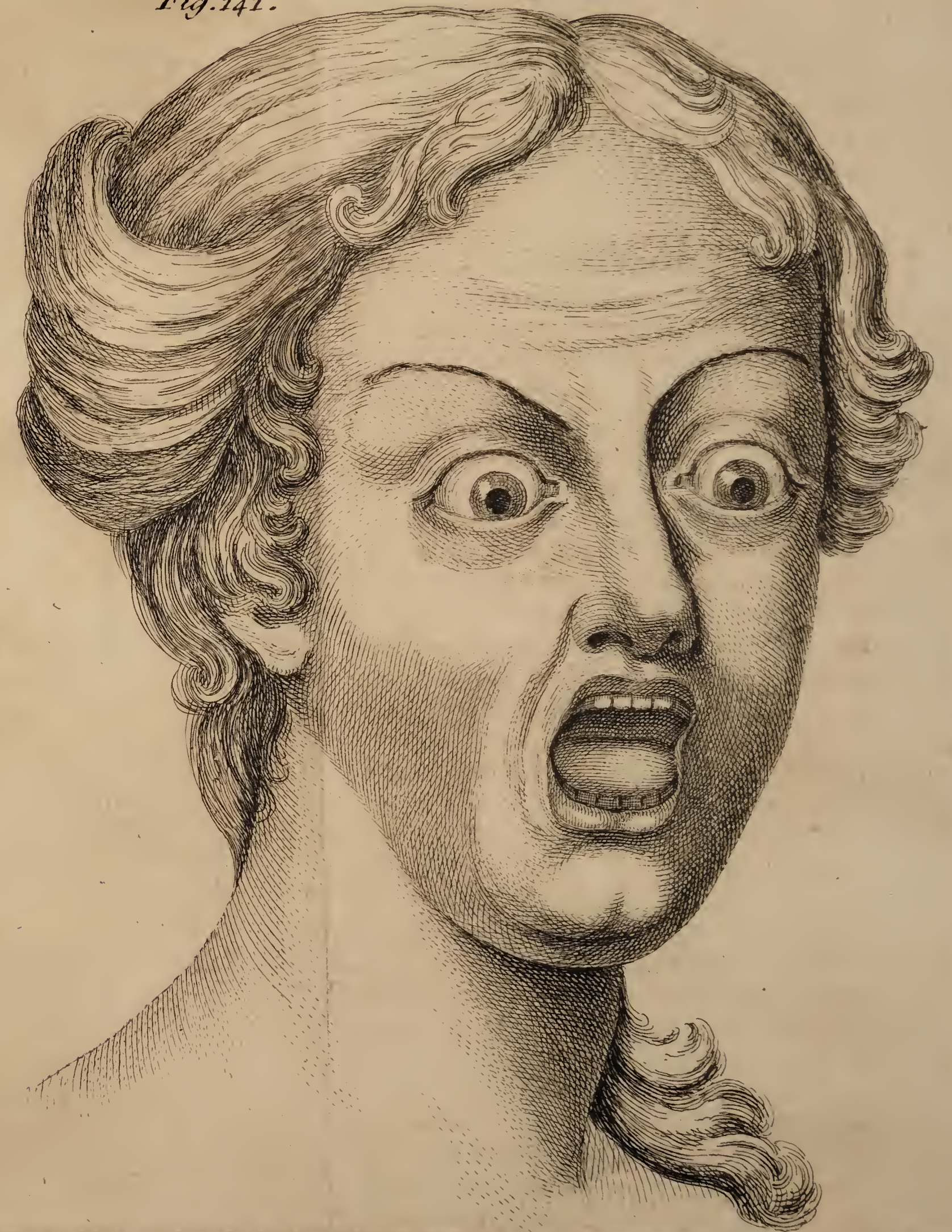
we

Fig. 140.



J. P. M. del.

Fig. 141.



J. Mynde sc.

1817



we see how ready nature is, upon any emergency, to lay hold of every occasion for self-preservation.

But when a person is frightened, so as not to be under an instantaneous apprehension of danger, by being pursued, or the like; then the upper part of the face will be as in this figure, and the lower somewhat different from what it appears here; that is, the mouth will open moderately, by the remission of the actions of the *temporal* and *masseter* muscles, having the corners, or *riētus oris*, inclined a little downwards.

It has been imagined, that the eye-brows might be moved in parts; that is, that one part of a brow may be pulled up, while the other is pulled down: but this cannot be, for the *occipito-frontalis*, which pulls up the brow, acts all at once on either side; so that the intire brow must be pulled up at once, or, by it's remission, let down at once. I never yet saw any one, who was able to give them this partial motion, and there are but few who can raise either without the other at the same time; so that this opinion amounts to something nature never intended, and is an exaggeration which renders any figures preposterous.

Fear, *horror*, and *terror*, are but terms which signify the same passion, only in different degrees; and are all expressed by this figure, only rendering the change of features, by the motion of the muscles, greater or less; which, if accompanied with certain actions of the body, would express them more absolutely. As for example; if on the ground, under an enemy resolved on the destruction of the frightened person, with hands lifted up, and fingers stretched far asunder, dreading the fatal blow; or, if flying from danger, with the hands pushed forward, and looking back at the object that affrights; which kind of fear may be excited by imagination, as well as by real objects.

If, in this state of fear, the countenance inclines to a livid colour, it is no great wonder, since the whole mass of blood, after the first surprize, grows languid; for, being pushed with great force to the extremities, it suddenly loses that *momentum*, and, as it were, stagnates in the minute capillaries on the surface, immediately after; and then the blood is, as *Virgil* has it,

—— *sociis subita gelidus formidine sanguis*
Diriguit : cecidere animi ——

Except in flight, where the exercise ought to increase the *momentum* continually, and heat the mass; then, indeed, the countenance is florid, notwithstanding the terror. In this circumstance of flight there is another reason why the lower jaw is pulled down, in order to open the mouth; and that is, since every assistance is necessary for the relief of the persons frightened, that they may the more freely exhibit their voices in shrieking and crying out, in order to alarm and summon all within the call to their rescue; who, without it, might know nothing of the distress, tho' near enough to give timely aid.

To

To this class may be added *despair*, and the same countenance will serve to express it, with very little alteration : for, as the first advance to that passion is the fear, terror, or dread of any danger, and is thus expressed, so the deprivation of any hope for relief, and the perpetual dread of meeting the fatal shock, is *despair* ; and therefore, by adding paleness, and a livid aspect, to this countenance, it will be well represented ; because, when all hopes are given over, the blood grows languid in every part, notwithstanding the same fear and terror remains, from the person's ceasing to make any defence ; and at length degenerates into a melancholy madness : so that flying, or sitting, or lying prostrate on the ground, may be suitable attitudes to this passion ; and then the countenance will be changed, as in the passion of grief.

I cannot but think it a wrong application of the passion of *despair*, to represent it with staring hair, corrugated face, the brows drawn down even with the eyes, and the mouth open, with a weeping aspect ; for, instead of such violent contractions, which are signs rather of bodily pain and torment, there is in deep *despair* a cessation from those muscular actions in the face ; and the first actions of fright are rather remitted, as it were, into a relaxation of those muscles, and a falling of the countenance ; because all exertion towards a defence, as I have just said, is given up, and a desponding and fainting are often the consequences.

Fig. 142.

Fig. 142. shews a countenance of scorn and derision, which is formed by the following muscles.

As soon as the mind suggests a contempt for persons or things, whether deservedly or not, the first muscles that begin to act are, the *elevator labii superioris proprius Cowperi*, and the *pyramidalis*, on one side only ; whereby that side of the upper lip is pulled up, so as to shew the teeth, the other side only inclining a little to grin ; and at the same time, the *slip*, which is an assistant to the *pyramidalis*, called, in the explanation of the first plate, the *pyramidalis socius* of Santorini, wrinkles the skin of the nose by it's contraction.

On the same side the *risorius* draws back the corner of the mouth ; and the action of the *aperiens palpebram* is remitted, whilst that on the other side is in it's action ; so that, tho' one eye is moderately open, and the other almost shut, the *pupils* are carried obliquely downwards, by the *abductor* of the one, and the *adductor* and *obliquus inferior* of the other, looking downwards and backwards at the object of contempt. Sometimes (especially in winking at a stander-by, who joins in the contempt) the *orbicularis*, by it's contraction, gathers up the skin of that side of the face over the *os mali*, and causes it to wrinkle under the eye, at the same time that the *occludens palpebram* shuts the eye intirely, or very near it.

There are several gestures of the body, which consent to, and favour, this villainous, ungenerous passion ; as, looking back at the object, with a toss of the head, and a shrug of the shoulders with this countenance,

nance, upon being asked an opinion of an absent person ; which is as keen an arrow, and stabs as deep, as even the slander of a base tongue ; and sometimes it is attended with a grinning laugh, which can have no real meaning, because there is no real cause for it ; and the hypocrisy of the mirth is easily distinguished upon the face.

This is that kind of aspect that one would imagine, according to *Martial*, was much in vogue in his time at *Rome* :

*Nescis, heu ! nescis, dominæ fastidia Romæ ;
Crede mihi, nimium Martia turba sapit.
Majores nusquam ronchi ; juvenesque, senesque,
Et pueri nasum rhinocerotis habent.*

Epigr. 4. l. 1.

The source of this countenance is *pride* ; and it's train of attendants are, *arrogance*, *derision*, *haughtiness*, and *scorn* ; for every one of which this aspect may serve as the representation : and it is remarkable, that, wherever these passions reign, there is no room for any thing valuable : for, as *Pope* says,

*Whatever nature has in worth deny'd,
She gives in large recruits of needful pride.*

Nor is there among all the passions of the mind one more injudicious and erroneous in it's application than pride ; for what is more common, than to see *arrogance* raise it's crest, where a decent *humility* ought to be practised ? What more ridiculous than *derision* and *scorn*, where *esteem* and *compassion* should be ? And what more base than *haughtiness* and *tyranny*, instead of *generous pity* and *mercy*, for those whom we have in our power ?

There are other actions of the body and extremities that accompany this countenance, as recoiling backwards from, and extending and opposing the hands to the thing one disdains, in order to keep it at a distance.

Fig. 143. is the representation of a morose envious countenance. *Fig. 143.* The action is frowning with a malicious grin ; and the muscles, which serve to form this aspect, are, the *corrugator Coiteri*, which strongly forces the brows downwards and inwards, at the same time that the *aperiens palpebram* drags up the upper lids of both eyes, and the *depressor* of each eye inclines the *pupils* downwards : the mouth also, which in this passion has a great share, is subject to the actions of several muscles. The *pyramidalis*, and *elevator labii superioris proprius* of *Cowper*, draw up the upper lip on each side, while the *triangulares* draw the corners of the mouth somewhat downwards ; and the *elevator labii inferioris proprius* of *Cowper* pulls the middle of

of the under lip upwards in the pouting way, raising with it the skin of the chin.

These are the commanding characters, and these the muscles, which are subservient to a turbulent mind.

The passions which are represented by this countenance are, *envy*, *malevolence*, *suspicion*, *fury*, and *jealousy*; and all these have a near relation to, and naturally follow, that of *pride*; for, to be proud, is to fancy one's self possess'd of something that others have not, or ought not, in our opinions, to have: and as there is included in this an opinion of superior self-merit, to see another possess what we have not, or be blessed with a good name, which, perhaps, we are not intitled to, is to kindle up an unextinguishable anger and hatred in such a breast; to cause the eyes to roll, the brows to knit together, the mouth to grin and pout, and to give free possession of the uneasy mind to those cursed plagues *envy* and *malice*; which reverse the nature of the soul, and change it into something lower than brutality: then arises a promptitude to evil, gladly to laugh at the misfortunes and distresses of the wretched, and to help forward the destruction of a neighbour.

The *suspicious* and *jealous* man may be represented also by this described countenance, or upon the same principles; for imaginary griefs of this kind, as well as real ones, produce a depravation of temper: the gay humour grows muddy, when this passion begins, and the struggle between love and resentment produces a conflict, which terminates in a resolution upon revenge, and which is accompanied with this sort of countenance.

This happens, when there is already a possession and pre-engagement of the object of love; but where the affection is only growing for one not yet possessed or engaged, and the lover sees another caressed, with but the slightest favour of simple friendship, whom he mistrusts is a rival, the jealousy arising in such a breast is not the same with that just mentioned; because there can be no cause for resentment nor revenge, since there is yet no possession or property in the object; and therefore that kind of jealousy ought to be expressed by a countenance of sorrow and dejection, the principles of which we shall endeavour to explain in the next table; and which must appear yet more obvious, by having regard to that inimitable picture of a jealous lover in *Sappho*, translated by Mr *A. Phillips*.

*Blest as th' immortal gods is he,
The youth who fondly sits by thee;
And hears and sees thee all the while,
Softly speak, and sweetly smile.*

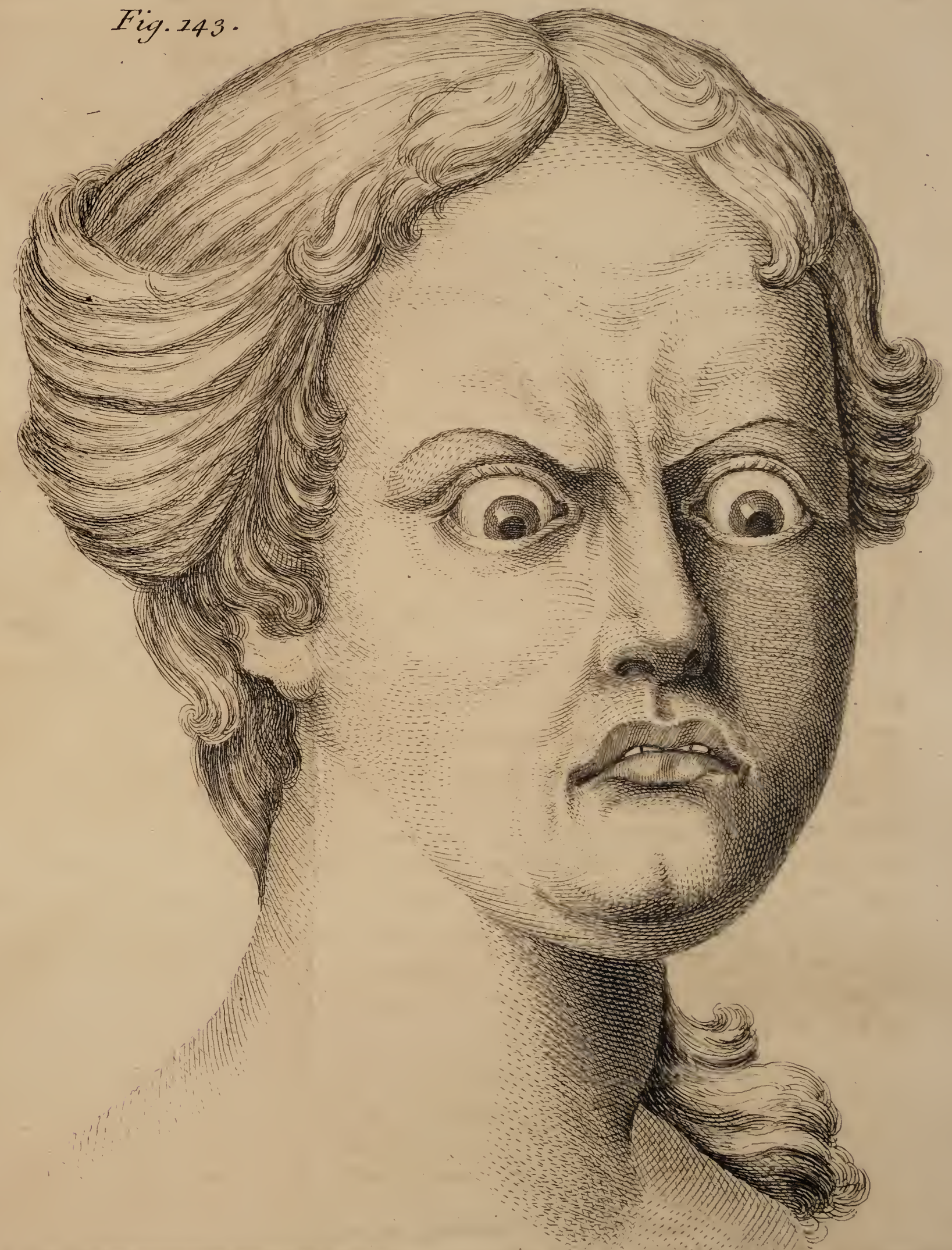
*'Twas that depriv'd my soul of rest,
And rais'd such tumults in my breast;
For while I gaz'd, in transports tost,
My breath was gone, my voice was lost.*

Fig. 142.



I. P. MD del.

Fig. 143.



J. Mynde sc.

*My bosom glow'd, the subtil flame
Ran quick thro' all my vital frame ;
O'er my dim eyes a darkness hung,
My ears with hollow murmurs rung.*

*In dewy damps my limbs were chill'd,
My blood with gentle horrors thrill'd ;
My feeble pulse forgot to play,
I fainted, sunk, and dy'd away.*

To which dejected countenance paleness added, heightens the picture ; and if in a reclining posture, or falling down and sinking amidst the arms of several assistants, it would yet be rendered more expressive.

When *revenge* is suggested in the mind, it circumstantially increases to *fury* and *rage*, which must be represented by this plan upon the face, heightened by adding fire to the eyes already starting forth, and an unequal colour to the whole countenance ; that is, some parts flushed with red, and others livid and pale ; because the muscles that act to form the features necessary in this passion, are now so violently contracted, as to squeeze forth the blood from the vessels proper to them, and the adjacent parts ; whereas the parts contiguous to the muscles, that do not act, remain florid, by the general emotion occasioned in the whole mass, having no such resistance attending them.

These, with the addition of suitable gestures of the body, such as combatants use in assailing one another, or a swift pursuit with an uplifted arm, holding some direful instrument of destruction ready to execute revenge, will render the representation yet more lively :

*His agitur furiis, totoque ardentis ab ore
Scintillæ abstunt : oculis micat acribus ignis.*

VIRG. Æn. XII.

*Ora tument ira ; nigrescunt sanguine venæ :
Lumina gorgonio sævius angue micant.*

OVID. de art. Am. l. III.

Fig. 144. represents a cheerful countenance.

Fig. 144.

The action is *laughter* ; and the muscles that are now brought into use are,

1. The *risorius* on each side ; and, because it is gone beyond a smile, the
2. *Buccinatores* have carried the corners of the mouth farther back, and the skin of the face, over the *os mali*, is pressed up by the contraction of the *orbicularis*, together with the under lid, at the same time that the ciliary fibres of the upper are a little contracted ; for, in laughing, the eyes are more or less shut ; and if it is increased to a vehement

- ment laughter, the *buccinatores* are more forcibly contracted, the skin of the face driven higher up, by the contraction of the *orbicularis*, and the eyes in many people quite shut up during that action.
3. Here also the *zygomaticus major* assists, in drawing the corners of the mouth upwards. There are many who open their mouths very wide in laughing and then the
 4. *Digastric* muscle, and the *latissimus colli* act in pulling down the lower jaw; and in some persons who laugh heartily, the muscle called the *socius pyramidalis* is drawn into consent, and, by it's contraction wrinkles the skin of the nose.

From this explanation it may be inferred, that the degree, of joy on the countenance, may be represented by rendering the action of these muscles more or less strong; for, supposing the mind was happy and glad upon any occasion, where immediate laughter did not seem necessary, a bare disposition to smile, upon the mouth, with the eyes a little more, but not quite, open, would sufficiently denote it. In the figure before us, the mirth appears to be greater, and a moderate laughter influences the whole, which seems to be real, from the general consent between the mouth and eyes; but there is a great deal of difference between this and a fictitious mirth put on to serve an occasion, when the mind consents not to it.

This is very conspicuous in some *players* (acting their parts in comedies which require much laughter), who do not always enter well into it, for want of the mind's influence; and then the laughter put on appears dry and unmeaning; and although the mouth puts on a smile, it seems forced, because the muscles of the eyes are not in consent; for, as the mind is employed in recollecting the part he is to speak, it's natural suggestions have not been applied to that passion; and, consequently, are not free enough to command laughter. But if he is perfect in his part, having nothing to study while he acts, he is then capable of taking to himself a share in the mirth, and of laughing in earnest.

Another case wherein laughter is unmeaning, is, when a person, dressed with all the marks of adulation, feigns a laugh, to favour that of a superior, and feed his vanity. Here, too, the other muscles of the face give the lips the lye, and prove the hypocrisy; for it wants their consent, as in the case just mentioned.

There is another miserable circumstance which makes laughter appear dreadful, and that is when it is convulsive: in this case, the nerves which communicate the consent or sympathy between the *diaphragm* and the muscles of laughter, are affected, and the cachinations, occasioned by the convulsive twitches of the former, draw the latter into consent to form that kind of unnatural and involuntary laughter, which is called by authors the *risus sardonius* *. It is a melancholy sight, when

* This appellation comes from the name of a plant, by some called *herba Sardonis*, or *Sardinia*, by others *apium agreste*, or *apium risus*; which is said to produce this kind of

when a person fallen to the ground shall appear all over convulsed and distorted, laughing at the same time that misery and distress appear upon the whole countenance: sometimes it is followed by immoderate weeping, from the same correspondence with the muscles of that passion in the face; which, though convulsive, is yet less shocking; as it seems, at least, more suitable to the miseries of the afflicted poor creature.

An unfortunate person sometimes feigns a laugh to conceal his distress; but it is easy to discern that the heart is not the dictator of that seeming mirth, by observing the want of the sympathy of other parts of the countenance.

When laughing is vehement, it is a common thing to see tears flow from the eyes, before the person who laughs has quite given over; because, by the violence and continuation, for some time, of the motion of the diaphragm, the blood is pushed with great force to the face, and, consequently, to the *lacrymal glands*; where, by that means, joined to the pressure made by the strong contraction of the *orbicular* muscles, the tears are squeezed out of the glands in abundance; and when once they are begun, will continue to trickle down, till the laugh ceases, and the diaphragm and muscles of laughter in the face are at rest.

The appearance of this countenance, with the body in an attitude suitable to the occasion, would make the representation still the better; as, sitting in a conversation either with the head lifted up on one side, or standing with the body leaning backward, where the object of mirth is present: and there is another frequent action that attends vehement laughter, which is, laying the hands on each side to the ribs holding the sides, and is caused thus: the upper part of the diaphragm is attached to the end of the *cartilago ensiformis*, and to the cartilages of the seventh ribs, and to the edges of the cartilages of all the lower ribs, and, in fine, to the bony part of the last; and the lower part to the *vertebræ* of the loins. From hence it happens, that when laughter is violent, and continued too long, the constant vellication of all those parts fatigues the person, and causes pain in the regions of the parts mentioned; that is, in the loins, breast, and sides: and this is the reason why we are so apt, in violent laughter, to hold our sides, breast, or back.

*Sport, that wrinkled care derides,
And laughter, holding both his sides.*

MILTON, *il Allegro*.

Here it will not be unseasonable to mention a circumstance, the bad effects of which I saw more than once, which regards laughing. A per-

of convulsion; and being said to grow in great plenty in *Sardinia*, thence came the name *herba Sardonica*, and thence this name *risus sardonicus*.

This laughing without cause, or *risus sardonicus*, had another source with some authors: it was said, the *Sardinians* used to sacrifice their prisoners to *Saturn*, who laughed, to shew their bravery and fortitude at their deaths: also, that the *Sardinians* laugh, when they intend any evil to another.

son playing with a child tickled him in the sides very much; and seeing him at first pleased with it, continued it ignorantly, till the poor child grew black in the face, was convulsed all over, and had respiration so impeded by it, that he was short-breathed as long as he lived, and had a fixed pain under his right breast for several years. It was remarkable, that after a little time, when he grew tired with laughing in earnest, he still continued a noise like laughing, when his face grew livid, without a due consent of the muscles, and it became the *risus sardonius*. Another fine child grew ill, from being provoked to laugh inordinately in the same manner, and dwindled and wasted away to skin and bones in less than a year.

To laugh in due season is agreeable and pleasing to society; but to prostitute this lovely passion to trifles, or be inclined to laugh others to scorn frequently, is sufficient to create in the minds of the prudent part of the world, a suspicion of weak understanding, or much self-conceit.

Fig. 145

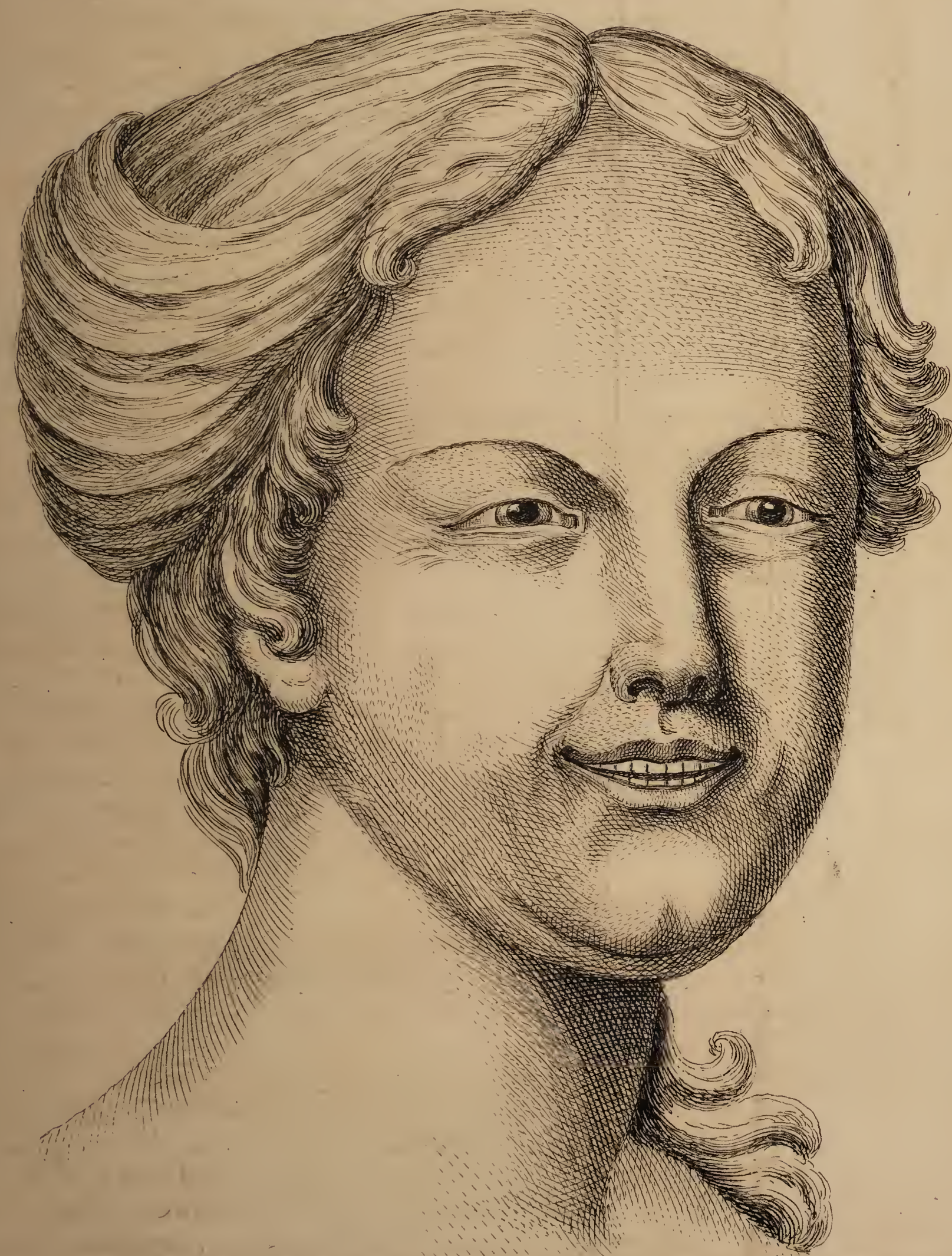
Fig. 145. shews a countenance of *sorrow*, whose action is weeping. In this passion the muscles that are the instruments for it's formation are: The *triangularis* on each side, which draw down the corners of the mouth, while the *elevator labii inferioris proprius* pulls up the under lip. At the same time the eyes have a principal part to act in this case; for the *aperiens palpebram* is remitted; while the *elevator* of the eye has a little raised the *pupil*, which is covered by the lid, and looks languid; the other muscles of the face are relaxed, and the distance between the *riētus oris* and the eyes is much lengthened. This gives occasion for the phrase commonly used, of *putting on a long face*, upon being sorrowful; and the relaxation mentioned causes the forlorn look.

Immoderate grief, by keeping those muscles relaxed for a series of time, brings on that haggard gloomy look, which no change of mind afterwards can alter, and round whose doleful aspect care sits brooding.

Grief may be real or imaginary; and, in either case, the aspect is sufficiently represented in this figure; but it may be also feigned; there will be, in that case, as little sympathy between the mouth and eyes as we shewed to be in the countenance of mirth, where it is insincere; and therefore, tho' the mouth, by the direction of it's corners downwards, may serve to represent that passion, yet the eyes cannot be influenced by any means but real grief, to shed tears, or appear languid; and therefore, if an hypocritical sorrow was to be represented, it could not be better done, than by making the eyes appear gay, or inclining to the laughing way, and the mouth at the same time on the weeping mood; the rest of the face being well-proportioned. When grief is real, and founded upon just reason, the pitying part of mankind are so moved at it, that one may venture to call it the touchstone of the generous breast; and this amounts to that most divine gift *compassion*, which is the root of many virtues, which comforts the afflicted, and eases them of great part of their distresses; and that pity, which arises at the tears of afflicted

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Fig. 144.



I. P. MD. del.

Fig. 145.



J. Mynde sc.

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ed beauty, often grows into the most lasting and generous friendship.

Tears are often too forced from their recess by nature, to express an overflowing joy ; but then it is accidental or constitutional, and not a true character of joy. The first efforts of the mind to rejoice, if moderate and deliberate, cause the *momentum* of the blood to increase without violence, and then there is no interruption to that pleasing transition of the mind's disposition to the muscles of the face, nor of their natural conformity to it ; but when the joy is received too suddenly, it amounts to a shock, whereby the blood is driven, at the first onset, with greater violence to the extremities, and in greater quantity, than can be time enough brought back to the heart ; whence it remains too long contracted before the *vena cava* can supply blood enough to force the *auricles* and *ventricles* open again, whilst in the mean time the head is loaded with too great a quantity, and the person in danger of an *apoplexy*, or sudden death. In such a surprize as this mentioned, *tears* have another noble use ; for, by flowing plentifully thro' the lacrymal passages, the lives of many have been saved ; for they lessen the bulk of humours, and gain time for the more happy return of the mass to it's natural circulation, and, at length, draw the muscles mentioned into consent, whereby this sudden joy terminates in real weeping. Thus, in vehement grief too, shedding of *tears* in plenty is known to ease the anxiety usually attending it.

XII. The external superficies of every muscle, it's origin and insertion, the use of it's action in regard to the animal oeconomy, from what *plexus* the nerve comes, whence the artery which supplies it arises, and to what vein the blood is carried, are disquisitions very worthy the knowledge of every *Physician* and *Surgeon* ; in order to discover the true seats and causes of many complaints, and to point out the most rational methods of cure. But as these researches have nothing to do with the immediate cause of muscular motion (the explication of which seems to be the chief design of the learned *founder* of these lectures). I shall wave all such enquiries, and proceed directly to investigate by what means a muscle so instantly and forcibly contracts itself, or shortens it's length, at the command of the will ; and this I shall endeavour to do by such rules and laws as are simple, uniform, and intirely agreeable to the known system of the universe.

The Croonian Lectures on muscular motion ; by B. owne-Langsh, M. D. F R S Supp. to the year 1747. Read at a meeting of the Royal Society, on March 5, 1746-7 LECTURE I.

In order to the easier illustration of this wonderful and important property in the fibres, I shall give you, 1. An analytical view of the component parts of a muscle. 2. I shall shew the true cause of cohesion, tension, and elasticity in the animal fibres. 3. I shall make it appear, that every fibre constituting a muscle, is, in it's ultimate division, tubular, and not a chain of vesicles or bladders. And *lastly*, I shall prove it to be more than probable, that muscular motion proceeds from the *attractive* quality of the constituent particles of every fibre.

fibre being increased and strengthened by the addition of some *ethereal matter* flying out from the extremities of the nerves; and that this is instantly occasioned by the will, and ceases again as soon.

By a chemical *analysis* of a muscle dissected from the buttock of a lean ox, which weighed exactly lb ij *Averdupois*, I procured,

| | Oz. | Dr. | Gr. |
|--|-------|------|-----|
| 1. Lymph — — — — — | xxiii | xiii | xv |
| 2. Volatile salt — — — — — | i | iiii | xxv |
| 3. Oil — — — — — | ii | vi | xx |
| 4. <i>Caput mortuum</i> — — — — — | ii | ix | v |
| 5. Lost in distillation, which I presume was } mostly mere air. — — — — — } | — | x ii | xvi |
| | 32 | co | oo |

There being no *Averdupois* weights in the shops, less than quarters of ounces, I ordered some to be made of a drachm, and others of two drachms. The drachm weighed 27 grains; so that, by casting up the grains into drachms, and the drachms into ounces, we have the exact weight of each of the above constituent principles according to the weight they were first of all weighed with.

It may be proper also to observe, that the *apparatus* I made use of in this process, was the same which I communicated a description of to the *Society* some time ago, * except that the retort I now used was made of copper, in order that I might remove it from the sand-heat into the actual fire, without unluting any part of the *apparatus*, when no more matter would arise by means of the sand-heat. By this method I could increase the fire till the bottom of the retort was red-hot, without any danger of breaking my recipients; a contrivance which may be useful in many chemical processes.

From the above-mentioned experiment we have evident proof of the proportions and qualities of the several principles, or constitutive parts of the muscular fibres; and let no one be surprised that the watery or phlegmatic principle abounds so much, as to be nearly $\frac{1}{6}$ parts of the whole mass, since we know that dried bones, and many other things as unpromising, afford half their weight of water.

That the particles of water are endued with a strongly attracting power, and are highly serviceable as a band of union in the formation and growth of every thing, animate and inanimate, is not only manifest from

from the great quantity employed in the growth of animals and vegetables, but also in our own manual operations, such as making several sorts of glue, pastes, bricks, and such like, where the watery particles prove a very durable and powerful *copula*, and are not to be all of them separated again, even by a very intense fire: water is to be regenerated from bricks and tiles after they have been burnt in the kiln. The driest wood, part of a mahogany-table, which had stood by the fire many years, being rasped and put over the fire in a copper retort, afforded a considerable quantity of water. In short, take away the water from the most solid animal and vegetable bodies, and they immediately become mere dust.

I don't apprehend that this considerable quantity of water, which is to be procured from such solid substances as bricks, wood, or even from the muscular fibres, remains in distinct drops or spherules, whilst it is a part of such solid substances; but I conceive that the *minima*, or primary solid particles of water may be attracted by, and actually joined with, the earthy, saline, and other component particles, so as to compose the several degrees of hardness, according to their respective proportions and qualities; and when these several constituent principles are disunited again, by the power of fire or by the length of time, they rise up into the air, or into the recipient, according to their divisibility and levity; first water, next salt, then oil.

Nature seems to delight in transmutations. Many kinds of fluids are easily converted into dense bodies. We all know how soon water is capable of being turned into a very solid friable stone, by the power of cold. Mercury also is easily turned into a hard brittle metal; and both these return to their former state of fluidity by means of heat. And a solution of copper in spirit of nitre being poured on oil of tartar, both liquids instantly become verdigrease in a dry powder.

From what has been said we may observe, that water, or the watery particles, not only make up much the greatest part of the muscular fibres, but by mutually attracting, and being attracted by the other component particles, they greatly contribute towards their cohesion and elasticity; for a fluid particle will be fixed, and become a part of a solid as soon as there is an attractive force sufficient to effect it's cohesion with the other solid parts, though it returns to it's former state of fluidity upon the analysis of the compounded body.

I would not by this be understood as if I designed to exclude the other principles from their share which they give to the true degree of firmness and elasticity in the fibres; the saline, sulphureous, and earthy parts are all endued with a strongly attracting power; and when brought into contact with each other, or with the watry and aëreal particles, they give firmness and solidity to the composition.

Water seems to be the *gluten* by which the other principles are wrought up. Too much water in the composition renders the fibres soft and lax;

as

as in children, and anasarcaous people. Too little moisture occasions a stiff, rigid fibre ; as in old age. There is a certain degree of texture and cohesion necessary towards muscular motion in it's greatest strength.

I have shewn in a former treatise *, that air is very instrumental in fixing and uniting the other principles which constitute an animal fibre ; for in the most solid parts of the body, where the cohesion is strongest, we find great plenty of air. That the air-particles are capable of being united, and fixed to solid bodies, and by that means may be esteemed a part of their composition, we have many evident proofs in Dr *Hales's Analysis of the air* ; and that those particles do in their fixed state strongly attract the other component particles, is evident, it being well known, that the most strongly repelling and elastic particles, when in a separate state, are the most strongly attracting, when fixed to other bodies.

Now, according to Dr *Hales*, since a much greater proportion of air is found in the solid than in the fluid parts of bodies ; may we not with good reason conclude, that it is very instrumental, as a band of union in those bodies ; those particles (as Sir *I. Newton* observes †) receding from one another with the greatest repulsive force, and being most difficultly brought together, which upon contact cohere most strongly ? And if the attraction or cohesion of an unelastic air-particle be proportionable to it's repulsive force in an elastic state, then since it's elastic force is found to be vastly great, so must that of it's cohesion also. Let us add to this, that the air generated from the fleshy fibres, in the experiment above-mentioned, was not separated without great violence ; for it did not rise in any quantity, till the clouds did, which contained and brought over the salt and oil : whence it is evident that the aëreal particles are firmly fixed, and consequently are very instrumental in the union of the other constituent principles.

Sir *I. Newton* || thinks, that not only water and air are convertible into dense bodies, but that even light may become a part of gross bodies, and that they may receive much of their activity from the particles of light which enter their composition. It is the opinion also of Monsr. *Homborg*, that light or fire is a part of the composition of all things ; though in the analysis of bodies it is always lost, escaping the skill of the artist, and passing through the closest vessels. We all know that solar fire, or light, adds weight to lead, tin, or regulus of antimony, when exposed to the fire of a burning-glass, though they otherwise lose much in smoak and steam.

These being then the component parts of the muscular fibres, our next task is to shew the cause of their tension and elasticity. That all the muscular fibres of the body are in a state of tension, during health, is manifest from every incision made across them, when the 2 segments of the muscle so divided, retire, one to it's insertion, and the other to it's origination ; that is, every fibre is always stretched out beyond it's

* Modern Theory of Physic, p. 56.

† Opt. Qu. 31.

|| Opt. Qu. 30.

natural state of rest or quiescence, so that both ends of it retract a considerable distance after being cut asunder. Now there are two things which seem to be principally concerned in this affair; the impulse and pressure of the circulating fluids, always distracting the fibres, and a constant *nîsus* or endeavour in the constituent particles of the fibres to run closer together, when so distended, by means of their mutual attraction towards each other. The equilibration which is ever preserved between the antagonist muscles, in a healthy state, unless when the will directs it otherwise, arises from this *vis restitutionis*; which being stronger or weaker according to the degrees of tension, and the degrees of tension depending upon the velocity and quantity of fluids circulating thro' every fibre; it follows, that as long as the fluids have the same free access to every voluntary muscle, so long will the *æquilibrium* be maintained.

In an essay which I published on this subject of muscular motion, in 1733, I endeavoured to prove that every the least corpuscle of matter is endued with an attractive virtue on one of it's sides, and a repulsive power on the other, something similar to the loadstone; and this I was first of all induced to believe, from what Sir *I. Newton* observes in his opticks, *Qu.* 31. When any saline liquor (*says he*) is evaporated to a cuticle, and let cool, the salt concretes in regular figures; which argues, that the particles of the salt, before they concreted, floated in the liquor, at equal distances, in rank and file; and by consequence, that they acted upon one another by some power, which at equal distances is equal, at unequal distances is unequal: for, by such a power, they will range themselves uniformly, and without it they will float irregularly, and come together as irregularly. And since the particles of *Iceland* crystal act all the same way upon the rays of light, for causing the unusual refraction, may it not be supposed, that in the formation of this crystal, the particles not only ranged themselves in rank and file for concreting in regular figures, but also, by some kind of polar virtue, turned their homogeneous sides the same way? And again, we are taught by the same *Great Man*, that fire is the most simple of all known bodies, and consequently the most immutable; that each ray of fire or light has sides differently affected, and which have different properties; and that *Iceland* crystal is found to attract a corpuscle of fire, if one of it's sides be turned towards the crystal, and repel it, if the other be; for one and the same ray is here refracted sometimes after the usual, and sometimes after the unusual manner, according to the position which it's sides have to the crystal; and since the crystal, by this disposition or virtue, does not act upon the rays, unless when one of their sides of unusual refraction looks towards that coast, this argues a virtue or disposition in those sides of the rays, which answers to, and sympathizes with, that virtue or disposition of the crystal, as the poles of 2 magnets answer to one another.

We are fully persuaded, that, in the crystallization of salts, they could not so regularly and constantly preserve their peculiar shapes, forms, and figures, if every particle of them had not it's determinate poles: for granting that the component particles of each kind of salt have a peculiar shape and size, different from any other kind of salt, yet if they had a power of uniting with each other indifferently, at their tops, sides, and bottoms, one would think they could not always coalesce into crystals of the same regular figure: but if the constituent particles of every kind of salt have their determinate poles, then they cannot possibly unite with each other, but when their poles square with one another, and consequently they will always fly together, and be joined at such points, only where their corresponding poles are; which must of course constantly produce the same regular form and figure in every aggregate of such particular saline particles. Hence it is, that *salt ammoniac* so elegantly imitates the branches of a tree; *salt of hartshorn* a quiver of arrows; *salt of tin* shoots into lines like little needles, which spread themselves every way from a point, as from a centre, so as to represent a star, &c. Now can it be imagined that these, or any other kind of salts, would immutably and perpetually coalesce into crystals of the same regular figure and shape from any other principle? Since therefore we have so much reason to believe that salts of all kinds, and even the rays of light are endued with a polar virtue, that is, every corpuscle attracts on one of it's sides, and repels on the other; and since it is a well known axiom, that *Nature is ever frugal in principles*, I think it not at all unphilosophical, or contrary to any of the known laws of nature, to believe that every particle of matter in the world is endued with an attractive and repulsive property.

Thus then, if the constituent corpuscles of the muscular fibres are formed together according to this law, if they are all united at particular points corresponding to their attractive virtue, it follows, that wherever a muscular fibre is stretched out to the least degree of tension, some of it's particles will touch each other in fewer points; whilst others may possibly be disunited and removed from each other, though perhaps to inconceivable small distances: hence there will be a constant *nixus* in the separated particles to get together again; and this *vis restitutionis* will be stronger or weaker, according to the number of corpuscles so disjoined, and their attractive virtue.

If the power of the circulating fluids (and I think it cannot be denied) be sufficient, from the first beginning of the circulation of the *fetus*, and so on as long as life continues, to distend the fibres beyond the size they would otherwise be of, by reason of their corpuscular attraction; this distractile power must always be the occasion of some degree of tension in them: and if, upon the removal of this tension, the component particles have a property of running closer together, and contracting the fibres in their length, by the means above-mentioned, this must be the true cause of elasticity in the fibres. Hence therefore it follows, that

since the fibres are always in a state of distraction, by the quantity and *momentum* of the circulating fluids, and as they are ever endeavouring to shorten themselves, by means of their corpuscular attraction, their elasticity must depend upon tension; for the fibres could have no power to retract, or abbreviate their length unless they were extended beforehand by some certain force.

It is not a sufficient objection against this scheme to say, that if we depend upon what is visible, we shall never see the dry solid fibres, or particles of any solid body, once divided or drawn out of contact, coalesce or unite again, or recover the close contacts they had before; without some fluid medium superadded. 'Tis true, when a visible crack or flaw happens in any dry, hard, solid body, such as a steel-spring, or a dry, rigid, wooden bow, the rupture will always continue, by reason the severed particles cannot be brought again into the sphere of each other's attraction without the intervention of some medium; but then it does not follow from hence, that such a spring or bow cannot be bent at all without breaking; or that the constituent particles, which must necessarily be displaced by such a distending power, do not fly together again by their attractive virtue, when removed only to such minute distances.

The *minima*, or primary atoms of all bodies are non-elastic, as being perfectly hard, solid, and inseparable; and therefore elasticity must proceed from the aggregate, or composition of such atoms, which, by being capable of changing their situations, according to the impressed force, and being endued with a powerful attracting virtue, they instantly resume their former positions, when left to themselves to obey those laws which the *Great Creator* hath impressed upon them. As for instance: whilst a common steel spring, or any such elastic body, is not extended or bent, we presume every individual particle of it to be at rest; that is, they are all situated, in regard to each other, according to their poles, and embrace one another by their common principle of attraction; but no sooner is such a spring bent, by some impressed force, but many of it's particles on the convex side, must of course touch in fewer points, or perhaps be disjoined from each other, though to the most minute distances that can possibly be; whilst other particles, on the concave side of the spring must necessarily slip upon, or be crowded over one another. Hence it will follow, that if those particles, which are separated from each other, or touch one another in fewer points than usual, are yet so near each other as to be within their sphere of attraction, and not at all, or very little altered in regard to their poles, they will consequently attract each other very strongly, and fly together again, as soon as the impressed force is removed; whereas it is no unreasonable conjecture to suppose, that those particles on the concave side of the spring, which are compressed, and as it were rumped over one another, may be so much altered from their former positions, that their poles do not now answer to each other; and if not, they will repel

one another, according to their respective powers, till they have attained their former situations, or, in other words, till the spring has recovered it's former shape.

The same principles of attraction and repulsion are the cause of restitution or elasticity in all other kind of bodies. When a muscular fibre is stretched out longer than usual, it is most certain that some of it's component particles must slip upon or by one another, or else be removed at exceeding small distances from each other; so that if the impressed force be too violent, if the tension be carried so far as to disjoin a great number of the component particles beyond their sphere of attraction, the fibre will continue to grow weaker and weaker till it breaks: but it is as evident, on the other side, that when a stretched-out fibre does not break, but retracts itself into it's former shape and dimensions, upon the removal of the extending power, the particles which were displaced return again to their proper positions, merely by the means of their attractive virtue. Now all this being so agreeable to those *laws of nature* which that divine man Sir *I. Newton* has discovered to us, I think we have good reason to conclude it to be the true cause of elasticity in the animal fibres. The elasticity in the air indeed, or in water agitated by fire, or in all the exhalations proceeding from the earth, arises from the principle of repulsion only; for the particles of vapours, exhalations, and air, stand at a distance from one another, and endeavour to recede as far from one another as the pressure of the incumbent atmosphere will admit them. No power yet known is able to compress the air-particles within the sphere of their attraction towards each other, so as to destroy their elastic property; and yet single primary particles of air are continually attracted by other bodies, and consolidated with them, till by the action of fire, or fermentation, they are separated again, and restored to their repulsive state. Hence we may observe, that elasticity, in different kinds of things, or in matter differently modified, may arise from two several causes, *viz. attraction and repulsion*; and perhaps, in many instances, from the influence of both at one and the same time. Whenever any kind of matter is actuated by fire, by fermentation, or dissolved by any menstruum, so as to throw off it's particles in subtile vapour, there will be a constant endeavour in those particles to recede further from each other; so that the more they are confined, or compressed, the greater will be their elastic power: whereas in solid bodies, this property of elasticity proceeds chiefly from attraction, or a *nisus* in the component particles to fly back, or run into close contacts again, whenever they happen to be stretched out, or bent, so as to touch each other in fewer points.

From what has been said we may deduce the following *corollary, viz.* That whenever *elasticity* proceeds from the principle of *repulsion*, as it does in air, vapours, &c. some *compression* is necessary, in order to force the elastic matter into a narrower compass than it would otherwise possess; but when it arises from *attraction*, as in the muscular fibres, and

all

all solid bodies, some *distractile* force is requisite to disjoin the component particles from their usual contacts, before it can exert it's power; and perhaps, for want of attending to this difference, so many various opinions may have arisen concerning the cause of elasticity.

In my former Lecture I endeavoured to explore the several constituent principles of the muscular fibres, and to shew the true cause of their cohesion, tension, and elasticity. In this, I hope, I shall make it appear, 1. That every fibre constituting a muscle is *tubular*, and of a *cylindrical* shape, or very nearly such; and not a string, or chain of bladders, according to an *hypothesis* which has been too long and too generally received. 2. That the corpuscular attraction between the component particles of the fibres is so far increased and strengthened by the influence of the nervous *æther*, which is always at the command of the will, as to purse up and shorten every fibre in it's length, whereby an intumescence arises in the belly of the muscle, though it is contracted in it's other dimensions, so as, in the whole, to possess less room.

Being favoured with the use of a most excellent microscope, I made the following experiments.

1. I divided some fibres as minutely as I possibly could, from the heart of an ox, from a part of the diaphragm, from the intercostal muscles, and from a rump of beef; all which were boiled to such a degree of tenderness, that we separated some fibres with the point of a needle, which were not visible till placed under the microscope, and even then they did not appear bigger than hairs, though others, which looked like hairs to the naked eye, were magnified to the size of wheat-straws. All these seemed to be fascicles of continued tubes, as far as we could view them, without any partitions or cells. 2. Upon rending a muscle, which was taken from a knuckle of veal, and boiled for 4 or 5 hours, several of the transverse, as well as longitudinal fibres appeared very distinctly; which being placed under the microscope, and having a strong focal light cast upon them by means of a *Florence* flask filled with water, they seemed to be shrunk up, either by being boiled so long, or by being exposed to the air, so that their surfaces seemed to be unequal and corrugated; which is what Mr *Leeuwenhoek* * says deceived him at first, so as to make him think these corrugations were so many vesicles or cells; but he soon discovered his mistake. In some of the fibres I could plainly discern a dark list running in the centre, from one end to the other; but what it was, I could not discover. 3. Having observed the muscular fibres in the leg of a sea-crab to divide very easily and distinctly from one end of the muscle to the other; we placed a great many of them under the microscope, but could not discern any thing like partitions or cells. In short, Mr *Leeuwenhoek* † assures us, that the minutest fibres that are visible to the naked eye, appear through a good microscope to be invested with a membrane, which includes within it

not one simple body, but a bundle of still finer fibres, the last, or smallest order of which he thinks to be simple hollow tubes. This perhaps is as good authority as we can have from the assistance of microscopes; but if we may be allowed to deduce our arguments from the analogy which the muscular fibres bear to some other parts of the body, whose shapes we are well acquainted with, the reasonableness of this opinion may appear yet stronger.

All Anatomists agree, that the muscular fibres have their rise from the extremities of the nerves and blood-vessels; every fibre being supplied by a branch of a nerve, and an artery, and having also a vein arising from it. That the nervous *capillamenta* are cylinders is not denied by any one that I know of; and though the arteries have been for a great while thought to be conical, yet the ingenious Dr John Stephenson * *F. R. Coll. of Phys. Edinb.* hath evidently demonstrated the whole arterial system to be cylinders, frequently divided and subdivided, still terminating in numbers of small cylinders, the aggregate of which is always of greater capacity than the trunk or larger cylinder before the ramification.

May we not therefore very reasonably believe, from the simplicity and uniformity in all the operations of nature, that the muscular fibres partake of the same figure with those from whence they have their rise; especially when such a shape (as will appear in the sequel) is more proper for all the functions of a muscle than any other whatsoever? I don't mean by this, that every fibre of every muscle is a perfect and regular cylinder from one end to the other; many of them may be thicker in their coats, and larger in their bores about the middle than towards each end, similar to the shape of the muscle; but what I think the most reasonable opinion is, that the smallest *fibrillæ* are hollow tubes not divided into an infinite number of cells or vesicles. The longitudinal, red, fleshy fibres seem indeed to be contorted and bound about in many places, with white, spiral, and transverse ramifications of the nerves; but I can see no reason to believe that these nervous filaments divide the longitudinal fleshy fibres into several apartments or cells; I rather think that they only dip into the cavities of the fibres, in order to convey into them the *æthereal medium*, which is contained in the nerves.

Before the laws of nature, and the animal oeconomy were so well known as they are now, I don't wonder that the vesicular scheme was thought a reasonable one, till it came to be examined by strict rules and experiments. The common experiment of raising weights by blowing up bladders might seem, at first sight, a very feasible way of explaining muscular motion; and without doubt this first of all gave birth to the vesicular *hypothesis*. But the fallacy of this experiment was not discovered for want of attending to the difference between bladders which have been already blown up, and dried, and such as are recent

and supple. If a string of dry bladders, which have been once distended as far as they could bear without bursting, and are now again squeezed close, and stretched out only in their length, by means of a weight hung at their bottom; I say, if such a string of bladders be blown up, it will undoubtedly distend their transverse diameters so as to raise up the weight: but in all tender yielding vesicles, such as the muscular fibres most certainly are, in their last or smallest order, it is well known, that if they were to be inflated with air, or any such-like matter, it would distend them in every direction alike; they would grow longer as well as wider. Hence it follows, that if the above-mentioned experiment was to be made with bladders just as they are taken out of animal bodies, it would not answer the purpose, as is evident from blowing up those of *calves, hogs, &c.* The muscular fibres, it is true, are always in a state of tension, but then this tension is very far from being to their utmost stretch; so that, were they to be inflated in the manner above-mentioned, every muscle would necessarily increase in length as well as breadth.

Another insuperable difficulty belonging to the vesicular *hypothesis*, is how to blow up a bladder open at both ends; which every vesicle is supposed to be, by having a free communication with the blood-vessels. Having therefore so much reason to conclude, that the muscular fibres, in their ultimate divisions, are not cellular, but tubular, let us proceed to shew the manner and cause of their contraction.

This proposition is clearly demonstrated by that famous experiment communicated to the *R. S.* by Dr Goddard* in 1669, where putting a man's arm into a glass cylinder full of water, the water always sunk when the muscles of the arm were contracted, and rose again to the first standard when they were relaxed. This we think may be looked upon as an *experimentum crucis*; whereas if every fibre was a chain of bladders, whose contraction in length arose from their inflation in breadth, all the world knows there would be a sensible swell of the whole arm upon muscular action.

A muscle in its motion very evidently grows less in bulk.

There are still other difficulties attending the vesicular *hypothesis*. If the animal spirits are supposed to inflate the cavities of the muscular fibres merely by a propulsive force, like unto the steam of boiling water working in the engine to raise water by fire, it ought to be proved from whence so strong an impulse should arise; and also how the nerves, which are the conduits through which this flatulent matter must be conveyed, should lie so loose and unelastic; it being evident from all experience, that if such an elastic flatulent vapour was to fly through the whole length of the nerves, with an energy sufficient to give a man a power of lifting up great weights, the nerves must be stretched out in proportion, and consequently would be very tense and elastic.

Those

* Vide Register of the Royal Society, Vol. IV. p. 95.

Those who suppose the inflation of the muscles to arise from a fermentative motion in the fluids, ought to prove, by a proper number of experiments, that there are juices existing in the body capable of such sudden and violent rarefactions or explosions upon mixing with each other; and if this possibly could be done, the diminution of the bulk of the muscles in action, would overturn all their scheme. Hence it is evident that the *vesicular hypothesis* ought to be intirely rejected, as being repugnant to the *laws of matter*, and to the *phenomena* of the muscles.

By undoubted experiments we are convinced, that the intumescence of a muscle, when it acts, arises merely from a change made in it's figure; that is, as it shortens in length the belly grows thicker, and yet the bulk in general is diminished. Let us therefore inquire after the agents which are capable of producing such surprising *phenomena*, and at the same time shall be consistent with every other operation in the animal oeconomy.

From what has been said it appears, that contraction, or muscular action, does not depend upon any fluid dilating or distending the fibres; but, on the contrary, they shrink up and grow less. The instantaneous alternations from constriction to dilatation, and *vice versa*, manifestly discover that muscular motion cannot be caused by such juices as the *blood*, *lymph*, and such-like; but it must be from some more subtile *æthereal* matter, which may be mixed with the blood in general, and secreted from it by the glands of the brain.

Let us but carefully consider the exquisite *apparatus* of the brain, the quantity of blood it receives, the infinite number of it's excretory ducts, and the great divisibility and subtilty of matter, and we shall find great reason to conclude that there is a most subtil, *æthereal*, volatile fluid, of great force and elasticity, perpetually secreted from the blood, by the glands of the brain, and continually flying into the nerves, for the uses of muscular motion, and many other great purposes of the animal oeconomy.

The delicate texture of the nerves, as well as that of the brain, implies that the fluid they convey to the muscles must be exquisitely fine. Indeed when a nerve is wounded, there flows from it a sweet, soft, clammy, balsamic juice, which no doubt is carried, at all other times, by the evanescent nerves to their ultimate divisions, in order to nourish and preserve the most minute *fibrillæ*, and all their expansions; and this may properly be called the *succus nutritius* of the nerves. But I cannot conceive that this visible juice has any thing to do with the immediate cause of voluntary motion; for so viscous a matter could never admit of such sudden vicissitudes, as are in muscular action, if it was capable of performing it in other respects.

There are abundance of considerations which evince the existence of some subtil spirit in the nerves, much finer than to be the object of our senses. We have no proof, either from experiment or reason, of any

any other instrumental or physical cause of sense or motion, but this *animal æther* which is elaborated from the blood. Dr *Mead* * thinks no regard ought to be had to the immechanical notions of those authors, who imagine that there is no such thing as a nervous fluid in an animal body; and that muscular motion and sensation are performed only by the vibrations of the fibres of the nerves, without the intervention of any spirituous fluid.

The surprizing discoveries which have been made of late years, by a variety of experiments upon Electricity, do in some measure give us an idea of the great subtilty and velocity of the nervous fluid. I have been informed by Mr *Watson*, that the swiftness of the electrical *effluvia* is prodigious; that one stroke of his hand down the tube, when well electrified, was felt as soon as his hand could be at the bottom of the tube, through five men standing upon electrical cakes, and communicating with each other by a cane, sword, or any other non-electric. Hence it follows, that if a tube could be always excited, and was always to be applied to the end of a proper cord or string; the electric matter, which is excited by friction between the hand and tube, would ever be ready to exert it's attractive influence on leaf-gold, and such-like things, when placed within a due distance of the end of the string; and perhaps this may be very similar to the motion and action of the nervous *æther*.

Thus much being premised, and it being taken for granted, that we have an *æthereal medium* in the brain, spinal marrow, and all the *capillamenta* of the nerves, ever ready to be conveyed into the muscular fibres, by the power of the will, and which *medium* consisting of the most refined matter in nature; it follows, that the motion of this nervous *æther* may be as quick as lightning, and also its attractive power must be exceeding strong, by virtue of it's vast degree of subtilty; as is evident from what Sir *I. Newton* † has calculated concerning the rays of light. From these observations therefore, and from what has been said above concerning the cohesion and elasticity of the animal fibres, I think we have great reason to conclude, that muscular motion does proceed from the influence which the nervous *æther* has upon the component particles of the muscular fibres themselves, by instantly increasing their attractive virtue towards each, so as to make them run closer together, or, as it were, up into heaps, as long as such an additional attractive *medium* is in the fibres.

If we look back and consider the nature of the *vis restitutionis*, or elasticity in the muscular fibres, we shall find, that the cause of that property only wants to be increased, in order to overcome the distractile force of the circulating fluids, and the resistance of the antagonist muscles: whence it follows, that if such a power was to be increased in one set of muscles, and not in their antagonists, those muscles, whose elastic

* Introduction to his Essays on Poisons, Edit. 3.

† Opt. Qu. 23, 24.

or retracting power was increased, would be abbreviated in their length, whilst the others would be extended and lengthened.

When any muscle is freed from the power of it's antagonist, by a wound, &c. it immediately contracts, and is not to be extended again by the power of the will. Whence it has been said, that contraction is the proper state of the muscles, and to which they always tend: but if we narrowly inspect into this affair, we shall find, that, when a muscle is contracted in this manner, it is no further so than the elastic restitutive property in the fibres is concerned. We do not find that such a muscle is indurated, or it's belly swoln like unto what it is in voluntary action; for here being no influx of the nervous *æther* to increase the corpuscular attraction, the muscle is shortened only by the inherent mutual attraction between the constituent particles of it's fibres, without any matter being superadded. This kind of contraction therefore is evidently the state to which the elastic fibres, tend by a continual *conatus* in the component particles to accede towards each other without the assistance of the nervous *æther*; so that this natural *vis motrix* in the musculous fibres is no more than what we mean by their elasticity, or restitutive property: it seems however to be demonstrated from hence, that muscular action, and elasticity in the fibres, proceed from the same cause in different degrees; *viz.* from corpuscular attraction.

Let us now endeavour to corroborate these arguments by some suitable observations. 1. From what has been said we may conceive more readily, than we know how to express, that the will has a power to direct the *æthereal medium* contained in the nerves, to any of the voluntary muscles, with such a degree of celerity as it pleases; and to stop the influx as suddenly. 2. It is evident that the coats, or carnous substance of every fibre, must necessarily increase in it's thickness, when it abates in it's length; and what power can produce this effect, but such a one as increases the mutual attraction between the constituent particles? 3. Hence appears the reason, why the belly of a muscle swells during it's contraction, notwithstanding it's dimension in general is diminished; for as the component particles of each fibre are more loosely joined together about the middle than towards it's extremities, which are generally tendinous, it is natural to suppose that the chief action is between them; that is, when a fibre grows shorter, such of it's particles which are most at liberty run nearer together, and as the motion of all bodies is ever in proportion to the impulse they receive, and the resistance they meet with; so when the constituent particles of the muscular fibres are drawn into a shorter compass, by the means above-mentioned, the middle of the fibres must swell either inwardly or outwardly, or both, according to the resistances they meet with. And *lastly*, Since the coats of the muscular fibres do most certainly grow thicker as they contract in their length, and yet the external surface of the muscle in general is diminished; it manifestly follows that their cavities must grow less, and their contained fluids must be pressed out, in proportion to the contraction

contraction of the muscle. This appears upon blood-letting, when squeezing any thing hard in the hand will make the blood fly out with a greater velocity, and thereby form a larger *parabola*. This also accounts clearly for the induration and paleness of a muscle during it's action. And again, it follows hence, that in the action of the muscles there is an alternate *diastole* and *systole* perfectly analogous to the action of the heart, which greatly contributes towards pushing on the blood in the veins.

The muscles being contracted merely by the influence of the nervous *æther*, and the influx of the *æther* being stopt by withdrawing the *impetus* given to it by the power of the will; the reason and manner of their relaxation will easily appear. For since the nervous fluid is extremely subtil, that portion of it which is thrown into the muscular fibres, acts but for a moment, or the least space of time, so quick is it in it's motions, and so penetrating in it's nature; and no sooner is the vigour of the attraction over, but the tension of the antagonist muscles, and the impulse of the blood will extend them again. Whoever duly considers the well known effects of magnetical and electrical *effluvia*, will be at no loss to conceive the instantaneous influence which the nervous *æther* has upon the muscular fibres.

It must be confessed indeed, that these *intima naturæ*, or secret operations in the animal oeconomy, are all skreened from our knowledge, the agents being too subtil ever to become the objects of our senses, though ever so well assisted; so that we can only form our schemes, and deduce our arguments from such collateral proofs, or from such *data* as we are pretty sure are true. As for instance; the influence which the *soul* has upon the *æthereal medium* in the nerves must be by impulse; for though our finite capacities are not able to comprehend the nature of immaterial impulse; yet nothing is more certain than that the most subtil matter in the universe cannot be moved without some impressed force.

That the will does exert itself after this manner, is in a good measure proved by Dr *Stuart's* * experiment upon a frog, where a proper impulse being given to the *medulla spinalis* did excite motion in the voluntary muscles, though the head was severed from the body. Hence also it appears, that the nerves are always replete with a subtil fluid capable of contracting the muscles, or otherwise such an impulse on the beginning of the nerves, could not have excited motion after the head was cut off. And again, common experience assures us, that tho' the nerves are always replete with an *æthereal medium*, yet this *medium*, in a state of health, never flies out at their extremities, into the muscular fibres, without some impulse by the direction of the will: whenever it happens to do so, convulsions and cramps are the natural consequences.

* Lectures on Muscular Motion.

It may perhaps seem strange to some, that I have not all this while taken any notice of the blood, as an agent in muscular motion; since it has ever been reckoned some way necessary towards it. But notwithstanding this opinion has been so long and so generally received, yet if our scheme be the true one, it evidently appears the blood hath nothing to do with the immediate contraction of the muscles. From the close connection of the nervous *capillamenta* in all or most of their ramifications, to those of the arteries, it seems as if the *diastole* and *systole* of the arterial system was some how useful to them. Perhaps it may assist in pushing on the *succus nutritius*, or that clammy balsamic juice which is in the nerves, towards their extremities; but I cannot conceive that the blood itself is in any way assisting towards muscular motion, except it be by keeping the fibres warm, supple, distended, and every way ready for the influx of the nervous *æther*. I have tied up and cut asunder both the *carotid* and both the *crural* arteries of the same dog, without destroying the motion of one muscle. Nothing less than laying a ligature on the *aorta descendens* will destroy the motion of the hinder parts; and possibly this may happen from the great distension of the *aorta* above the ligature, pressing upon the nerves which go to the lower parts. It is certain indeed, when all the blood is intercepted the fibres will soon collapse, and grow flaccid, and muscular motion will cease, merely for want of the warmth, suppleness, and distension which the muscles receive from the blood. But what I think most reasonable is, that the blood is no way concerned as an efficient cause in purring up and contracting the fibres; it rather by it's motion through the muscles, acts as an antagonist to their contraction, by extending and distending them; for the blood, by the *diastole* and *systole* of the arteries, is continually urging on it's passage through the muscles.

Thus I have endeavoured to deduce and illustrate the cause of muscular motion from true principles, by pursuing only those *laws of nature*, which our great Philosopher has in so surprizing a manner discovered to us. But I am far from thinking this a complete account; I know it requires more experiments, and better reasonings than I am master of, to explain it as it ought; and even after all, there are, and ever will be, some things above the reach of our capacities to demonstrate, any otherwise than by their effects, or second causes: such are, the nature of an immaterial impulse; the real existence of so subtil a fluid as is attributed to the nerves; and the true causes of attraction and repulsion.

That the SUPREME BEING hath implanted an immaterial spirit in every living creature, for the purposes of sensation and voluntary motion, I think cannot be denied by any one in his senses: but perhaps it may not become us to be too solicitous about the *modus* of action betwixt the *soul* and *matter*; these things being above the reach of human reason. It is sufficient for our purpose, that we know the will has a power of determining the nervous *æther* immediately and directly to every individual voluntary muscle.

The existence or non-existence of the nervous fluid, commonly called the animal spirits, has been a controversy of long standing. The first searchers into the structure of the human body soon found that muscular motion depended upon the nerves, or something within them; and this has constantly been asserted, and admitted as a known truth. The advocates for the existence of animal spirits have generally supposed that voluntary motion was performed by a sudden inflation of the muscles, either by the power of the nervous fluid itself, or by an instantaneous ferment with some other fluid; and I am apt to believe, that this doctrine, proving inconsistent with many things relating to the animal oeconomy, and contrary to some known experiments, might give rise to the vibrating scheme, where the existence of the animal spirits is denied, and where it is supposed that both sensation and muscular motion may be performed merely by the elasticity of the nerves, and contractions first of all begun in the brain, and so communicated to the fleshy fibres: but this is so immechanical a notion as not to deserve an answer; it being impossible for a vibrating motion in one cord or string, were it ever so elastic, to cause a contraction in another, without the intervention of some fluid.

I shall conclude this lecture therefore with observing, that the existence of an *æthereal medium* in the nerves is past all manner of doubt; it being otherwise contrary to the known laws of nature for the nerves to be the cause of muscular motion if they were solid, or did not admit the most subtil fluid, secreted by the glands of the brain, to pass through them. And since it is known from experiment, that the muscles grow less in action, and, consequently, the constituent particles of every fibre must run nearer together before such a *phænomenon* can happen; we think it very manifest that this property of constriction arises from the principle of corpuscular attraction being increased and strengthened by the influence of the nervous *æther*; a principle, which, from the endless divisibility and subtilty of matter, we may never be able to comprehend, though we know it to exist in nature from innumerable observations and experiments.

In confirmation of the truth of what has been said in the two preceding lectures, and for a further illustration of this subject of muscular motion, I beg leave to offer some thoughts concerning the cause and manner of action in the involuntary muscles; and after that I shall relate some experiments, which I have made in order to illustrate our theory in general, as far as the nature of the subject will admit.

When any muscle, voluntary or involuntary, is fully contracted, that is, when it's component particles are drawn into the closest contacts they are capable of, by the influence of the *æthereal medium* in the nerves, it is evident, from all the laws of matter, that they would not recede from each other again without some impressed force. Now in all the voluntary muscles we very well know, that when one set of them are contracted, their antagonists are lengthened, and *vice versa*;
so,

LECTURE
III. Read
April 9. 1747.

so that the *vis restitutionis* in all the stretched-out fibres, and the *momentum* of the fluids to enter again into the contracted muscles, will instantly dilate and distract them, when the impulse of the will ceases, till an exact equilibration is restored.

This being the plain matter of fact in regard to the voluntary muscles; let us now endeavour to find out the mechanism belonging to the involuntary ones, and more particularly of that very curious one the heart. Dr Hoadly, in his *Lectures on Respiration*, has manifestly shewn that the external ranges of the intercostal muscles appear to have all the characteristic marks of antagonist muscles. If you take three ribs together, *says he*, and observe the different ranges of the fibres in the two intercostal muscles, which lie on each side the middlemost of these ribs; you will see, at first sight, that the internal range in the lower muscle, counter-acts directly the external range in the upper muscle. The action of the *diaphragm* is also counter-balanced by the abdominal muscles, and contents of the *abdomen*; which squeeze it up, in a convex form, into the cavity of the *thorax*, upon every expiration. As to the *sphincter* muscles, they seem to be always in the same natural contractile state; and whenever they are stretched out, by some superior power, they recover their usual dimensions merely by their restitutive or elastic property. The same may be said of the *stomach*, *uterus*, *vesica*, which contract by the same means, into a narrower capacity, as soon as their contents are discharged. I shall confine my thoughts therefore to the action of the heart only, as being the most perfect involuntary muscle.

The heart is a *viscus* which has given the *literati* a great deal of trouble to find out it's real mechanism, and the true cause of it's regular alternations of contraction and dilatation. I have oftentimes laid open the breast of a dog, and kept his lungs playing with a pair of bellows, in order to observe how regularly and alternately the *systole* and *diastole*, both of the heart and it's auricles, followed each other; that is, when the ventricles were contracted, the auricles were dilated, and *vice versa*: so that the auricles seem, in some measure, to act as antagonists to the ventricles, and the reflux blood may be of the same service to the auricles. But there being so much disparity between the contractile strength of the ventricles and that of the auricles, there must necessarily be some other cause, which, when the heart is fully contracted, makes it unbend again, or cease to contract; by which means the auricles, though so very weak, in comparison of the ventricles, have power enough to throw in blood, and thereby to distend the ventricles to a certain degree, before they are capable of acting again. If we might be allowed to make an estimate of the difference between the contractile strength of the heart and it's auricles, from the difference of their bulk, or quantity of fibres, we should find it to be about 9 to 1; as I have observed, by weighing the hearts of several animals, and their auricles separately. Hence it does not seem reasonable to think, that the weak efforts of the auricles would be sufficient to cause the *diastole* of the heart,

heart, without some other assistant. One great use of the auricles is, to receive a quantity of blood, during the *systole* of the heart, sufficient to fill the ventricles again at their *diastole*. The same may be said, in some measure, of the veins nearest the heart, which may be plainly seen to dilate during the contraction of the auricles; so that the heart, auricles, and veins, have all their *systoles* and *diastoles* in subordinate degrees. Without such receptacles as these it would be impossible for the ventricles of the heart to be filled from the veins so suddenly as they are; for though the *areas* of the transverse sections of the veins are much larger than those of the arteries, yet we find, by Dr Hales's *Hæmostat. Exp.* that the velocity of the blood in the arteries is above 6 to 1 to that in the veins: but since the auricles keep receiving the blood whilst the heart is in it's *systole* (the veins doing the same by the auricles) a due quantity of blood is always ready to be thrown into the heart, by the time it is fit to receive it; for the *diastole* takes up $\frac{2}{3}$ of the space of time between each pulse.

Here then we may observe that the *momentum* of the reflux blood acts upon, and distends the veins nearest the heart, whilst the auricles are contracted; that it also dilates the auricles, whilst the heart is in action; and that the heart is in it's *diastole*, whilst the auricles are in their *systole*. But the great difficulty in accounting for the *diastole* of the heart, is from the disproportion between it's contractile power, and that of the auricles: it being plain, from what has been said above, that the *momentum* of the blood in the veins is stopt by the contraction of the auricles, so that the ventricles of the heart can receive no other impulse from the blood at that time, than what is derived from the contraction of the auricles. Hence therefore it follows, that if the *systole* was the natural state of the heart, and to which it always tended with it's full contractive power, the impulse of the blood, from the contraction of the auricles, could never be able to dilate it.

Dr Lower makes the *systole* the natural state or action of the heart, and the *diastole* the violent one: Boerhaave, on the contrary, makes the *systole* the violent, and the *diastole* the natural state. But perhaps neither of these opinions may be right, in the strictest sense; for if we look back we shall find, that if the fibres were not tense they could not be elastic; and if some violence was not put upon them, by the impulse of the circulating fluids, they would not be tense: hence it appears, that elasticity proceeds from a state of violence. And again; when a muscle is freed from the power of it's antagonist, and is thereby left at full liberty to contract, as it always will by it's elastic restitutive property only, it does so no further than in obedience to the common power of attraction between it's component particles; but in all other muscular action, when this attractive power is greatly increased by the influx of the nervous *æther*, the constriction is carried much further, and the muscle is more fully contracted than it ever is in the other state: hence it follows, that contraction, in it's fullest degree, is not the natural state of a muscle.

muscle. And further ; when the constituent particles of the fibres are drawn into their closest contacts, by the influx of the nervous *æther*, it requires some force, in a contrary direction, to elongate the fibres again ; so that extension, or dilatation is also a state of violence. From the whole then it appears, that neither the *systole* nor *diastole*, in a full degree, is the natural state of the heart ; and this we shall shew more plainly hereafter by inspecting the hearts of animals after being bled to death.

From what has been said above, it seems reasonable to conclude, that if contraction, in it's fullest degree, was the natural state of rest or quiescence in the heart ; the *momentum* of the blood from the contraction of the auricles, could not be a sufficient counterpoise. And since so many fruitless attempts have been made to account for the *diastole* of the heart ; from the *impetus* of the blood in the veins, and from the pressure of the atmosphere, &c. give me leave to propose the following queries. 1. May not the heart be a compound muscle ; that is, may it not have it's antagonist within itself ? or, in other words, are not some of it's fibres so ranged, that whilst one set of them is contracted and shortened, others may be stretched out ; analogous to the action of the intercostal muscles, or any other muscles with their antagonists ? 2. Is the nervous *æther* transmitted from the brain to the heart in a pulsatory manner, at equal distances of time ; or may it be supposed to move uniformly thro' the nerves, and some interruption is given to it's influx into the muscular fibres, when the heart is in it's *systole* ? 3. Does not the *diastole* of the heart depend upon an abatement of the tension in the contracted fibres ; a motion of restitution in such as are over stretched ; and the influx of the blood conjunctly ? Without some such mechanism as this, no power that we know of, belonging to the animal oeconomy, would be able to cause the *diastole* of the heart : but if such a structure, as is above-mentioned, could be proved, no more difficulty would attend the explication of it than that of the voluntary muscles.

In dissecting the heart we find a great many different orders or series of fibres, variously contorted, and running in contrary directions ; so that, for ought we know, it may be an *epitome* of muscular construction in general. We may here observe what care *nature* has taken to prevent too great a dilatation in the *diastole* of the ventricles, the right one especially, as being the weakest, by forming the *papillæ*, or *columnæ*, which run from the *septum*, or middle partition, to it's opposite sides ; whereby they act as so many braces in the *diastole*, and when they contract, they also assist in the *systole*. And perhaps it may not be the most improbable conjecture to think ; that as much care may have been taken in providing a sufficient number of fibres, or little muscles, which may be so formed as to act in the *diastole*, as antagonists to those which occasion the *systole*. The hearts of frogs, vipers, eels, &c. seem to evince the reasonableness of this conjecture, by continuing their *systoles* and *diastoles* after they are taken out of the body ; when there is no reflux blood

blood to dilate the ventricles, and, consequently, if there were no fibres upon the stretch, when the heart is contracted, which, by their elasticity, or restitutive power, did pull back, or elongate such as were contracted, there could not possibly be any *diastole* in such circumstances.

We are assured by Mr Boyle, in his *Physico-Mechanical Experiments*, that the heart of an eel hath continued to beat an hour, in an exhausted receiver; after which, finding it's motion very languid, and almost ceased, by breathing a little upon that part of the glass where the heart was, it quickly regained motion; and an hour after that, finding it almost quite gone, he was able to renew it, by the application of a little more warmth. The same *illustrious author* further assures us, that he has sometimes cut the heart of a flounder transversely, into 2 parts; and freeing each from the blood it contained, he observed, for a considerable time, that both of them together continued their former contraction and relaxation. And once, thus cutting one into several pieces, he found, to his surprize, that they not only moved as before, but that even the whole, thus separated, long preserved the same succession of motion, as appeared therein whilst coherent. Now can the *diastole* of the heart, in these experiments, be accounted for upon any other principles than those we have laid down? Here was no impulse from the reflux blood to dilate the auricles, or to distend the ventricles. The pressure of the atmosphere was also quite taken away in the first experiment, and could not possibly be of any service in the last; and yet the *diastole* continued. Can the same fibres which are contracted, have it in their own power to fly out again to their usual lengths? If this cannot be, what power is there in a heart, taken out of the body, to unbend itself, or to dilate it's ventricles, after they are once contracted, unless we suppose some of it's fibres to act as antagonists to others? We have good reason therefore to believe that some fibres of the heart are always stretched out beyond their natural tone, when others are contracted; so that, by their elastic, restitutive property, they do act as antagonists in a certain degree.

In regard to the *systole*, in such hearts as are taken out of the body, and cut into several pieces, we conceive, that as long as warmth and moisture remain, so long may the *æthereal matter* in the nerves, continue to fly into the fibres, and contract them; and when it ceases, more warmth, or a gentle impulse (even with the point of a needle only) will revive the motion. Hence we may, in some measure, discover the amazing subtilty of the nervous *æther*; when such very small sections of the nerves, as in the above-mentioned experiment upon the heart of a flounder, should contain matter sufficient for so many contractions.

Let us now return to our *second query*, and examine whether the nervous *æther* is transmitted from the brain to the heart, in a pulsatory manner, at equal distances of time; or whether some interruption is only given to it's influx into the muscular fibres, when the heart is in it's *systole*. In order to understand this, we must look back and con-

sider, that, according to our theory, if the *æthereal medium* in the nerves was perpetually flying into the muscular fibres of the heart, it would be constantly contracted, notwithstanding the *momentum* of the blood, the contraction of the auricles, or the *vis restitutionis* in the stretched-out fibres. Hence then it is evident, that the alternate contractions and dilatations of the heart proceed from an alternate influence of the nervous *æther*; but how this alternation happens, when the nerves which supply the heart are not, in the least, under the direction of the will, is the difficulty we labour under.

In all the nerves which supply the voluntary muscles, it is certain there are restrictions in some parts of them which the *æthereal* matter is not able to dilate without an additional impulse from the will; or otherwise cramps and convulsions would perpetually happen. But in those which supply the heart, the motion of the *æther* through them cannot have any assistance or impulse from the will, as not being in the least under it's influence: so that the vibrations of the *meninges* of the brain, and the dilatations of the arteries may be supposed to be the agents which propel the *æther* towards the heart. Hence then it seems to follow, that the alternate contractions of the heart may proceed from the alternate impressions made on the nerves, by the *meninges* of the brain, and dilatations of the arteries.

Upon opening the skulls of living animals, the *dura mater* may plainly be seen to have it's *systoles* and *diastoles* corresponding to those of the arteries; but since the heart continues to beat after the head is cut off, or even after it is taken out of the body, where there cannot be any alternate succussions made on the nerves by the *meninges* of the brain, or by the pulsation of the arteries, it seems reasonable to believe that this alternation is occasioned by some impediment being given to any further influx of the *æther* into the muscular fibres of the heart, when it is fully contracted; or otherwise, the heart would constantly remain in a state of contraction, as long as there was any *æthereal matter* flying from the nerves.

If we consider in how many different directions the muscular fibres of the heart run, how much they are corrugated, thickened, and swelled, when fully contracted, and how strong and uniform the pressure must be in their greatest degree of action; it may not perhaps appear unreasonable to think that the extremities of the nerves, which are inserted into every fibre, and are extremely small and tender, may be pressed upon and squeezed, so as to prevent the influx of the *æther*, till the pressure is abated, or till the fibres are extended again to their usual lengths. What seems to evince the reasonableness of this supposition is the nature of the *shaking palsy*; where the voluntary muscles immediately become involuntary ones, so far at least, as to be alternately contracted and relaxed without the consent or direction of the mind. Now this distemper we believe to proceed from a particular weakness in the nerves, whereby those little restrictions in them, which keep the

æthereal

æthereal matter within due bounds, in a state of health, are so far weakened and destroyed, that the *æther* has a power of flying into the muscular fibres without any impulse or direction from the will, after the same manner it does into the heart. If therefore no interruption was to be given to the influx of the nervous *æther* by the muscles themselves, when they were contracted, it would follow, that if the *flexors* of any limb were to be first contracted, the *extensors* would not be able to recover the *æquilibrium*, and to be contracted in their turn, without the assistance of the will; for if our doctrine be right, that muscular motion proceeds from the constituent particles of the fibres being drawn into closer contacts by the attractive influence of the nervous *æther*, it necessarily follows, that if the same quantity of *æther* was continually to fly into a muscle already contracted, it would have a greater influence on the component particles so approximated, than on others in the antagonist muscles, which are distracted, and consequently touch each other in fewer points. Hence it seems to be evident that some impediment is given to the influx of the nervous *æther* when the muscles are contracted, or otherwise that they would always remain so; for, though the will may be able to remove such little impediments, and to keep the voluntary muscles in a state of contraction for a considerable time; yet since the voluntary muscles, when affected with a palsy, are regularly and alternately contracted, and have their *systoles* and *diastoles* analogous to the heart and it's auricles, I think we have good reason to conclude, that the same principles which contract and dilate the voluntary muscles, in the above-mentioned condition, are the principles which occasion the *systole* and *diastole* of the heart, with this difference only, that the influx of the blood into the *sinuses* of the heart, is what no other muscle in the body has or receives; and without doubt this may truly be reckoned the greatest assistant in dilating the ventricles of the heart, after the attractive influence of the nervous *æther* ceases, and the *æquilibrium* is restored between the stretched-out fibres and those which were contracted.

What has been said seems greatly to evince the truth of our *third* and *last* supposition, that the *diastole* of the heart may depend on an abatement of tension in the contracted fibres, a *vis restitutionis* in such as are over-stretched, and the influx of the blood conjunctly.

Give me leave to relate 2 or 3 experiments which I have made, in order to illustrate the foregoing theory, and then I shall conclude.

Having observed that the ventricles of the hearts of all animals, when bled to death, are dilated, or stand wide open to a certain size, though there happens to be very little blood in them, it seems as if the *diastole*, in a certain degree, was the last motion of the heart. In order to know the truth of this, I opened the *thorax* of a dog, and kept his lungs playing with a pair of bellows, that I might perfectly see the several actions of the heart and it's auricles. The auricles seemed to me to begin the motion, and the *systole* of the heart always instantly followed that of the auricles. Then the *apex* and sides of the heart sunk down, and

EXPERI-
MENT I.

and were lengthened of their own accord, before any blood was thrown into the ventricles, from the contraction of the auricles ; for there was oftentimes more than double the time taken up in the *diastole* of the heart, than the *systole* both of the auricles and ventricles required. If I desisted from blowing fresh air into the lungs for some little time, the heart would lie still, and yet I could recover it's motion again, by strongly distending the lungs. In this action I never could discern that the heart began the motion, but the auricles always contracted first, and then the heart immediately afterwards ; though, at last, I saw several contractions of the auricles, which were not succeeded by any motion of the heart. I must not forget to mention, that in another dog I saw several *systoles* and *diastoles* of the heart after I had purposely cut asunder both the *vena cava's* ; and could plainly perceive the ventricles to relax and open themselves, when very little or no blood could possibly flow into them. This however is much more easily seen in the heart of a viper, when taken out of the body, which will continue it's *systole* and *diastole* for a long time ; and if it ceases to beat ever so often, and you renew it's motion again and again, by breathing upon it, &c. you may observe that the auricle always contracts before the ventricle, and that the *diastole* is the last motion, though there is no blood to distend the heart. The capacity of the right ventricle of the dog's heart, in the above experiment, as it opened of it's own accord, was something more than a cubic inch, as I found, by pouring in melted wax from a ladle, without any pressure from a perpendicular height, after the auricle and blood-vessels were cut off transversely.

EXPERI-
MENT II.

Taking the heart out of an ox as soon as I possibly could after he was killed, and having cleansed it from the blood, by washing it in warm water, I filled both the ventricles with melted wax, without any distending force more than by pouring it from the ladle. The heat of the wax seemed rather to contract the fibres, and to lessen the capacities of the ventricles ; for some of the wax kept running out from the heart after I had done pouring it from the ladle. The capacity of the right ventricle was equal to 5 cubic inches. The capacity of the left ventricles was not quite 5 cubic inches. From these experiments it manifestly appears, that the last or restitutive motion of the heart is to dilate or open the ventricles, and that without any assistance or impulse from the reflux blood. Dr *Hales* * injected the left ventricle of the heart of an ox from a column of melted wax $4\frac{1}{2}$ feet high, which distended the capacity of it to 12.5 cubic inches. Now supposing the impulse of the reflux blood to be equal to a column of melted wax $4\frac{1}{2}$ feet high, and that in every *diastole*, whilst the ox was alive, the left ventricle was distended to the capacity of 12.5 cubic inches, yet we may observe that the heart, immediately after each *systole*, relaxes and dilates it's ventricles to a certain degree, by it's own mechanism, and to whatever

* *Hæmæstatics*, pag. 25.

capacity the ventricles are afterwards dilated, it must necessarily proceed from the impulse of the blood only; all which is perfectly agreeable to our *third query*, viz. that the *diastole* of the heart may depend upon an abatement of the tension in the contracted fibres, a motion of restitution in such as are over-stretched, and the influx of the blood conjunctly.

I took the heart out of a live viper, and placing it upon a piece of writing paper, I found it beat at the rate of 24 or 25 pulsations in a minute, for 3 or 4 minutes. After this I laid the paper upon the palm of my hand, the warmth of which increased the number of pulses to 37 the first minute, and to 48 the second. Last of all I put it into warm water, a degree or two warmer than the blood, where it beat 87 pulses the first minute, and afterwards declined in it's motion more and more till it quite ceased. May we not from hence observe what vast influence *heat* had upon the nervous *æther* to make it fly so quick into the heart as to increase the pulse to almost 4 times their usual number? How far the heat in some sort of fevers may quicken the pulse, is not to our present purpose to enquire. It is amazing to think that such very small portions of the nerves, as were cut out with the heart in this experiment, should contain matter sufficient for so many contractions; and that it did not all of it instantly fly out from the wounded ends, even before it was put into warm water.

EXPERI-
MENT III.

C H A P. VIII.

M O N S T E R S.

I. **T**HE first notice given to the R. S. of this extraordinary child, was a letter communicated by Mr *Philip Miller*, F. R. S. to the President, inclosing one he had received from the Rev. Mr *Almond*, Minister of the parish; which was read at a meeting of the Society on Nov. 8. last: at which time was likewise read a letter from one Mr *Baily* to *James Theobalds*, Esq; F. R. S. on the same subject, and much to the same purpose.

Some account
of the gigantic
boy at Wil-
lingham near
Cambridge.
N^o. 475.
p. 249. Jan.
&c. 1745.

The following is an extract of the Rev. Mr *Almond's* letter, dated *Willingham*, Oct. 3. 1744.

1. A servant, who married out of my family, was delivered of her second child, a son; who, at his birth, had something very extraordinary about him above other infants; in particular, *in partibus generationis*; besides uncommonly large in his whole body. He grew to admiration for $\frac{3}{4}$ of a year, having only the breast sustenance; when his mother died suddenly; and, as is supposed, he was accessory thereunto, by drawing away her vital nourishment.

Rev. Mr Ed-
mund Al-
mond's letter.
Read Nov. 8.
1744.

Since

Since her death, he has continued growing in proportion: and, although now but 2 years and 11 months old, is 3 feet 9 inches high and better; and every part in proportion thereunto: his strength and courage such as to overcome boys of 6, 7, and 8 years of age: his voice, like a man's, very groom; weight above 4 stone; and he appears to have as much understanding as a boy of 5 or 6 years old. But, what is most surprizing, his *penis* is 4 inches long when erect; the hair on the *pubis* an inch long and thick. The report of him has brought people far and near to see him.

A letter from
Mr Tho.
Dawkes, Sur-
geon at Hun-
tington, to
Dr Mead,
F. R. S. &
Med. Reg.
concerning the
same child;
dated Jan. 4.
1744. Read
Jan. 10.
1744-5.

2. At the instance of the midwife concerned at the time of labour, I went to a village called *Willingham* alias *Wivelingham*, about 6 miles N. of *Cambridge*, to see a boy, then (the latter end of *August* last) just 2 years and 10 months old; of whom I took, with the utmost accuracy, the following dimensions; viz.

| | Feet. | Inches. | Tenths. |
|---|-------|---------|---------|
| His height, which was ————— | 3 | 8 | 5 |
| The measure round his neck | 1 | 1 | 2 |
| ————— of the waist | 1 | 11 | 2 |
| From the head of one <i>humerus</i> to the other | 0 | 11 | 8 |
| From the head of the <i>humerus</i> to the <i>cubit</i> | 0 | 8 | 2 |
| From the <i>cubit</i> to the extremity of the <i>ulna</i> and <i>radius</i> | 0 | 6 | 0 |
| The measure round the wrist | 0 | 6 | 0 |
| The length of the <i>thigh</i> , from the head of the <i>femur</i> } to the head of the <i>tibia</i> | 0 | 10 | 8 |
| The measure round it's thickest part | 1 | 2 | 3 |
| From the head of the <i>tibia</i> to the <i>astragalus</i> | 0 | 9 | 1 |
| The length of the foot | 0 | 6 | 0 |
| The breadth of the same | 0 | 3 | 3 |
| The measure of the thickest part of the calf | 0 | 10 | 3 |
| The length of the <i>penis</i> , as pendent | 0 | 3 | 3 |
| It's circumference | 0 | 2 | 7 |
| The testes, measured round to each side of the <i>penis</i> | 0 | 5 | 2 |

His weight, in his cloaths, was four stone and two pounds.

The hair of his head is long, strong, of a dark-brown colour, and displays itself, naturally, in fine curls.

He has the distinguishing mark of *puberty*; for the *lanugo* upon the *pubes* is as long, as thick, and as crisp, as that of an adult person.

He is very strong: I saw him take up and throw from him, with much facility, a *blacksmith's hammer*, which weighed 17 pounds: and when he is provoked by other children, (for he goes to school) he does not fight with his fist, nor legs, but collars them, and lays them prostrate by mere strength. Thus, they tell me, he will frequently serve boys of 8 or 10 years of age.

His voice, in common, is extremely manly, as deep a bass as you can well conceive; and he pronounces very distinctly. He has something a little

little *savage* in his look ; tho' they tell me, he is not naturally quarrelsome. He eats and drinks with great moderation. His understanding seems to be equal to that of a child of 5 or 6 years old. He is quick of apprehension, and has a very retentive memory. He has something of a *stately walk* with him ; and seems conscious of that uncommon share of strength which the Author of Nature has endowed him with. He is not made a *public shew* of, but walks about, and plays in common with other children. When I saw him, the *glans of the penis* was quite uncovered ; and his aunt, and the midwife, assured me it was always so. His *father* is a *little man*, a *Labourer* : his *mother* (who died when he was but 9 months old ; and, as was supposed, by giving him suck ; for, they tell me, she was found dead with him at her breast) was a woman of a *middle stature*. The midwife assured me, that, when he was first born, he was no other than what they commonly term, a *lusty boy* ; save that the parts of generation were *remarkably large*, and that the *lanugo* first appeared when he was near a year old ; which gave great uneasiness to his parents, who were very religious people.

To a second letter from the same were subjoined the *affidavits* and *testimonials* of the midwife, the Minister, Churchwardens, and others, That this child, *Tho. Hale*, was born on 31 Octob. 1741.

He was assured, by Mr *Almond*, That, between Aug. 28. and Nov. 30. 1744. this child was grown $2\frac{1}{2}$ inches, *i. e.* from 3 feet 8 inches $\frac{5}{10}$ to 3 feet 11 inches. C. M.

II. “ *John Coan*, a dwarf, was born at *Twitshall* in *Norfolk*, in 1728, *Extract of a letter from Mr William Arderon, F. R. S. to Mr Henry Baker, F. R. S. containing an account of a dwarf ; together with a comparison of his dimensions with those of a child under 4 years old ; by David Erskine Baker. N°. 495. p. 467. May &c. 1750. Read June 14. 1750.*

“ and has been shewn in this city for some weeks past. I weighed him myself Apr. 3, 1750, and his weight, with all his cloaths, was no more than 34 pounds. I likewise carefully measured him, and found his height, with his hat, shoes, and wig on, to be 38 inches. His limbs are no bigger than a child of 3 or 4 years old : his body is perfectly strait : the lineaments of his face answerable to his age ; and his brow has some wrinkles in it, when he looks attentively at any thing. He has a good complexion, is of a sprightly temper, discourses readily and pertinently considering his education, and reads and writes *English* well. His speech is a little hollow, tho' not disagreeable ; he can sing tolerably, and amuses the company that come to see him, with mimicking a cock's crowing, which he imitates very exactly. In 1744 he was 36 inches high, and weighed $27\frac{1}{2}$ pounds. His father says, when about a year old he was as large as children of that age usually are, but grew very little and slowly afterwards.”

On receiving the account of this little man, a child of 3 years and not quite 9 months old, son of the late *W. Jones*, Esq; F. R. S. was measured and weighed. This boy, tho' very lively and handsome, is no way remarkable for his size ; and therefore his dimensions and weight,

weight, compared with the dwarf's, may give a tolerable idea of the real smallness of the dwarf.

The weight of the dwarf, with all his cloaths on, was no more than 34 pounds.

The child's weight, with it's cloaths likewise on, was 36 pounds *.

| | | | Inches. | |
|---|---|---|-------------------|-------------------|
| The height of the dwarf, with his shoes, hat and wig on | | | 38 | $\frac{5}{10}$ |
| The height of the child, without any thing on his head | | | 37 | $\frac{7}{10}$ |
| | | | Dwarf. Inches | Child. Inches |
| Round the waist | — | — | 21 | 20 $\frac{5}{10}$ |
| Round the neck | — | — | 9 | 9 $\frac{7}{10}$ |
| Round the calf of the leg | — | — | 8 | 9 |
| Round the ancle | — | — | 6 | 6 |
| Round the wrist | — | — | 4 | 4 $\frac{3}{10}$ |
| Round the thumb | — | — | 2 | 2 $\frac{1}{10}$ |
| Length of the arm, viz. from the shoulder to the wrist | | | 15 | 13 |
| From the elbow to the end of the middle finger | | | 10 $\frac{4}{10}$ | 10 |
| From the wrist to the end of the middle finger | | | 4 | 4 |
| From the knee to the bottom of the heel | | | 10 $\frac{4}{10}$ | 10 $\frac{7}{10}$ |
| Length of the foot with the shoe on | | — | 6 | 6 $\frac{4}{10}$ |
| Length of the face | — | — | 6 | 6 $\frac{2}{10}$ |
| Breadth of the face | — | — | 5 | 4 $\frac{8}{10}$ |
| Length of the nose | — | — | 1 $\frac{2}{10}$ | 1 $\frac{2}{10}$ |
| Width of the mouth | — | — | 1 $\frac{8}{10}$ | 1 $\frac{8}{10}$ |
| Breadth of the hand | — | — | 2 $\frac{5}{10}$ | 2 $\frac{5}{10}$ |

It may not be improper here to take notice, that as the measures of the dwarf were said to be taken with his cloaths on, these of the child were also taken over his cloaths; and they (being a tight stay and petticoat) probably fit closer to his body, and therefore make less difference in the measuring round his waist (the only dimension wherein it could have any effect) than the looser coat or waistcoat of the dwarf.

Description of a monstrous foetus, without any distinction of sex; by Job Baſter, Acad Cæſ. & R. S. Lond. Soc. N^o 495 p. 479. May &c. 1750. Read June 28 1750.

III. A woman in perfect health, the third time of her being with child, about the 7th month of her pregnancy, was delivered of a monstrous child, without any previous fright, or extraordinary impression on the imagination.

The head was not of the natural globular figure, but very much acuminated: the right arm was well formed; but the *radius* and *ulna* of the left seemed to be much shorter. There was not the least appearance of any genital parts, or *anus*, or it's aperture: but instead of that the skin lay in rolls, with the abundance of fat. From the middle of the belly came one foot, ending as it were in one toe, but without a nail.

Fig. 146, 147

* The cloaths, being weighed afterwards by themselves, were 2 pounds 14 ounces; consequently the real weight of the child is 33 pounds 2 ounces; which is but 14 ounces less than the dwarf's weight with all his cloaths.

IV. About

IV. About the middle of *Sept.* last a woman in *Holborn* was delivered with much difficulty of 2 girls joined together by the bellies in so singular a manner, as to deserve a particular description to be laid before you and the *Society*, as a very curious subject. The care of preparing these children for keeping in spirits was committed to Mr *James Sherwood* Surgeon, who was so kind to send for me to observe them with him; and it was resolved to inject them, in order to make our anatomical examination the more accurately, which was very ingeniously executed by Mr *Sherwood*, and the state of the children was as follows:

The skin of part of the breast and belly was continued to each child, from the lower part of the *sternum*, down to the insertion of a single *funis umbilicalis*, which, instead of one to each, serves in common to both. Each child had it's peculiar muscles of the *abdomen*; but the strait muscles were so divided, as that the *rectus* on the right side of the one child had the *linea alba* between it and the *rectus* on the left side of the other, and *vice versa*; so that the line of each lying directly upon the other, was colligated and opened, and the conjunction of the *musculi recti*, thus formed but one common abdominal cavity up to the diaphragms of each child; above which each had it's own proper *thorax*, even evident from their external appearance; whereas, had their junction been but never so little in a lateral way, each would undoubtedly have had it's own separate *abdomen*, since they would not have been so closely pressed forwards, as to occasion that intimate coalescion of parts in the subject before you; which is manifest in the dissections of several of these kinds of monstrosities, some of which have been joined by the hips, some by the backs, some partly by the sides, and one or two cases mentioned by *Parée* and *Tulpius* joined by the bellies.

None of these uncommon subjects ought to be touched with a knife, until it is well injected, because the vascular system, where there are any preternatural adhesions or distortions, can never be understood nor traced without it; and therefore *Tulpius*, whose account of his subject is very inaccurate, and who certainly did not inject it, confesses he could make no distribution of the vessels, nor find out any thing of them distinctly.

But in the present case, a complete injection of the children being made by the vessels of the umbilical cord, we were enabled to give the following exact account of the vascular system and other parts; to which however we shall premise a description of the intestinal canal of both.

When we came to examine the intestines, the only proper means for laying them fairly to view, before they were taken out of the body, was to inflate them; which was accordingly done, and thereby every part of them was rendered as conspicuous as the drawing now before you, and of the same size exactly. Each child had it's own peculiar *oesophagus*, *stomach*, and *pylorus*, in a natural state; from each of which

* See a similar case in these *Trans.* N°. LXV. p. 296.

An account of a preternatural conjunction of two female children; in a letter from James Parsons, M. D. F. R. S. to the President.
N°. 489. p. 527. Oct. & Nov. 1748.
Read Nov. 17. 1748.

the *duodenum* descended about 3 inches, and then united into one common duct, which we shall call the beginning of the *jejunum*, and which was near 4 inches long : this was inserted into the upper part of a large *sacculus*, formed out of the very coats of the intestines, and differing in no wise from them in colour, density, or any other quality but the form and extension.

It's horizontal diameter was about 5 inches, and it's vertical about 4, and it was formed out of the *jejunum*, which, in some subjects, is as long as the *ileum*, in most near that length, and no doubt was an attempt of nature to supply the want of two regular *jejuna* : for we are to observe, that if these children had lived, each having it's own proper stomach, would probably have eaten a due quantity of food for it's sustenance ; and the office of the stomachs might have been well enough performed ; but each requiring a separate system of intestines to dispose naturally of the digested chyle, and this preternatural conjunction happening between them, the *jejuna* of both were confused together ; and having room in the *abdomen*, now large and common to both, these parts of their organizations, that ought to have grown into 2 guts of a considerable length, being hindered from a regular accretion, the joint growing powers of both formed the sack of communication now before you ; which is proportionably capacious enough to answer the purposes of two natural *jejuna* ; below which the rest of the intestines of each child were sufficient to do their several offices.

In the lower part of this *sacculus* there was an outlet on each side, which were the origins of their separate *ilea* : these were in a good state, and regularly inserted each into it's *cæcum* ; and this in each had it's natural *appendicula* ; these were regularly succeeded by their *colons*, and terminated by their proper *recta intestina* to their natural outlets ; with this difference only, that the *colons* were out of their natural situation, and were convoluted in each child, by as narrow portions of the *mesocolon*, as any part of the *ileum* is by it's mesentery ; and that as low as the going off of the *rectum*.

Of the vascular system.

We are to take notice, that as these children had but one abdominal cavity between them, so it contained, in appearance, only one liver of a considerable size, and an irregular form ; but this consisted of two in reality preternaturally joined, as there were two gall-bladders. The umbilical vein is inserted into this, pretty nearly in the usual manner, and afterwards this canal is divided into 2 branches, which carry the blood into the *vena cava* of each child ; whence it falls naturally into the right auricles of their hearts.

The heart of the larger child is but small, has a bifid *apex*, and from the division has a vestige of the *septum*, on both the upper and under sides ; which forms a *fulcus* in a longitudinal direction, from between the *apices* to the *basis* ; from whence arises a *pericardium* which extends itself over each side from the *fulcus*, and so forms a separate *capsula* over each ventricle of this heart, and may therefore be called a double

pericardium.

pericardium. The ascending vessels are distributed according to the standard of nature; but the descending trunk of the *vena cava* rides over that of the *aorta*, above the going off of the emulgents, and sinks back again behind the external *iliac artery*, before it is itself divided into *iliac veins*, descending naturally to the lower extremities, as do the arteries from thence also. The kidneys, urinary and uterine parts were in a natural state; and the lungs appeared well, and seemed as if this child had breathed. The heart of the smaller child was single, but above a third larger than it naturally ought to be; out of which the ascending arteries are very regular and natural; but there was scarce any vestige of lungs in this child on the right side of the *thorax*, and but a small portion of the pulmonary substance in the left. The descending trunk of the *aorta* is very small in comparison of the other; yet goes down regularly towards the extremities, except the internal *iliac arteries*, which were obliterated and degenerated into ligaments, whilst the externals continued down, as I have just said; for only the *iliacs* of the larger *fœtus* took place in the umbilical cord, which was the reason that we found but two arteries in it; so that, although both children received nourishment by the division of the canal from the liver to the *venæ cavæ*, yet the superfluous blood of both could be sent back to the *placenta* no other way than by the internal *iliacs* of the greater child.

The descending branches of the *vena cava* entered as usual, on the right side, into the auricles; but those of the left join in one trunk, pass round the left auricle, and enter into the right close by the *cava ascendens*, which is of a natural size, and very regular up to the diaphragm, from which it extends a full inch before it reaches the auricle; the kidneys differ a little in size from each other; yet these, with the other urinary and also the uterine parts in general, are in good order; but the most remarkable *lusus* of nature in these subjects is an artery which arises from the *aorta* about the place of the *celiac* of the one child, running along before the liver, and is inserted into the same place of the *aorta* of the other. It was much larger than any other artery in either child, and bestowed branches on the *stomach*, *mesentery*, and *mesocolon*; being about 5 inches long; for there were neither *cæliac* nor *mesenteric* arteries, according to nature in either child.

When some former authors (before the Learned were so happy as to know the sweets of Experimental Philosophy) endeavoured to account for monstrous productions in the animal world, they could have recourse to no other means to explain them, than to the then reigning systems by which they usually explained the *phænomena* of the natural generation of animals; and that was in general, that an animal was produced by the admixture of the supposed seminal matter of both the parents; that the quantity produced by this commixture was supposed always to contain only a quantity of particles sufficient to produce a natural *fœtus*, by the mediation of a certain *plastic* power, said always to

Observations.

attend it, as well as any other natural production in the world. Upon this plan many little alterations were made by succeeding authors, without differing widely from this general notion ; all as liable to objections, and as easily refuted as the source from which they sprung. And notwithstanding the truths that have since been traced out by later philosophical advances, leading to a more reasonable knowledge of the subject, yet there still are some who appear unwilling to quit these old errors. But as this is not the place intended for a discussion of the several opinions concerning it, we shall here only consider how the conjunction of these 2 children happened ; as well as the means whereby children acquire superfluous or want the necessary members, or are any otherwise deformed.

According to a late reading I had the honour to exhibit before you, upon the analogy between the propagation of animals and vegetables, I hope it appeared pretty clearly, that both these parts of the Creation are daily propagated from organizations already formed and treasured up in natural receptacles provided for them, till they come to be removed into proper places of nourishment ; from which opinion we can find no reason to swerve at present, and which we must have recourse to, in order to account for the present subject. If the old systems had weight formerly, later enquiries have exposed their absurdities. Truth admits of nothing absurd, and as to what regards the works of the Creation, especially that of animals and vegetables, that system of accounting for their generation which is most simple, and is least liable to objections, is most likely to succeed in the enquiry ; and since the ordination of Providence was that all should be good, it will appear the means of the propagation of animals and vegetables, which was partly the subject of the discourse lately read before you, will seem least of all liable to any accidents that might degrade the general usage and standard of nature, by the production of monstrosities in any part of her works.

When the *vis plastica* was thought to be the agent for the guidance of the work of generation, and that a monstrous child appeared, it was blamed by the authors of most nations in some of these particulars ; the seminal matter was either in too small or too great a quantity, and the *vis plastica* puzzled in the management of the particles that go to form the different parts of the animal ; or it was sometimes careless and negligent in the application of the ideas of some or other of the parts, and consequently the animal must want some limb or other ; or, from a superabundance of matter, have superfluous limbs added to them by this unskilful erring agent : but in whatsoever manner the commixtion of the male *semen*, with that ignorantly supposed to be in females, and the formation of a *fœtus* therefrom, is said to be conducted, the accidents and chances against the welfare of all animal beings would be so numerous, and the state of nature so miserable, that the greatest part of the inhabitants of the earth and waters could not avoid being monstrous

strous, and full of confusion: the Almighty would have produced an effect contrary to His Divine Goodness, and care for his creatures; and, in fine, it would be highly absurd, to suppose the regulation of things of this high consequence to be committed to any finite subordinate ignorant agent, which must undeniably be insufficient for this great work.

But the system of generation which supposes the organizations of animals and vegetables already formed with an incapacity of growing into any other forms than those of their parents, is the most secure from any confusion, or any preternatural digressions from their due forms, is most compatible with that gracious design, that all should be good: for every animal and vegetable body is daily seen to be constant to its own kind, and can be subject to no accident but one to render it monstrous in its accretion, and that in general is compression, for all animal and vegetable *ova* are most certainly perfect in their first formation, as the seeds of the latter plainly shew, and in a state of rest, until they are deposited in their natural *matrix*, be it sooner or later; the *ova* of females in the *ovaria* during life, and the seeds of vegetables in our repositories for any number of years; and, after that, would certainly, without accidents or interruption, continue their perfect form to their utmost growth.

From hence it will be easy to account for the preternatural adhesion of these two children, and the confusion of their *viscera*, upon the most easy plan, and most simple and persuasive reasoning imaginable; and from hence also we shall be able to account for every other monstrosity that can attend animals and vegetables.

We have observed before, that each seed and *ovum* contains the animal and vegetable proper to its species. Now, when two or more of these animal *ova* are fecundated, and come into the *uterus*, the sides of the *ova* (which are the membranes that contain the fluids in which the little organizations swim) must inevitably come into contact; and if the membranes of each continue in a good state, the *fætus's* will be free in their several apartments, and grow proportionably; but if the parts of the membranes, which are close together, by being thin and weak, or by any irregular resistance, or friction, come to be dissolved or broken, then the fluids of both unite, and the two little organizations, having no longer a partition between them, come together, adhere, and intertwine into each other, their tender parts easily coalescing; and from the natural disposition of each to grow and increase, their accretion goes on, there is a mutual insinuation of vessels where the parts are compressed, and a mutual confused circulation carried on, and at length the whole becomes irregular and monstrous.

We have many facts to corroborate this opinion, and to shew that the fibres of animals and vegetables have a wonderful capacity of extending and insinuating themselves into one another; and of continuing a circulation reciprocally; and the blood-vessels of being elongated, and even of producing new ramifications where the restoration of a part requires.

requires it, from the principles I before laid down in the first part of my Analogy ; else how should wenny tumours of a monstrous size be propagated on the surfaces of human bodies ? how should the lungs adhere to the *pleura* so intimately as to become one entirely united mass, as incapable of being separated without a knife as any part of a muscle ? How should the *surculi* or *buds* of trees implanted into others by grafting or inoculation, so insinuate their fibres into those of the stock in which they grow, as to become one continued piece of wood with them ? How are the *sutures* of the *skull*, and those of the *epiphyses* of bones, totally obliterated in an advanced age, but by the insinuation and reciprocal combination of the fibres of each other ? And, in a word, how are many recent deep wounds so soon agglutinated, if there be not a speedy insinuation of vessels, and a circulation soon carried on ?

Indeed if we are only to look on, and consider the subject before us in it's present state, it will be somewhat difficult to conceive how this strange conjunction could happen ; but we are to go back, and consider two minute tender organizations, whose remotest parts from each other might not exceed perhaps the fiftieth part of an inch at the time of their adhesion, and the difficulty is taken away. Thus our children happening to be compressed by their bellies, the tender integument between the *musculi recti* in each were soon thinned and dissolved, the coalescion happened as I have said before, and the intrusion and commixtion of parts, that appear before you, was begun and carried on by their growing wheresoever the resistance was least.

And thus if the contact and pressure of the two *fetus's* be pretty equal and moderate, they will grow equally ; if the compression be very great, and both be compressed all round, having no room, because of the *uterus* not giving way, the confusion of both, or indeed of one only, will be so great as to cause a mass without any form or regularity at all : if their contact be so disposed, as that one *fetus* is much compressed and confined, the other has sufficient room, this will grow proportionably, whilst the growth of the other being intirely stopped from the beginning in it's minute state, except perhaps an arm, or leg, or head, &c. that which has liberty will have such superfluous limb or part growing with it, as remained uncompressed, whilst the rest is obliterated and lost.

The same is also apparent in vegetables : a carrot, parsnip, radish, and such-like vegetables as naturally grow strait and well-formed, may be distorted and altered at pleasure, as they grow, by compression : for, as the nutritious juices are equally distributed, and attracted in the same quantity for the use of the whole ; if a compression be made on any part, those juices, which are hindered to flow into the compressed part, will be determined elsewhere, and form gibbosities and deformities in other parts of the organization, where the resistance is less, and the whole become changed from it's natural form. Thus *gourds*, as they grow, in applying pressure by ligatures, or otherwise, may be brought to various forms ; and *apples*, placed in cylindrical *phials*, whilst small, will, by the

the lateral pressure, lose their roundish form, and acquire that of a cylinder. And thus nuts, apples, &c. may be conjoined and become double; and not because there was a superfluity of a supposed matter to form them, by any subordinate help: and thus also a nut or apple, &c. among a bunch of sound ones, may, by compression of it's organization, be vitiated and ill-formed; and not because there wanted a sufficiency of this supposed matter.

In this manner, it is highly probable, all animal monstrosities happen; and this was the case of the *cow*, which many of us saw a fortnight ago. Her twin sister happened to be confined and compressed to her spine; nothing remaining free but the *abdomen*, the *dugs*, one of the anterior extremities, and the dew-lap; every other part was obliterated, whilst these continued to grow, by the communication of vessels between them, in the manner above explained.

Fig. 148. represents 2 female children preternaturally joined by the abdominal integuments, from the *umbilicus* up to the *cartilago ensiformis*, in such a manner, as to form between them but one *abdomen*. An explanation of the drawings representing the above described two children joined together.

Fig. 149. shews a back view of the intestinal canal of each child, from the stomach to the *anus*. Fig. 148.
Fig. 149.

a, a, the stomach of each. *b, b*, the *duodenum*. *c, c*, part of the *jejunum*, which is common to both children. *d*, the remaining part of the *jejunum* formed into a *sacculus*, out of which, *e, e*, the *ileum* of each child arises. *f, f*, the *cæcum* of each. *g, g*, the *colon* of each. *h, h*, the *rectum* of each.

Fig. 150. is a fore view of the other *viscera*, and vascular system of both children. *a*, the umbilical vein entering into the liver, which is formed of that of each child preternaturally conjoined. *b, b*, the lungs of the larger child. *c*, the heart, which has a bifid apex, and of which each ventricle has it's particular *pericardium*, from the *sulcus* that divided it. *f*, the great vessels arising out of the heart. *g, g*, the kidneys in some measure misshapen. *h, h*, the trunks of the large vessels descending to the lower extremities. The vein running before the artery, and sinking behind it again where it divides. *i*, the right ureter. *k*, the bladder; and *l*, the umbilical arteries, with the *urachus* turned down, to shew *m*, the *uterus*, &c. *m, n*, is an artery communicating with, and entering into, the *aorta* of each, near the going off of the *emulgents*. *o*, the heart of the smaller child, much too large in proportion, together with the right auricle. *p*, part of the lungs, which were rendered much too small, in proportion, by the compression of the large heart upon them. *q*, the *aorta* and pulmonary artery, as they are connected by the *canalis arteriosus* (*r*). *s, t*, the descending trunks of the veins; the latter of which was preternatural, running round the left, and entering into the right auricle in it's posterior part. The other parts were much as those of the former child in general; except the *aorta*, which was much smaller, as the figure shews.

An account of
double *fœtus's*
of calves; by
Monf le Cat,
M. D. F. R. S.
&c dated
Rouen, Aug
20 1748.
Translated
from the
French by
T. S. M. D.
F. R. S. Ibid.
p. 497. Read
Oct. 27. 1748.

V. I have, since *Jan. 1735.* been in possession of a child, born in our city of *Rouen*, which has 2 heads, 4 arms, 4 lower extremities, and 2 trunks united, and as it were blended together. About that time I published * a description of the internal parts of this monster, which had but one heart; but I did not cause draughts to be taken of those parts: and it would now be a difficult matter to have them drawn so as to exhibit a good representation of the state in which they then were. This negligence, through which I am deprived of those curious and instructive figures, which this monstrous birth would have afforded, made me wish for a like opportunity, in some measure at least to make amends for that fault. This opportunity presented itself in *January 1748.* not in a human *fœtus*, but in a calf, which the butchers of our hospital cut out of a cow.

The description which I shall give of this monster, will be the explanation of the figures that represent it.

- Fig. 151. *Fig. 151.* the outward surface of the double calf about $\frac{1}{4}$ of the natural size.
- Fig. 152. *Fig. 152.* the integuments of the breast being raised, there appears the union and reciprocal insertion of the pectoral muscles of each subject into one common *linea alba*. None but the inmost plans were attached to the bones.
- Fig. 153. *Fig. 153.* the muscles being removed, one *sternum*, common to both subjects, appears in sight. There was a *sternum* intirely similar to this, on the other or opposite side.
- Fig. 154. *Fig. 154.* *A*, the apex of the heart common to both. *B*, the right auricle of the subject *B*. *C*, the inferior *aorta*. *D*, the superior *aorta* of the subject *B*, from which issue the right subclavian †, and the carotids **. *E*, the pulmonary artery of the same subject *B*. *P*, it's lungs. *FF*, the superior *venæ cavæ* of both subjects. *ff*, their inferior *venæ cavæ*. *GG*, the *thymus*, or throat sweet-breads. α , of subject *A*, a trunk formed by the re-union of the carotids **, and the subclavians †; which trunk commonly constitutes the superior *aorta*, as appears in the subject *B*: but in this subject *A*, it only sends a small *canalis arteriosus*, γ , into the inferior *aorta* *C*. β , a thick common trunk of the pulmonary artery and the inferior *aorta*. The latter plainly appears a continuation of this trunk; whereas it is commonly a continuation of the *aorta*, α , *D*: and the pulmonary artery, β , *E*, only furnishes the *aorta*, α , γ , *C*, which makes but one canal in ordinary subjects, with a *canalis arteriosus*, or canal of communication (See *Fig. 155.* in the subject *B*). And indeed I am of opinion, that this structure, which seems extraordinary, is natural to every *fœtus* that is not far advanced, as I explain it in my course of Physiology under the article of the *Fœtus*; and that it is a consequence and proof of the mechanical and successive formation of the

* *Journal de Verdun*, for March 1735. p. 194.

Fig. 148.



Fig. 150.



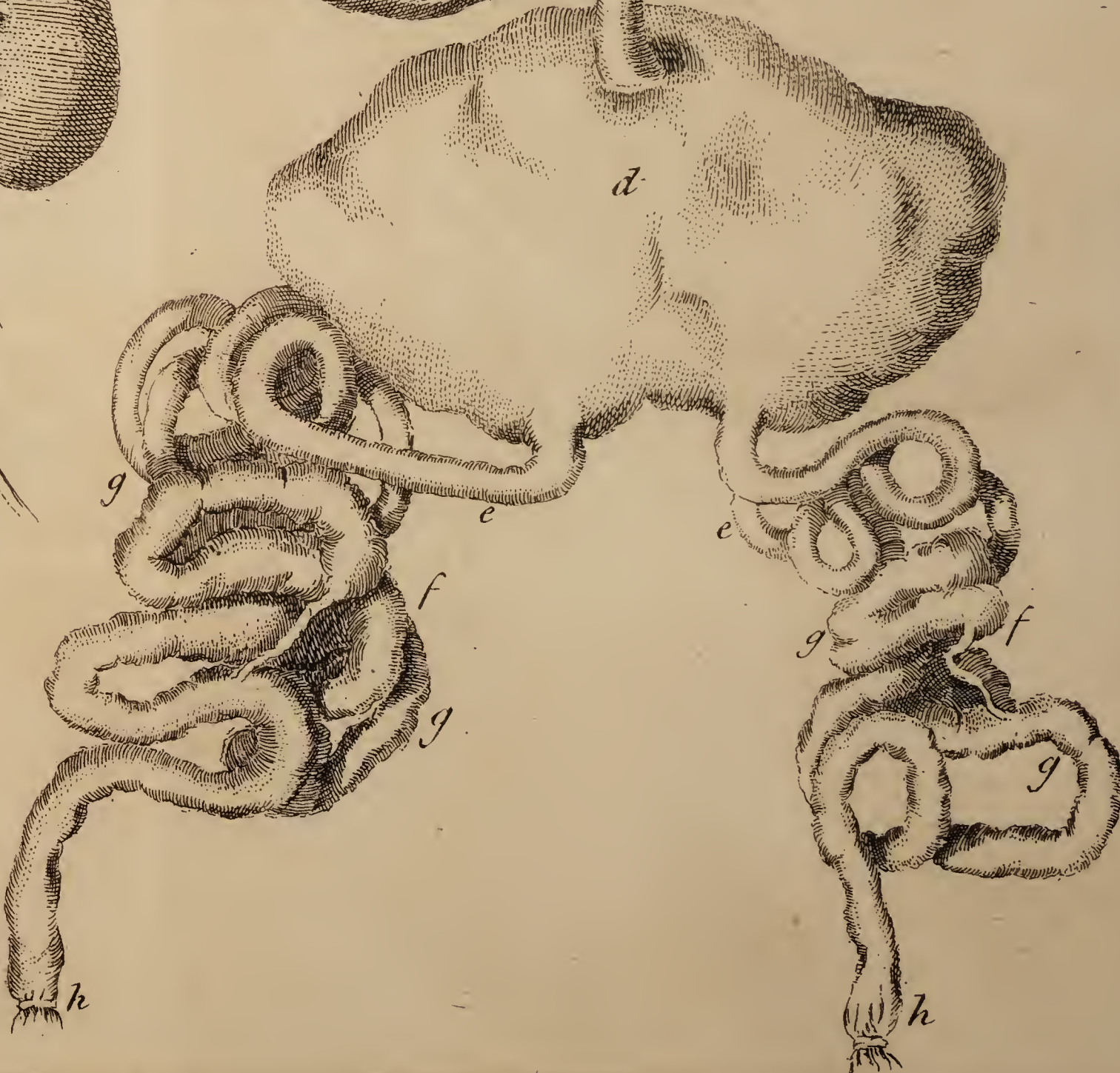
Fig. 149.



Fig. 147.



Fig. 146.



organs of it's circulation, which begins by the lower circle made by the umbilical vein, as the first mover ; the trunk of the *vena cava*, the inferior *aorta*, and the branches of the *vena cava*, which correspond with it. Now the subject *A* had several marks, which demonstrated that it's formation was less advanced than that of the subject *B*. *a, a, a*, the *oesophagus* of each subject. *TT*, the windpipe. *R*, the spleen, *S*, the stomach of the subject *A*. *V*, the liver, which seems to belong to the subject *B*. Under them the great and small intestines. *K*, the umbilical vein of the subject *A*, through which having made an injection, the whole liver was injected, but the heart and vessels were not : so that it is probable this vein was distributed no farther than the liver, and had no *ductus venosus* that passed to the *cava* and heart. *L*, the umbilical vein of the subject *B*, which received a large branch of the umbilical vein of the subject *A* : and which branch seemed to supply the place of the venal duct, that was wanting. Having thrown in the injection through this vein *L*, the heart and vessels of the two subjects were injected. *MM*, the orifices of the umbilical arteries, which were but two in number, one for each subject ; the one and the other issuing from the right iliac of each subject. *N*, the openings of the *urachi*, which were very large.

Fig. 155. the other side or view of this monster, wherein the subject *A* Fig. 155. is to the right ; the subject *B* to the left.

a, the *thymus* of each subject. *b*, the right auricle of the subject *A*. *c*, the left auricle of the subject *B*. *d*, the superior *aorta* of the same. *e*, it's pulmonary artery. *f*, the *canalis arteriosus*, which here had the same structure that all fœtus's of 9 months or more usually have. *g*, it's inferior *aorta*. *h h*, the inferior *venæ cavæ* of each subject. *i*, the *azygos* of the subject *B*. *k*, it's *oesophagus*. *l*, it's stomach. *m*, it's spleen. *n n*, part of the lungs of each subject. *o*, the liver of the subject *A*.

Fig. 156. represents the heart, which is common to the 2 subjects, laid Fig. 156. open transversally. The anterior part is lifted up, to shew the cavities and the orifices of the vessels of it's base. The figure is of about half the natural size. This heart had only two cavities, *D, E*, as usual ; but the right cavity or ventricle *D* belonged to the subject *B* ; and the left ventricle *E* to the subject *A*. Into each of the cavities *D E* there opened four orifices ; viz. two arterial, which were those of the pulmonary arteries, *a, a*, and of the *aorta*'s, *b b* ; and two venal orifices, or those of the right and left auricles, for the blood of the *cavæ*, *c, c*, and of the pulmonary veins, *d, d*. I give the name of *aorta* to the superior arterial trunk *b*, of the subject *A*, in conformity with the usual appellations, and because in common subjects this trunk alone deserves that name ; although in this case the pulmonary artery *a* visibly constitutes the principal part of the inferior *aorta*, *c*.

f f, the inferior *cavæ*. *g g*, the superior *cavæ*. *h*, part of the branches

of the pulmonary artery. *k*, the valve of the orifice of the right auricle in the subject *B*.

Concerning a
wether giving
suck to a
lamb; and
of a mon-
strous lamb;
in a letter
from the Rev.
Dr Doddridge
to Henry Ba-
ker, F. R. S.
Ibid. p. 502.
Dated Nor-
thampton,
July 2, 1748.
Read Oct. 27.
1748.

VI. The occasion of my writing to you now is to inform you of a remarkable fact, which I have just heard from a member of the church of which I am Pastor, and in whom I can intirely confide. He tells me, that he has in *Upper Heyford Field*, about 4 miles from this town, a wether-sheep which now suckles a lamb. I know not by what accident, the lamb sometime since ran after it, and fixed upon it's paps; drawing hard, milk followed. The lamb has subsisted very well upon what it sucked from him, and at the late shearing time he himself pressed the teats, and milk came out in a considerable quantity.

This reminds me of what Mr *Ray* tells us from *Boccone*, that a countryman in *Umbria* nourished his child by his own milk, and *Florentinus* and *Malpighius* are quoted on the same occasion. *Bartholinus*, in his *Anatomy*, p. 215. has some remarkable passages to this purpose: he quotes a passage in *Aristotle* concerning a he-goat in *Lemnos*, who had a great quantity of milk.

I shall add to this a short account of a monstrous lamb, which was yeaned in a field near *Newport Pagnel* about the middle of last *March*, and was brought to me soon after it died. It had 2 perfect heads (and 2 long necks), each as large as that of a common lamb, but sucked only with that on the right side. So far as I could learn the organs of both were compleat. It walked only on 4 legs, but had a fifth hanging down between the 2 necks, rather longer than the other four; the bones and hoof were double, and had 4 claws: the concave side of it was turned upwards, and whenever the creature walked this leg moved up and down as it seemed spontaneously, and in a manner answerable to the motion of the other four: it had two tails, but no vent behind: it had also two distinct spines, but they met about 5 inches above the tail, and then divided again; but where they met they were not as one intire spine, but as two adhering to each other. There were two sets of ribs, only those which met upward (where the spine should regularly have been placed) were rather shorter than the other: and it seemed that the blade-bone belonging to the doubled leg that grew between the necks was larger than the rest, and seemed to be two bones, but not intirely distinct: it had two hearts of equal bigness, lying over each other almost like a *St Andrew's* cross, or (as we should say in heraldry) saltire-wise. There were 2 *oesophagi* and 2 *asperæ arteriæ*: 4 small lobes of lüungs, but the 2 gullets were inserted into one common stomach. I am not master enough of the formation of ruminating animals to inform you farther what was peculiar in this. We found nothing preternatural in the formation of the intestines, but the tails grew so near, that the return of both seemed to point to one vent, tho' as I observed, the *anus* was deficient. It had three kidneys, one of them very large in proportion to the other two; so that I apprehend there was a conjunc-
tion



tion. I send you this account whilst the matter is fresh in my memory.

CHAP. IX.

Period of Human Life.

| An. Dom. | Born | Males | Females | Baptized | Buried | An abstract of the bills of mortality in Bridge-Town in Barbadoes for the years 1737—1744; communicated by the Rev Mr John Clark. N ^o . 487. p. 345. Apr. &c. 1748. Read May 26. 1748. |
|----------|------|-------|---------|----------|--------|---|
| 1737 | 52 | 26 | 26 | 77 | 208 | |
| 1738 | 81 | 41 | 40 | 106 | 250 | |
| 1739 | 91 | 54 | 37 | 119 | 244 | |
| 1740 | 91 | 49 | 42 | 123 | 242 | |
| 1741 | 68 | 33 | 35 | 95 | 261 | |
| 1742 | 87 | 42 | 45 | 130 | 296 | |
| 1743 | 92 | 43 | 49 | 126 | 252 | |
| 1744 | 89 | 46 | 43 | 120 | 166 | |
| | 651 | 334 | 317 | 896 | 1919 | |

CHAP. X.

PHARMACY and CHYMISTRY.

I. **I**T does not appear very plain, from any thing that has occurred to me in the writings of the elder Greek Physicians, that they were much, if at all, acquainted with any substance that now goes under the name of *manna*. They had the term, but applied it to a subject very different from what we do at present. The ὑπόσεισμα τῆς Λιβανωτῆς, or the *mica thuris concussu elisa*, the bits broken off from the *olibanum* in carriage, was the substance they knew by that name.

If the *Arabians* did not first of all introduce some kind of purging *manna* into practice, they at least rendered the use of this drug more common and extensive. Their country afforded several species of it; which being familiarly known, is, perhaps, the reason why no descriptions have been left sufficiently clear, whereby to distinguish them from each other. Whoever has consulted the *Arabian* writers, or the best of their commentators, will allow that their accounts are defective. That they had 3 sorts of this drug, distinguished by the names of *manna*, *tereniabin*,

tereniabin, and *siracost*, is certain; but whether these are now known, or by what appellations, has been very much disputed.

Rauwolf, in his Itinerary published by *Ray*, and *Tournefort*, in his Voyage to the *Levant*, have given the clearest intimations of any writers that I know of: if to these we add *Clusius*, we have all that we are to expect of certainty, amongst those who have mentioned it; they being eye-witnesses of what they wrote: yet the descriptions of these are either so imperfect or unknown, that a very late writer upon the *Materia Medica* *, either not having seen or understood them, has fallen into a mistake about the *Manna Arabum*, and his authority may perhaps mislead others.

My worthy friend *Peter Collinson*, having procured a sort of *manna* from a Gentleman at *Petersburgh*, under the title of *Manna Persicum ex planta Al-Hagi Maurorum*, was pleased to favour me with a specimen of it: I consulted the principal writers on the *Materia Medica*; and, finding their opinions to be frequently opposite, and their accounts in general perplexed, I imagined it would not be unacceptable to the curious, to have such a description of this species, as would probably make it clear, that we have one kind of the *manna Arabum* still extant, viz. the *terniabin*, and prevent any future mistakes about it.

The specimen of *manna* that now lies before me, appears, at first sight, to be a dirty reddish-brown coloured mixed mass; which, upon a nearer view, is found to consist of,

1. A great number of globular, crystalline, almost pellucid bodies, of a yellowish white colour, and different sizes; the biggest not much exceeding a large coriander-seed, or a very small pea: they differ from grains of mastich, in being more upon the reddish cast; but in figure and transparency it varies not much.
2. Some small sticks like prickles, and others like footstalks of leaves or fruit.
3. A few narrow-pointed firm small leaves.
4. A large quantity of long reddish-coloured pods, of a sweetish gelatinous taste, containing from 1 to 6 or 7 hard, irregular, somewhat kidney-like seeds, which to the taste are very acerb. And,
5. Some sand and earth. Four ounces of the *manna*, dissolved in warm water, left 3i or something more of these in the filtre.

The globules (N^o. 1.) are hard, and break between the teeth like sugar-candy; they are of a pleasant sweet taste, with much less of the *manna* relish than the *Calabrian*; but with enough to discover to what family this substance belongs. The sticks, leaves, pods, &c. seem to be parts of the plant that produces the *manna*. Some of the seeds have been sown, and proved so fresh as to afford some plants of the *albagi*.

About 1537, when *Rauwolf* wrote his Itinerary, it appears, that large quantities of this kind of *manna* were brought from *Persia* to *Aleppo*,

* Geoffroy *Traictat. de Mat. Med.* Vol. II. p. 586, &c.

where it was then known under the name of *trunſchibil* or *trunſchibin*; a corruption, doubtleſs, of the antient *terenjabin*; or, as it ought to be written according to *Deuſingius* (a), *terengjabin*. *Rauwolf* informs us, that this ſpecies of *manna* was gathered from the *albagi*; a plant which is minutely deſcribed by *Tournefort* (b), who alſo confirms the account which *Rauwolf* had long before given, with the following particulars:

“ It is chiefly (ſays he) about *Tauris*, a city in *Persia*, that it is gathered, under the name of *trungibin* or *terenjabin*, mentioned by *Avicenna* and *Serapion*: thoſe authors thought it fell upon certain prickly ſhrubs; whereas it is only the nutritious juice of the plant.” He adds, “ That, during the great heats, you perceive ſmall drops of honey upon the leaves and branches of theſe ſhrubs; theſe drops harden, in grains about the bigneſs of coriander-ſeeds: they gather thoſe of the *albagi*, and make them into reddiſh cakes full of duſt and leaves, which alter the colour, and leſſen it’s virtue. This *manna* is much inferior to the *Italian*. The ordinary doſe is from 25 to 30 drams.”

Cluſius (c) informs us, that the *terniabin* of the modern *Arabs* is gathered from a prickly ſhrub, ſuch as the *albagi* is deſcribed to be. *Avicenna* (d), according to his preſent tranſlation, tells us, that the *tereniabin* falls *ſuper lapides*; but (e) *Deuſingius* ſays that it ought to be read, *ſuper albagi*; and that his tranſlators were led into this miſtake, from the reſemblance betwixt *al-bhagier* (the word in the *Arabic* text, which ſignifies a kind of thorny plant, ſuch as the *albagi* is ſaid to be) to *al-bagio*. It is therefore evident, that the *Manna Perficum*, now before us, is the *tereniabin*, *terenjabin*, *terræjenbin*, or more properly, the *terengjabin*, of the old *Arabians*, and of *Cluſius*; the *trungibin*, or *trunſchibil*, of the later, of *Rauwolf* and *Tournefort*; very probably, the *manna maſtickina orientalis* of *Matthiolum* and *Baubine*; as it is the *maſtickina* and *albagina* of *Geoffroy*; tho’ this author makes the *tereniabin* a ſpecies of *liquid manna* (f), in complaiſance to his countryman *Bellonius*; who, tho’ in general a diligent obſerver, yet, in this caſe, was miſled by the *caloyers*, or monks of *Mount Sinai*.

Bellonius ſays, in his obſervations (g), and more largely in his treatiſe *De Arboribus perpetua fronde virentibus*, that theſe *caloyers* collect a kind of liquid *manna*, which they call *tereniabin*; that this ſpecies was known in the ſhops at *Cairo* by the ſame name; and that this is the *mel roſcidum* of *Galen*, and the *mel cedrinum* of *Hippocrates*.

I think it is very plain, that *Bellonius* was miſtaken in the firſt part of his aſſertion, from what has already been advanced. The *caloyers* told him, that they called it *tereniabin*; and he takes it for granted,

(a) *Deuſingius Tract. de Manna & Sacch.* p. 11.

Levant, Vol. I. p. 247, 248.

cen. Oper. Tom. I. p. 404.

Mat. Med. Tom. II. p. 587.

(b) *Tournefort's Voyage to the*

(c) *Cluſ. Exotic.* Vol. II. p. 164.

(e) *Tract. de Manna*, p. 19.

(g) *Bellonii Obſerv. apud Cluſ.* p. 129.

(d) *Avi-*

(f) *Tract. de*

that it was the *tereniabin* of the old *Arabians*, contrary to their own accounts, supported by the testimony of their successors, who are known to vary as little as any people whatever from the traditions and customs of their predecessors, and still retain a great many of their appellations. For proof of this, I need only refer to the accounts which *Rauwolf*, *Tournefort*, and *Shaw*, give us of those people.

That this liquid *manna* was the *δροσόμελι*, seu *αερόμελι*, of *Galen* (a), and the *μέλι κέδρινον*, of *Hippocrates* (b), (supposing there is no mistake in the text) seems very probable. The description which *Galen* has left of the *mel roscidum*, and the manner of collecting it on *Mount Sinai* in his time, tallies exactly with *Bellonius*'s account; and thus far, I believe, all authors agree: but that the virtues of *manna* were known so early as in the times of these 2 authors, will be difficult to prove.

Galen takes notice of this *mel roscidum* more as a curiosity, than a medicine. He no-where, that I know of, mentions it's use, or describes it's qualities: he introduces the account of it with a *memini aliquando*, and says, that the *mel roscidum* was rarely met with in his country, but was gathered at *Mount Sinai* every year: and, indeed, from the manner in which it is spoken of by an old *Greek* writer in *Athæneus*, as cited by *Salmasius*, it would seem, that it was only used for pleasure, as an agreeable sweet, *melle ipso suavius*; and, probably, continued to be of no other use. *Mesue* tells us (c), that *Galen* mixed *manna* with *scammony*. In the spurious piece *de Dynamis* ascribed to *Galen*, *scammony* is ordered to be mixed with honey; but he never once mentions *manna* in any of his extant writings. As *Galen* is known to be very minute in his account of the *Materia Medica* of that time, his silence is a strong argument against the supposition, that even the *mel roscidum* was in pharmaceutical use, much less any other species of *manna*.

If *Galen* was unacquainted with this substance, it is very probable, that *Hippocrates* was so likewise; since a drug that must have made a considerable figure in his *Materia Medica*, would not have soon been struck off the list, or dropped into oblivion and disuse.

But how shall we get rid of the *μέλι κέδρινον*; the name sufficiently intimating what substance was intended? Perhaps *Foesius*'s suggestion may help us. He thinks, that the words might have been read with a comma intervening, whereby we should have had 2 distinct well-known substances, honey, and resin of the cedar; two simples that were then, and continued long after, in familiar use; instead of one, which he mentions no-where else, and seems to be unknown some ages after.

Upon the whole, I have not hitherto met with evidence sufficient to induce me to believe, that either the *mel roscidum*, or any kind of *manna*, was in common medical use either with *Hippocrates* or *Galen*. *Actuarius*

(a) *Galen de Alem. Facult* L. III. c. 39.
p. 876. Edit. Foesii.

(c) *Mesue de Simpl.* c. 8.

(b) *Hippocrat de Ulceribus*,

mentions it once *, and, as I know of, only once: he makes it a purgative, and to be somewhat stronger than *cassia*.

It is now pretty generally known, that the *manna's* in use are not a *mel aërium*, or honey-dew, as was long believed, but a *succus proprius* issuing out of some particular trees, at proper seasons, and in some climates only; and that, during the summer's heats, a great number of vegetables, in almost all the temperate countries, afford a juice somewhat akin to *manna*, from whence the bee collects and prepares her honey. It may not, however, be amiss, nor very foreign to our subject, to exhibit a short account, how the *manna officinarum* is collected.

In *Calabria* and *Sicily*, in the hottest part of the summer months, the *manna* ouzes out of the leaves, and from the bark of the trunk, and larger branches, of the *fraxinus*, or *Calabrian* ash. The *Ornus* likewise affords it, but from the trunk and larger branches only, and that chiefly from artificial apertures; whereas it flows from the *fraxinus* thro' every little cranny, and bursts through the large pores spontaneously.

What is got from different parts of the tree acquires different names; the trunks generally afford those large white pieces to which we give the name of flaky; but the finest of all is such as is collected from artificial incisions, in which little straws, &c. are purposely placed in such a manner, as that the flowing juice may concrete upon them, and form those long, white, cylindrical, perforated pieces, which are so much valued.

This juice is secreted in the largest quantity, betwixt noon and evening. In the night it is condensed, if the season is dry, otherwise the *manna* is spoiled: they scrape off the small with wooden knives, early in the morning, and gather the larger flakes; both which are afterwards dried upon clean paper in the sun, till they stick no longer to the fingers; and the different sorts are then carefully packed up for use and exportation.

II. Upon hearing lately part of *Don Antonio de Ulloa's* † letter to you, I was suspicious *M. de la Condamine* had taken some facts there

* *Actuar. Method. Medend.* L. V. c. 8.

† This Gentleman was one of those sent by the King of *Spain* to attend and assist the *French Astronomers* of the *R. Acad. of Sciences*, in their late measure of a degree of latitude near the equator. He was taken prisoner at *Cape Breton* in his return home, and brought into *England*, where his papers all relating to the measure of the degree, and other astronomical and philosophical observations, were by the favour of the Lords Commissioners of the Admiralty restored to him, to be published in his own country. An abstract of the same was however by their Lordship's leave communicated to the *R. S. Society*. by their *Pres.* who was entrusted with the perusal of the same: and the author himself, who is a Gentleman of great merit, was soon after unanimously chosen a Fellow of the *Society*. Some time after his return he procured the above-mentioned specimen of this *Indian poison*, which he sent over together with some books, as a present to his friend the *President* of the *Society*.

Concerning the Indian poison, sent over by Don Ant. de Ulloa of Seville, F. R. S. and mentioned by M. de la Condamine, Member of the R. Acad. of Sc. at Paris, (in his account of the river of the Amazons in S. America) in a

letter from
Richard
Brocklesby,
M.D. F. R. S.
to the Pref.
N^o. 482. p.
408. Jan. &
Feb. 1747.
Read Feb. 5.
1746-7.

upon the authority of others, or else had been himself a little too much addicted to that general bias of mankind, the love of prodigy and wonder.

In order to be better satisfied, I dissolved, in a certain quantity of fair water, as much of the *Indian* poison as could be suspended, and let it stand to clear 24 hours; and, having made a superficial incision with a lancet into the nose of a young cat, a few drops were sprinkled on the wound. The creature at first discovered no marks of injury received; yet in $\frac{1}{2}$ an hour she seemed, by mewling more than before, to be sensible of some pain. Thus she remained about 20'; when at length she shivered, was sleepy, soon became convulsed, and, in about $\frac{1}{2}$ an hour, her limbs were flaccid, and her belly swelled. These symptoms continued, till she in a short time expired.

Some time passed, e'er I sat down to inquire what visible effects had been produced on the body. I then separated the head from it's trunk, and carefully examined the brain, and particularly the origin of the nerves; but when I had considered it thoroughly, I could not discover any preternatural appearance in any of these parts. Having spent near $\frac{1}{2}$ an hour in this inquiry, I opened the *thorax*, and, with some surprize, found the pulsation of the heart as regular, as if the animal were in perfect health. This appearance continued above 2 hours after the cat's head was off; but afterwards languished, and was much weaker.

I then opened one ventricle of the heart, in which the blood was somewhat coagulated. This may be thought to be partly owing to the medicine; for, soon after it had produced convulsions in the creature, I had a mind to see what bleeding would do, and with that view cut off the tail; but, contrary to my expectation, the arteries that supply it with blood bled very little; and, upon cutting off the head, the carotids and both vertebrales did not pour out above $\frac{1}{2}$ a common spoonful. But as it might be questioned by some, from the continuance of the heart's pulsation, whether the cat might not possibly, if let alone, have recovered, I poured a few drops of the same solution as before into a superficial wound of a young dog, weighing 12 pounds: the creature, in less than an hour, shivered, became sleepy, was very cold, and so stupid, that he suffered himself to be often burnt by the hot ashes beneath the grate, where he lay for warmth. In this comatous way he continued near 4 hours, and then shook off his *stupor*, and was much better. I left him all night, and found him next morning quite well, and as hungry as ever. Upon this I made an incision at that time into one of the crural veins, and poured a few drops of the solution into it: in less than 10' the dog gave signs of great pain, soon shivered, grew cold, was convulsed, and in less than 20' died.

Upon opening him nothing uncommon was found, nor was the blood in this creature's heart so thick as in the former. The crural vein did not bleed from a large orifice, after the poison was infused, though it was likely to do it before.

But,

But, as some authors have said, that birds in particular are instantly deprived of life, if the least particle of certain poisons are infused into the blood, I infused a few drops of our solution into a cuticular wound of a small bird. This occasioned hanging of the feathers, and a *stupor*, in less than 10', and killed him in somewhat more than 15.

I gave about 3ij of sugar to another bird of the same kind, and shortly afterwards poured a little of the solution into it's mouth; but 2 drops had scarce touched his tongue before the creature was convulsed, and I could with difficulty lay him down before all motion was taken away. I gave these two birds to 2 cats; and whether from eating them or not I don't pretend to say, the cats made so uncommon a noise the whole night, that they disturbed the family's rest.

From these experiments we find that the supposed specific is of no manner of use, even when the poison is only taken at the mouth; and from them it may appear probable, that our poison is nearly upon the same footing with white arsenic in the cure of the tooth-ach.

III. I have sent inclosed a scheme, which I have a great while thought of, for the improvement of distillation in the chemical way. I flatter myself, it will be very advantageous in procuring many fine volatile things, which we cannot so well do in the ordinary method; and that we may be enabled by it to analyse some things, as blood, and such-like substances, without breaking our glasses; as the learned *Boerhaave* complained he could not do, by reason of a pitchy bituminous matter rising up into the neck of the retort, and bursting it. But, in short, I have found, from many experiments, that it is the vast quantity of air, arising from such substances, which bursts the glasses, and which this method will prevent.

Fig. 157. *A*, a common retort in sand-heat. *B*, the first receiver, with an opening at the top and bottom. *C*, a quart-bottle fixed to a neck out of the bottom of the receiver; which, being tied on close by means of a bladder, may be removed at any time, and another instantly placed in it's room; by which means, very little of the steam will make it's escape: and any proportion of the volatile part to be distilled may be saved by itself, without unluting the recipient from the retort. *D*, a second receiver, inserted into the opening at the top of the first, in order to give more room to the rarefied and new-generated air, and to receive the most subtile and volatile parts, which might not be so easily contained in the first recipient, without great danger of breaking it, or forcing the luting. *E*, a smaller bottle, for the same uses as the other, marked *C*. *F*, a bladder tied on to an opening, or upper neck, of the second recipient; which, as it is much thinner and weaker than any of the glasses, will always give way first, and prevent their bursting. Or, wherever the matter to be distilled is of such a nature, that, we are sure, all the glasses, put together, will not contain the fumes and air arising from it; then, if the small-

Concerning a new contrivance of applying receivers to retorts in distillation; in a letter from Mr Browne Langrish Licent. Coll Med. Lond & F. R. S to the Rev Dr Hales, F. R. S. No. 475 p 254. Jan &c. 1745. Read Jan. 24. 1744-5. Fig 157.

est pin-hole be made through the top of the bladder, as soon as the fumes begin to rise, it will be sufficient to let out the air as fast as it generated: and, I think, at that great distance from the fire, very little, if any thing, but air, can make it's escape.

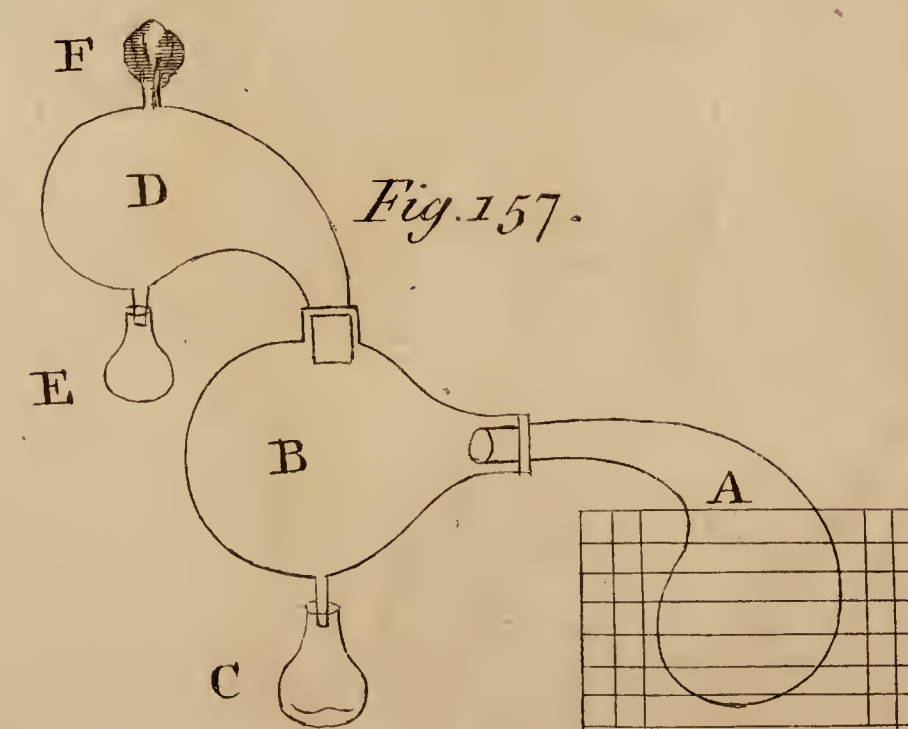
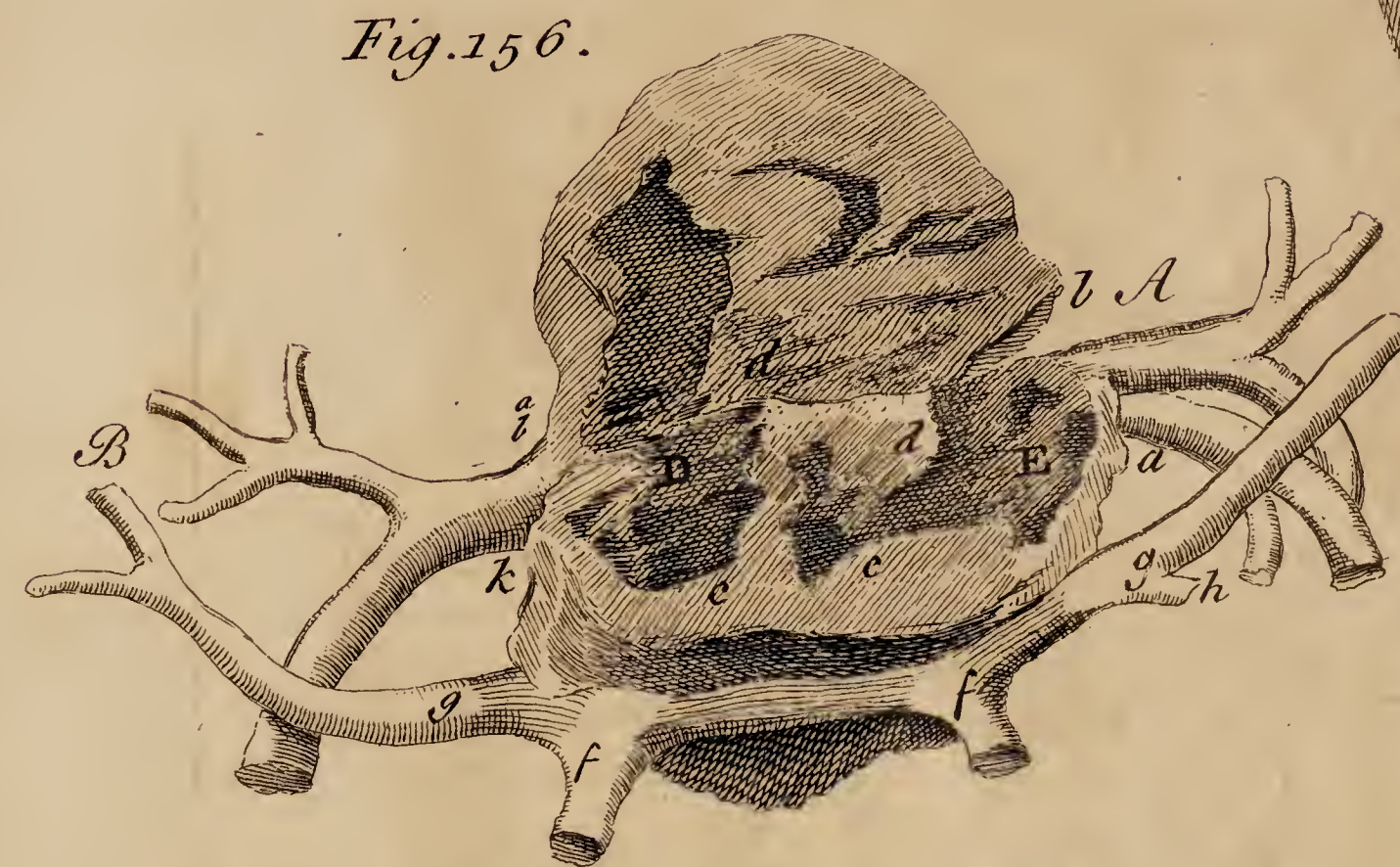
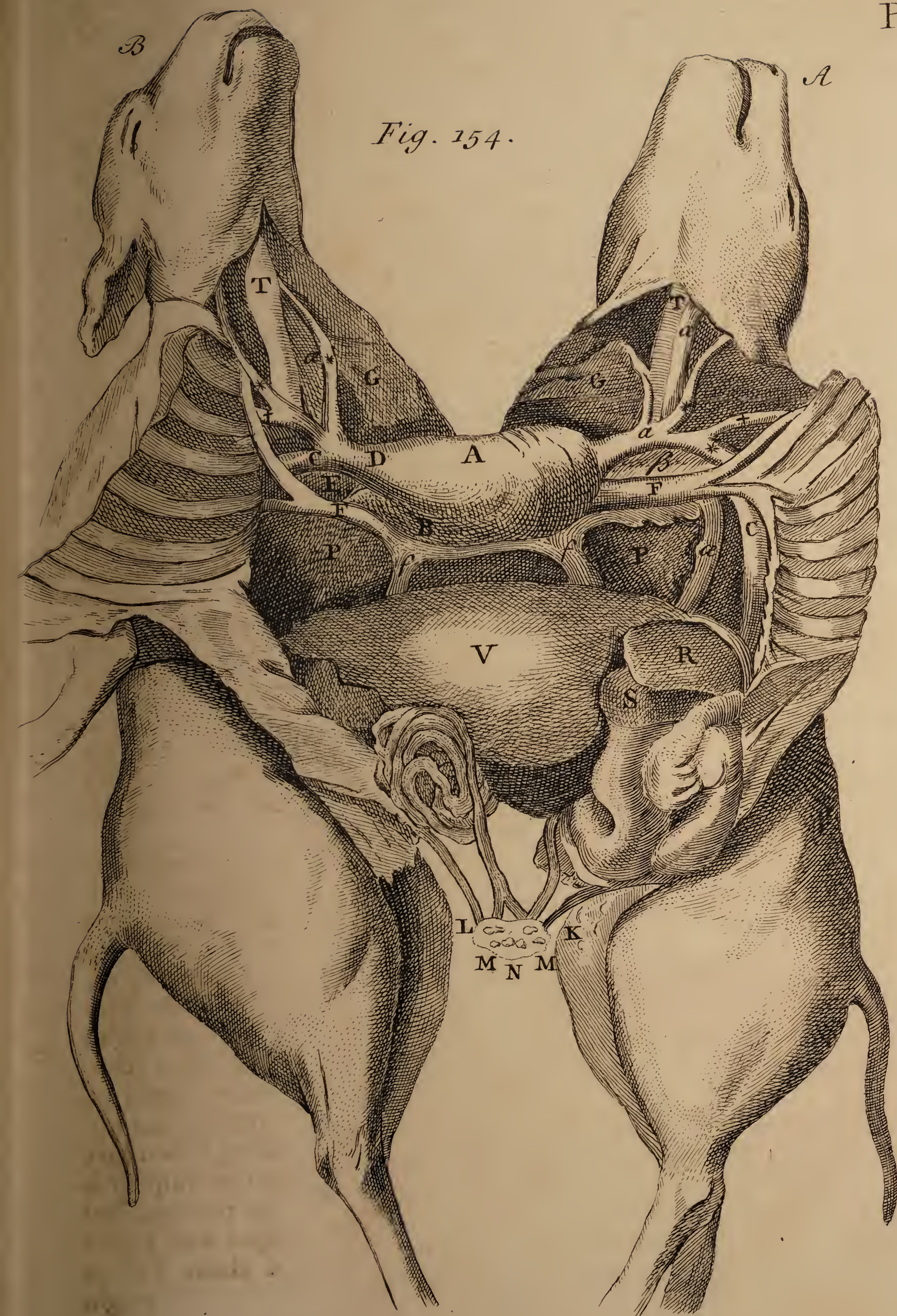
Hence great advantages will arise from this way of distillation: for,
1. We can keep a greater fire, if occasion be, without fear of breaking our glasses. 2. The matter distilled may be removed as often as we please; whereby we may always prevent the drawing off any spirit, &c. too low. And, 3. Any pure, fine, volatile salt, which shall arise into the second recipient, will not be so liable to be melted down, either by the heat, or too watery a fluid.

An easy method of procuring the volatile acid of sulphur; by Ephraim Rinkel Seehl; addressed in a letter to the Pres. and Fellows of the R. S. N^o. 472. p. 1. Jan. &c 1744. Read Jan. 19. 1743-4.

IV. The difficulties which attend the making of the *oleum sulphuris per campanam*, even after the improvements of M. Homberg, are so great, the process so tedious, and the produce so moderate, that this preparation is rarely to be met with in the shops; but the oil of vitriol is commonly sold, and used, in it's stead. Nor, perhaps, if the true *ol. sulph. per camp.* could be obtained cheap, is this the thing so much wanted in Chemistry, Pharmacy, and Physic, as the volatile acid of sulphur; containing not only the *oleum sulphuris*, as it is called, but, at the same time, the pure native gas, or highly volatile spirit, of the sulphur; which in our method is preserved; and which, we have reason to believe, considerably increases it's virtues, both as a menstruum, and a medicine. It was Dr Stahl's method of procuring the volatile acid of sulphur, that put me upon the thought and endeavour of doing the same thing in a better manner: and, indeed, I look upon my method to be no more than an improvement of his; and this will appear by the process which follows: though I have two ways of obtaining the spirit of sulphur; one by the means of fixed alkali salt; and the other by means of the same salt and quick-lime.

PROCESS I.
To make the volatile spirit of sulphur with alkali salt per se.

Take ℥ i of the flowers of brimstone, and ℥ v of dry fixed alkali salt; grind them together, and put the mixture into an iron pot; add, by degrees, a little water, so as first to dissolve the fixed alkali; then gradually dispose the whole to boil, in order the better to dissolve the sulphur: when these have boiled $\frac{1}{4}$ of an hour, add more water by degrees; and, when the sulphur appears to be dissolved, filtre the solution; evaporate it to perfect driness in an iron pot, till it almost begins to melt; then take out the dry powder when cool; put it into a tubulated retort; which being placed in a sand-heat, and a receiver luted on, pour in at the tube, by degrees, ℥ ii of rectified oil of vitriol; and immediately secure the tube with a stopple of chalk, and luting: then give a gradual fire for some hours, till all the volatile spirit of sulphur is come over; after which let the fire go out; take off the receiver, and carefully pour the liquor into a glass phial, to be stopped with a glass stopper. The volatile spirit, thus procured, will be about ℥ xij in weight



weight, and appear tolerably limpid, smell extremely quick, pungent, and gassy or sulphureous, almost like the *gas sulphuris*, prove strongly acid to the taste, and in all other experiments; so that it may be used in the way of a general acid; being, perhaps, the best, in all respects, that is hitherto known, except the following:

Take ℥ i of the flowers of brimstone, ℥ iv $\frac{1}{2}$ of fixed alkali salt; PROCESS II. grind and mix them well together; put the powder into an iron pot set over the fire; add a little water, by degrees, to dissolve the salt; then boil gently for $\frac{1}{4}$ of an hour; add more water, and afterwards ℥ iii of strong quick-lime; let all boil together for a while: when the solution is complete, filtre the lixivium, and evaporate to a dry powder, as in the first process; put this powder into a tubulated retort; and pour on, by degrees, ℥ i of rectified oil of vitriol; proceed to distil as before: thus ye will obtain ℥ viij of a more strong, more acid, and more volatile spirit, than the former, and of a yellowish colour. *To make the volatile spirit of sulphur cum calce.*

1. The proportions of the several ingredients here set down, I have found, by repeated trials, to be the best. ℥ v of alkali salt are thus absolutely necessary to dissolve ℥ i of sulphur; though, when quick-lime is used, as here specified, ℥ ivs of fixed salt are sufficient; or even ℥ iv, if the quick-lime be very good and strong: so much doth the lime strengthen the lixivium, or enable it to dissolve the sulphur. OBSERVATIONS.
2. These two processes differ somewhat considerably, as to the quantity and quality, both of the spirit and *caput mortuum* they afford. The spirit made with lime is less in quantity, but specifically heavier, and yet more volatile, than the other: and the *caput mortuum* with lime is much whiter, purer, and fitter for making the *tartarus vitriolatus*, than that made without lime.
3. By mixing ℥ i of fixed alkali with the sulphur at first, boiling them a little, and filtering the liquor, then adding ℥ ii more of the salt along with ℥ ii of lime, I have found, that the sulphur sooner dissolves, than if I put the whole quantity in at once; and thus, after the second filtration, I put in the rest of the salt and lime, till all the sulphur is dissolved; as finding this the readier way to perfect the solution.
4. In the distillation, a little of the sulphur will sometimes sublime into the neck of the retort; and this seems owing either to making the fire too fierce at the beginning, or using the oil of vitriol too weak: But such a sublimation of the sulphur is no farther detriment to the operation.
5. When lime is used in this process, a considerable proportion of fixed alkali salt may be saved, the spirit be rendered stronger, and the *caput mortuum* cleaner and whiter, so as to make an excellent tartar of vitriol, by solution, filtration, and crystallization: but it must be observed, that the produce of this tartar of vitriol, when prepared, is not near so large as when no lime has been used in it; and accordingly

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ingly I have found, that the dry powder, remaining after the solution and evaporation of the *sal alkali* and sulphur alone, weighs as much as they did originally : whereas, when lime has been used, the remaining powder has weighed ~~half~~ less than the original weight of the sulphur and fixed salt ; which seems a curious phænomenon ; and might lead to farther discoveries of the relation betwixt lime and fixed alkali salt, &c.

6. The advantages of this method, in respect of M. *Homborg's*, are,
 - (1.) That it gives a much larger quantity of the acid of sulphur.
 - (2.) That it gives a very volatile acid ; whereas his is fixed, so as scarce to differ from oil of vitriol.
 - (3.) That it is obtained in a much more easy and cheap manner.
 - (4.) That this spirit has, probably, much greater medicinal virtues.
 - (5.) That it is a much more powerful menstruum ; especially with regard to metals, and particularly their crystallization.
 - (6.) That the *caput mortuum* is a medicine of great use ; and may defray the expence of the whole operation ; being, perhaps, the best way of making the *tartarus vitriolatus* perfectly pure and neutral for medicinal purposes ; it's expected virtues greatly depending upon it's being clean and neutral.
7. Our method has also several advantages over Dr *Stahl's* ; though his indeed affords a volatile acid. But then, (1.) His method burns the sulphur, and, consequently, destroys it's texture, and throws off, or exhales, part of the spirit or gas ; whereas ours gently dissolves the sulphur, and only divides it, so as to leave the acid afterwards separable by a stronger or more ponderous acid ; and no way consumes or destroys the inflammable part, as burning does. (2.) Our method is more neat or elegant than his, and affords a larger produce, at a cheaper rate, and in greater perfection, both as a medicine, and as a menstruum ; leaving also the *tartarus vitriolatus* cleaner, and fitter for use as a medicine.
8. Persons but little versed in chymical Philosophy, and the operations it makes use of, might be apt to suspect, that this spirit of ours is not a pure spirit, or acid of sulphur ; but mixed with the oil of vitriol, here used as the medium to separate the spirit from the sulphur and fixed alkali : but ye very well know it to be an universal law, that an heavier or stronger acid, used in a suitable proportion, constantly, in these cases, separates a weaker, and leaves it free to rise by itself in distillation, as it remarkably does in the present operation ; where all the oil of vitriol employed unites with the fixed alkali, so as to make the true *tartarus vitriolatus* ; and leaves the lighter spirit quite detached and free to rise, and come over the helm in distillation : so that this volatile spirit and the fixed oil of vitriol are by no means the same thing ; nor should the one be used for the other, especially in physic.
9. But tho' the oil of vitriol be allowed to differ from the volatile acid of sulphur, some may imagine, that there is no difference betwixt

this

this volatile acid and the volatile spirit of vitriol, as it comes over in the rectification of oil of vitriol; or betwixt our spirit and the *gas sulphuris*, which is extremely pungent and volatile: but whoever attentively examines and compares the volatile spirit of vitriol, or the *gas sulphuris*, with our spirit, will soon be convinced of a great difference; tho' indeed, they agree in the point of *gassy* volatility: for the volatile spirit of vitriol is only an impure phlegm of vitriol, containing very little acid, and is chiefly impregnated with the wild fumes of the vitriol; so as, upon standing a while, to quit the liquor, and leave it nauseous, vapid, and gross; whereas the volatile spirit of sulphur long preserves it's volatility, the purer *gas* being here lodged in a pure acid liquor, less dense and gross than oil of vitriol; so that when, by being long unstopped, this acid spirit loses of it's volatility (as it will do), yet it never loses of it's acidity; and even then appears to be the most pure and perfect mineral acid we can any way procure. And, as to the *gas sulphuris*, when made in perfection; this is no more than the fumes of burning brimstone caught and detained in water: so that this preparation, wanting the acid, cannot be compared, in that respect, with our spirit, which has it in perfection.

10. What the medicinal virtues and uses of our volatile acid of sulphur may be, I humbly submit to you, and the learned Physicians, to whom it belongs; only beg leave to observe, that if what we find in numerous learned physic-books be just, there are hopes, that it may prove a noble medicine in many kinds of fevers, the small-pox, and even in plagues. In some of these books we read, that malignant fevers are owing to a superabundance of volatile alkaline salts in the body; and, if that be the case, one might hope to neutralize or destroy such a superabundance of volatile alkaline salts, by the prudent use of this fine volatile acid; which is capable of being mixed with water, juleps, and most sorts of drinks.

11. I likewise find, that the origin of all pestilences and plagues has been assigned to the following causes; *viz.* (1.) The carcases of men, horses, or cattle, killed or slain, and putrefying above-ground by heat and moisture, and thus infecting the air by their noxious, volatile, urinous alkaline salts, that copiously issue from them in such a putrefying state. (2.) Dead fish, thrown out of the sea, and putrefying on the shore; or swarms of dead insects, bred in fens and marshes, drowned in the ocean, and thrown on shore by the tides, and left to putrefy in hot moist climates. (3.) Woollen goods, silks, and apparel, packed up or worn by infected persons, or those that attended the sick, or that came from infected places. (4.) Unwholesome diet, or corrupted putrefying meats, abounding with too subtilized, or too rarefied, volatile, urinous salts. (5.) Mineral, arsenical, and poisonous damp, vapours, exhalations, &c. arising from vulcano's,

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vulcano's, mines, grotto's, by means of subterraneous heats and fermentations.

12. It were easy, by natural reasoning upon these causes assigned of the plague, to shew that distemper consists in a kind of putrefactive state of the body, when the salts are volatilized, unsheathed, and let loose to tear and wound the solids, after destroying the texture; and, consequently, that the volatile acid, here shewn to be easily procurable, is a natural remedy in such cases: but, being sensible how fallible all such kind of reasoning may be, I intirely submit the whole to your maturer judgments.

The End of the THIRD PART.




THE
Philosophical Transactions

A B R I D G E D.

P A R T IV.
CONTAINING THE
Historical and Miscellaneous
P A P E R S.

C H A P. I.
HISTORY and ANTIQUITIES.

- I.  HE subject of our late conversation turned upon the affectation of some nations, in carrying up their histories to so immoderate a height, as plainly to shew those accounts to be fictitious and without foundation. This, it was agreed, was the case of the *Babylonian* and *Egyptian* accounts; and you seemed to think it would be found to be the same with any other people that should make the like pretensions.
- The only people in later times that have been thought to contradict this opinion are the *Chinese*, of whose history the world hath been taught to entertain very extraordinary conceptions. But that even They will
- Concerning the Chinese Chronology and Astronomy; in a letter from the Rev. Mr G. Costard, to the Rev. Thomas Shaw, D. D. F. R. S. and Principal of St Edmund-*

Hall. N^o.
483. p. 477.
Mar. &c.
1747. Read
April 30.
1747.

be no exception to your surmise, but, on the contrary, a strong confirmation of it, will, I persuade myself, appear, from what I am now going to offer.

I need not inform you, that the eastern writers in general are much addicted to fable and romance. This is a fact too well known to need any proof; and therefore great judgment is many times required to distinguish what is real from what is purely imaginary, improbable, and absurd. I say this, not so much with regard to their accounts of foreign nations, with whose affairs they may be presumed to be less acquainted, as of their own ancient state and condition, and that in ages not exceedingly remote. But if this observation holds but too true, with respect to those whose history we are in some manner acquainted with, how much ought it to put us upon our guard as to those we are in great measure strangers to?

The best accounts we have received of *China* are owing to the *Jesuits*. But those accounts themselves are, I am afraid, to be frequently received with great caution. These Fathers have been sometimes, perhaps, not sufficiently versed in *European* or *Chinese* learning, or both, to give us proper information. At other times, it may be, they have been too much prejudiced in favour of their converts, or had ends to serve, of which the world hath not been properly enough apprised. To have propagated their religion only in a barbarous and uncultivated nation, would not have been so much for the credit of the mission, as to have been able to introduce it among a people civilized and polished by arts and literature.

Suspicious as these and the like circumstances are, is it not surprizing to hear authors, upon their words only, and upon little or no foundation besides (as I question not will appear), asserting with so much positiveness, that the *Chinese history reaches up indisputably to the times of Noah* (a)? A thing so far from being *indisputably* true, that no article whatever perhaps will admit of greater debate. 'Tis true indeed, the *Chinese* give us a long list of Kings that reigned among them from the time of *Fo-hi*, and a series of dates, that, if allowed, may carry up his age 2952 or 2847 years before the Christian *era* (b). But how easy is it to feign * dates and successions of kings! Let it be made appear what foundation this Chronology depends on; what ancient monuments the *Chinese* have, and in what manner preserved. Marbles, I suppose, they have none; and their paper, such at least as is brought into *Europe*, appears to be of too fine a consistence for the preservation of records.

You will be told, Sir, perhaps, that a great part, at least, of their *chronology* is verified by *eclipses*. A very pompous argument! but, when narrowly examined into, will be found to prove just nothing at

(a) *Shuckford's Connect* Vol. I. p. 101.

(b) *Ibid.* p. 29.

* See these *Transf.* N^o. 415. p. 397, where this Chronology seems to be set in a true light by the viceroy himself of *Canton* 1724. C. M.

all. We are told indeed (c), that the ancient *Chinese* observations consist of 26 *eclipses* of the *sun*, and 21 conjunctions of *Jupiter* with the fixed stars. The oldest *eclipse* of the sun is placed in the first year of the reign of *Tching-Cham* (d), supposed to coincide with the year before *Christ* 2155: but the oldest conjunction of *Jupiter* reaches no higher than the year after *Christ* 73 (e): and how inaccurate the observation was, appears from hence; that the *Chinese* only mark the day when that conjunction happened.

But the question naturally arising here is, How it comes to pass, that the *Chinese* accounts afford no example of any planetary conjunction before this, when they produce an *eclipse* of the sun 2228 years earlier? By what good fortune came that *eclipse* to be preserved, and all appulses of the *planets* to fixed stars for so many years be lost? Let us suppose, that these were things below the notice of *Chinese* Astronomers; or that they did not know what use to make of them. But in what manner must we account for this, That we hear nothing of any other *eclipse*, till the year before *Christ* 776 (f)? That all the *eclipses*, observed during so long an interval as 1379 years, should have perished, and this one have escaped, requires a pretty strong faith to believe.

But farther, we are told, that they observed the *winter solstice* in the year before *Christ* 1111. There is nothing, it is true, impossible in this; for it is not said how accurate the observation was. The difficulty is only to ascertain the fact, and convince reasonable people that it was made at all.

'Tis well known, and allowed by the Missionaries themselves, that the reception they have met with in *China* hath been more owing to their character as *Philosophers* than *Apostles* (g). When therefore they brought with them into the country accounts of *European* discoveries, and particularly in *Astronomy*, might not the *Chinese*, agreeable to their vain-glorious character, tell them, that they had of their own much older than any thing they could pretend to? It may be said indeed, that this is no more than supposition, and which consequently argues but little: but then the supposition is so easy and natural, that it requires at least the contrary to be made out by some very good proof.

One reason why this may be insisted on the more is, that the *Chinese*, according to the *Fathers* themselves, have not always been faithful in their relating observations. *X-hang*, about the year after *Christ* 721, had the reputation among them of an able *Astronomer*; but being mistaken, it seems, in his calculation of an *eclipse*, rather than own his ignorance, he pretended, that the *heavenly* bodies did not always observe the same laws. In support of which extraordinary hypothesis, he urged,

(c) *Obs. Math. Astron. Geogr. Chronol.* Tom. I. *Pref.* p. 13, 14.

(d) *Ibid.* p. 18.

(e) *Ibid.* p. 15.

(f) Not much before the oldest *Babylonian* eclipse that is preserved. See *Letter to M. Folkes*, *Esq.* p. 21.

(g) *Observat. ut sup.* Tom. 2. p. 117.

that, in the time of *Tsin* (*b*), the star *Sirius* was eclipsed by the planet *Venus*; tho' the latitude of *Sirius* is $39^{\circ} 32' 8''$, and that of *Venus* never exceeds 4° . The same sort of observation with this, I suppose, is the other of the conjunction of *Saturn*, *Jupiter*, *Mars*, *Venus*, and *Mercury*, in the constellation *Cbe*; when the *Sun* and *Moon* likewise were in conjunction in 15° of *Aquarius*, in the time of *Tchouen yu* (*i*).

And to put it out of all doubt, that the *Chinese* are capable of obtruding upon the world *fictitious* observations, we need no other authority still than that of the *Learned Fathers* themselves. In the year 1725 (*k*) the *Missionaries* sent into *Europe* an account of an approximation of the four planets *Jupiter*, *Mars*, *Venus*, and *Mercury*. Such planetary conjunctions, it seems, in *China*, are looked on as happy omens of good fortune to the prince upon the throne. The *Chinese* therefore, as if bred up at the court of *Versailles*, with a true *French* politeness, in compliment to their sovereign, marked in their registers a conjunction of all the 7. This false account of an imaginary conjunction, as the *learned Jesuit* himself observes, may, in future times, be the occasion of very great errors. To the *Chinese*, I suppose he means; for in *Europe* the danger will be but small; where there are better tables, exacter accounts, and more accurate observers, than the most sanguine *Jesuit* will pretend to be among the *Chinese*. But if they would venture at recording such a spurious observation, at a time when they were sure of being detected, what may we not suspect them to have been guilty of, when they had none to confront them; and how little may we presume they know of the uses to be made of *celestial* observations?

We are told, with great pomp and assurance (*l*), That there *always* was in *China* an office of *Mathematics*, and another of *History*: that it is the business of the former to calculate *eclipses*; and of the latter to register them, and every other occurrence that happens in the State.

It would have been well, if the *learned Fathers* had told us with a little more exactness what we are to understand by the term *always*; and whether the *Chinese* are acquainted enough with the uses for which *eclipses* serve, to make it probable, that they should have had such an office any *considerable number of years*, and much less *always*. May we not in the mean time suspect, upon hearing such language as this, that the *Fathers* mistake *Pekin* for *Paris*; and, having their heads full of the *Academy of Sciences*, cannot help figuring to themselves the like in the remotest corners of *Asia*?

Be this as it will, they tell us, That the *Mathematicians* have often had the credit to take out of the registers their *false* calculations, and substitute in their room others, agreeing with observations. But where things are kept with so little exactness, what can be expected but con-

(*b*) *Observat. ut sup.* Tom. 2. p. 86. *Flamsteed's British Catal. Greg. Astron.* p. 5.

(*i*) *Observat. ut sup.* Tom. 2. p. 149.

(*k*) *Observat. ut sup.* Tom. 2. p. 33.

(*l*) *Observat. ut sup.* Tom. 2. p. 158.

fusion? What less indeed can be expected than what the *Fathers* assure us themselves hath frequently been the case (*m*), That by this means *eclipses* have been preserved, that calculations made by *European* tables demonstrate to be false? Can we wonder likewise, that the *learned Fathers* should doubt many times, whether such or such a particular eclipse be an *observation* made at the time, or the result only of a *calculation*, and perhaps a false one (*n*)?

For, after all that hath been said of *Chinese eclipses*, and the *calculations* of them, it is agreed, (*o*) that, before the time of *Lieou hong*, or *A. D.* 206. they had no fixed principles upon which to proceed in that business. This observation, I am afraid, will extend to much later times; or 'tis scarce to be imagined they should look upon total *eclipses* of the sun as ill omens. In consequence of this superstitious belief, we are told (*p*), That the *Chinese Astronomers* have carried their compliment to the family on the throne so far, as to affirm no such *eclipse* could happen during their time. On the other hand, should an *eclipse* of this sort happen, without being foretold, they immediately pretend it to be a warning from Heaven of some misfortune likely soon to befall the government. But if it should be foretold, and not come to pass, they would then make the many virtues of the sovereign the preventing cause; and, what is better still, a shelter for their own ignorance. Such notions as these however, I think, plainly demonstrate them to be very *bungling Astronomers*; and that they can hardly look upon these *phænomena* as depending on established invariable laws of nature; the consequence of which is, that they can no more attempt bringing them to a *calculus*, than winds, thunder, and lightning; and the like.

It was observed before, that the *Mathematicians* had many times the art or the credit to take out of the registers their false calculations; but we are told in the same place, That, before they were repositied there, they were presented to the emperor, for his inspection. Let any one, that knows the least of the despotic governments of the East, reflect on the probability of this; and whether the attempter would not run great danger of paying for it with his head.

What hath been already said, will, I suppose, be more than sufficient to shake the credit of *Chinese* observations. But what must we think of those very ancient ones, when we are farther told (*q*), from the time of *Tchun-tsieou*, or 480 years before *Christ*, the *Chinese* themselves allow *Astronomy* was almost intirely neglected among them; and that *Tsin-chi-hoang*, whose reign began in the year before *Christ* 246, ordered all books of *History* and *Astronomy* to be burned (*r*)? But every one will easily imagine what destruction of observations must have been made

(*m*) *Ut sup.*
Tom. 2. p. 32.
(*r*) *Ibid.* p. 2.

(*n*) *Obs. ut sup.* Tom. 2. p. 159.

(*p*) *Ibid.* p. 33.

(*o*) *Observat. ut sup.*

(*q*) *Observat. ut sup.* Tom. 2. p. 1.

during a neglect of these studies for 234 years; and how little would remain to be burned by this *anti-astronomical Prince*.

It was owing to this devastation, it seems, that the *Chinese* are said to have lost the method taught by the Ancients, and particularly the Emperor *Yao*, of calculating the places of the 7 *planets*, and the fixed stars (*s*). It may with good reason be questioned, whether they really had any methods of calculating their places at all: for to what purpose could such calculations serve, when their catalogues of stars, many centuries later, are acknowledged to have contained nothing more than bare names, without *latitudes*, *longitudes*, *right ascensions*, or *declinations*? Such their catalogues were, if they deserve that name, that were made under the race of emperors called *Soug* (*t*), or between *A. D.* 591 and 620; and it will be difficult to prove they were any thing else, before the *Jesuits* introduced there *Tycho-Brabe's*, or other *European* ones. As to the places of the planets, how little they were able to compute them will appear from hence, that the utmost that *Lieou-hin* and *Lo-bia-hong*, in the year before *Christ* 66, pretended to, was to calculate a plain rectangled triangle (*u*). In what manner they did this is not said; but it would be well, if the *learned Fathers* would make it appear that the *Chinese* had, in much later times, any thing like a table of natural sines and tangents. A small skill in *Mathematics* is requisite to apprehend from hence how bungling their astronomy must have been; and if so, much more that of the ages preceding them! And it will be yet further hard to imagine that they knew how to find the places of the planets, when we are assured (*x*), that *Tchang-tse-tsin*, about *A. D.* 550, was the first person that introduced equations into their computations of the planets motions; that *Co-chiou-king*, about *A. D.* 1280, was the first *Chinese* that knew any thing of *Spherical Trigonometry* (*y*); and that, before the arrival of the *Jesuits*, they were intire strangers to the inclinations of the planes of the planetary orbits (*z*).

After what hath been said, I think, we need but little more to convince us of the small acquaintance of the *Chinese* with *Astronomy*. They tell us however themselves (*a*), That, from the time of *Tsin-chi-boang* above-mentioned, they had no expert *Astronomer*, no books of *Astronomy*, nor known method of computing. All that remained were some *confused traditions*, *catalogues of stars and constellations*, and *fragments of books*. A mighty encouragement all this, to expect reforming the *European Astronomy* or *Chronology* by the *Chinese*! What sort of *catalogues* these were, hath been already observed; and we may, without offence, I presume, beg to be excused from paying over much deference to *Chinese Tradition*; at least, till the *Fathers* have better determined what degree of assent it deserves.

(*s*) *Observat. &c. Tom. p. 3.* (*t*) *Observat. &c. Tom. p. 65.*
ut sup. Tom. 2. p. 8. (*x*) *Observat. &c. Tom. 2. p. 58, 59.*
p. 114. (*z*) *Ibid. p. 84.* (*a*) *Observ. ut sup. Tom. 2. p. 3.*

(*u*) *Observ.*
 (*y*) *Ibid.*

About *A. D.* 164. several *Jew* families, and other subjects of the western empire, came into *China* (*b*). At that time, as it is allowed by the *Jesuits* themselves, *Ptolemy's Astronomy* was in great vogue all over the East; and they seem to suspect, that by this means the *Chinese* might get some faint knowledge of it. It is certain, that from this time we meet with things unknown to their former accounts of *Astronomy*. At this time, it is said (*c*), *Tchang-heng* made a catalogue of 2500 stars. Such a catalogue as those already mentioned were, we may suppose it to be, if it was at all; for *Tchang-heng's* book is lost; and what accounts we have of him or his works, depend on the authority of others.

A. D. 284. (*d*) lived *Kiang-ki*, the first *Chinese* that is said to have known any thing of the motion of the fixed stars. This we see was 120 years after the above-mentioned arrival of the *Jews* in *China*; but either they must have been unskilful relaters, or the *Chinese* bad disciples, since *Kiang-ki*, it seems (*e*), made that motion to be at the rate of 1° in 50 years; whereas *Ptolemy*, it is well known, made it 1° in 100 years. It may be said indeed, that this difference shews it could not be borrowed from *Ptolemy*; but then it shews at the same time, that it could not be the result of any series of observations; and that is as much, I think, as we need be concerned about. And this will appear yet farther, by remarking, that *A. D.* 460, it was made by *Tson-chong* 1° in 45 solar years and 9 lunar months (*f*). At other times it was made yet different still; but never, I think, from observations of the stars themselves. The method, it seems, was by comparing the places of the solstices in their own time, with their places in the reign of the Emperor *Yao* (*g*), whom they supposed to have lived at a time co-inciding with 2300 years before *Christ*. Let us suppose them to have been right in this (a thing we are by no means obliged to allow), yet, as they never appear capable of taking the *solstices* with any tolerable degree of accuracy, we cannot wonder at any mistakes we may meet with.

You have seen, Sir, all along, constant mention made of *Chinese calculations*; the very word used by the Fathers themselves; though I am sensible that term will be apt to lead the unwary into great errors. For the most that can be made of their *calculations* is nothing more than finding the places of the luminaries by numbers (expressing their periods and parts of periods), or, in other words, by their mean motions. For, as to the *Sun*, we are assured (*h*), they made his motion one *Chinese* degree in a day, without knowing any thing of an *equation* necessary to correct it. It was not till the third century that they had any *equation* for the *moon* (*i*); and *Tchang-tse-tsin*, about *A. D.* 550, as we have seen, was the first that used any for the *planets*. It is observable, however, with regard to this author, that there are no writings of his extant (*k*);

(*b*) *Observ. ut sup.* Tom. 2. p. 119. (*c*) *Obs. ut sup.* Tom. 2. p. 25. (*d*) *Ibid.* p. 44.
 (*e*) *Ibid.* (*f*) *Ibid.* p. 52. (*g*) *Observ. ut sup.* Tom. 2. p. 148.
 (*h*) *Ibid.* p. 6. (*i*) *Ibid.* p. 24. (*k*) *Ibid.* p. 58, 59.

and therefore what is here said of him, may be nothing more than report. Among an idle vain people, unacquainted with critical learning, round assertions pass for proof; and in this manner, beyond question, the *Chinese* have frequently imposed upon credulous unskilful *Europeans*.

A. D. 618. began the reign of the Emperors of the race of *Tang*; and about this time other western strangers came into *China* (l). From them therefore the *Chinese* might learn what farther improvements we may chance to meet with in their *Astronomy*, besides those for which they are assuredly indebted to the *Jesuits*.

It was observed but now, that in computing the places of the heavenly bodies, the *Chinese* at best knew nothing but their mean motions: but in such computations it is necessary to begin from some *radix*, or other: *European* tables generally begin with the commencement of the Christian *era*; the *Chinese* appear never to have known any. One *epoch* indeed they have, but intirely imaginary, called *Chang-Yuen* (m), and which began some time or other at *midnight*, at the moment of the *winter solstice*, when the *Sun*, *moon*, and 5 planets, *Saturn*, *Jupiter*, *Mars*, *Mercury*, and *Venus*, were all in conjunction, and the *Moon* without any latitude.

This extraordinary *epoch* began, according to these able Astronomers, 143127 years before the *winter solstice*, in the year before *Christ* 104 (n). What time this *epoch* came first into use is not known; but the *Fathers* think, and it must be acknowledged with great *probability*, that it is not older than the burning the books under *Tsin-chi-hoang* (o), or, as we have already seen, the year before *Christ* 246. Should we place it however many years later, or say that it never served any real use at all, we may perhaps come much nearer the truth.

For the *Chinese* Astronomers, as the *Fathers* observe (p), have spent an infinite deal of time and pains, in searching out this *Chang-Yuen*; and which has been carried up, they say, by some 2, by others 3 millions of years beyond the time it was above fixed at. But this shews to a demonstration, that it is an *epoch* purely fictitious; that, if it was real, it could only be of an *astronomical* nature; and that they must be *sottishly stupid*, that should from thence collect, that the *Chinese* had any historical memoirs of so ancient a date. For the *Fathers* themselves allow, that the opinion which ascribes to the world a duration of some millions of years, is neither the general opinion of the *Chinese*, nor of any ancient standing among them (q).

From what has been here offered, I think it is pretty evident, that, how ingenious soever the *Chinese* may be in works of art, their talents do not lie towards *Mathematics* and *Astronomy*: for, was not this the case, must it not be surprising, that having, as they say, so long a series

(l) *Obs. ut sup.* Tom. 2. p. 71, 72, 96. (m) *Ibid.* p. 16. (n) *Ibid.*
(o) *Obs. ubi sup.* Tom. 2. p. 18. (p) *Ibid.* p. 17. (q) *Ibid.* p. 17.

of observations in the one science, and of professors in the other, they should never have been able to get beyond the first elements of either?

It is not my design to enter into any controversy with the *learned Fathers* of the *Society of Jesus*; the world hath been frequently indebted to them for their philosophical labours; and will be so again, when they shall have considered the *Chinese* history with proper accuracy, and told us in what manner they have been able to preserve accounts and observations of so ancient a date. Public libraries, it is allowed (*r*), they have none; nor doth it appear they ever had. Where then could things so useles, as the generality must have thought *astronomical observations*, be repositied? When intrusted to private hands, they must have run great risque of being destroyed by wars, by fires, and in popular commotions; which must frequently have happened in so long a course of years.

Let us suppose things of this sort are of more value to the *Chinese* commonalty, or, if you please, their nobility, than they are to the *European*; and that they would lay hands on every thing they could meet with of the kind; and, when once in their possession, would preserve it with a religious exactness: but whence then comes it to pass, that it is *so difficult a matter* in *China* to meet with books upon these subjects (*s*), to understand them when found, or to get any assistance from the natives towards understanding them?

But besides, are not writings thus kept in private persons custody, unless carefully laid by, apt to be scribbled on by the *Sciolists*; so that it may be hard to distinguish many times the text from what may be called the comment? Is not this, in fact, according to the *learned Fathers* own account (*t*), very frequently the case? May not, by this means, a *calculation*, if it must be so called, be mistaken for an *observation* made many hundred years before? It is confessed (*u*), that *Martini* was imposed upon in this very manner; and it is much to be suspected that he hath not been the only one.

You see, Sir, that I have produced the *Jesuits* own authority for every thing here offered. I designed to have cited their words at length, but that I found would have swelled these papers above the size of a letter. I am not conscious to myself of having misrepresented them; I am sure I have not done it wilfully; for I have nothing in my view but truth.

P. S. The title of the work I have here cited, is, at length, *Observations Mathématiques, Astronomiques, Géographiques, Chronologiques, & Physiques, tirées des Anciens Livres Chinois, ou faites nouvellement aux Indes, à la Chine & ailleurs, par les Pères de la Compagnie de Jesus*. It consists of three volumes, printed at *Paris*, 1729, 1732.

(*r*) *Observ. ut sup.* Tom. 2. Pref.
Pref.

(*s*) *Ibid.*

(*t*) *Obs. ut sup.* Tom. 2.

(*u*) *Obs. ut sup.* Tom. 2. p. 103.

Some account
of a curious
Tripes and
Inscription
found near
Turin, serving to discover the true
situation of
the ancient
city Industria;
by David Erskine Baker.
N^o 477
p 54. Aug.
&c 1745.
This is an abstract of the
paper, read
Dec. 5. 1745.

II. Dr *Joseph Laurentius Bruni*, F. R. S. and Physician of the college at *Turin*, having, in *March* 1744-5, sent from thence to my Father the description of a most curious antique *tripos* of metal, found, some little while before, together with a plate of the same, bearing an extraordinary inscription thereon, at a village called *Monteu*, on the right side of the river *Po*, about 16 miles from *Turin*; and the same Gentleman having lately sent us likewise an *Italian* dissertation printed at *Turin*, wherein the learned authors (*Paul Ricolvi* and *Anthony Rivautella*) undertake to discover, from the said inscription, and other concurring circumstances, the true place of the ancient city *Industria*, mentioned twice by *Pliny*; a short account, collected from the whole, and translated into *English*, may prove, I hope, not unacceptable.

This *tripos*, they say, far exceeds every thing of it's kind, preserved hitherto in any of the cabinets in *Europe*, as well for it's structure, as for the variety and elegance of the several *relievo's* wherewith it is adorned. Each of it's three pillars has on it four small figures: the first, which is placed at top, represents a *terminus* of *Venus*; the second is a *Victory*, or a winged *Fortune* rather, standing with her feet upon a globe; the third, which is near the middle of the pillar, is an harpy, winged, with a woman's face; and the fourth figure, at the foot of the pillar, appears to be an old *Silenus*, or *Satyr*, crouching himself together in an odd manner.

The pillars are joined to one another by little bars of metal, fastened by rivets at top, and rings at bottom, in such a manner that they may be closed together, or drawn asunder, at pleasure; and when they are extended to the utmost, the size of the *tripos* is somewhat more than a *Turin* foot, which, Dr *Bruni* says, is equal to 20 *English* inches.

Fig 1.

Fig. 1. is an exact copy of the inscription on the metal plate, as given before the Dissertation. The authors explain the reading of each word, and give their comments thereon, together with a long description of the *Roman* customs and offices; which being sufficiently known, I shall take from them only a brief abstract of such passages as are least easy to be understood.

The first observation of this kind is, that whereas, in other inscriptions, it is usual to find *honori*, and then the name of the person in the dative case, as *Honori Memmio Vitrasio Orfito* *; or else, *Honori Imperatoris Caesaris*, &c.; or as in another stone † in *Honorem T. Claudii Imperatoris*; and consequently the titles of the person in the genitive case; here, after having said, *Honori L. Pompei Herenniani*, we find *Curatori* and *Patrono*, which two words must again refer to the words *Genio* & *Honori*. And they mention another inscription, where a mistake of the same kind is found; viz. ||

* Grut. p. 443.

† Marm. Taur. p. 189

|| Marm. Pisaur. p. 17. n. 38.

GENIO ET HONOR
L POMPEI L F POLHEREN
NIANI EQ ROME EQ PVB
Q AER PETALIM AEDIL
II VIRO CVRATORI
KALENDARIOR REIP
COLLEGIVM PASTO
PHORORVM INDVS
TRIENSIVM PATRO
NO OB MERITA

I · GRAE · TROPHIMVS · IND · FAC ·

HONORI
M.GAVI.M.F
POP.SQVILLANI.
EQ.PVB.III.VIR.I.D
IIII.VIR.APVB
CVRATORI.VICETINOR
APPARITORES.ET
LIMOCINCTI
TRIBVNALIS.EIVS
EQ.ROM. EQ. PVB.

These words infer, that *Lucius Pompeius*, the person to whose honour this plate is inscribed, was a *Roman Knight*, who had a stipend from the public. The *Roman Knights* served at their own expence till the year of *Rome* 451, when their horses began first to be maintained at the expence of the commonwealth; and it appears, from various inscriptions under the Emperors, that the words, *Eques publicus*, *Equo publico donatus*, or *ornatus*, &c. always mean a military dignity, and must be distinguished from the *Roman Knights* towards the end of the commonwealth, who were a degree of citizens between the *Senators* and the *Plebeians*.

Q. ÆR. PET. ALIM.

It appears hereby that *Lucius Pompeius* was *Quæstor Ærarii*, tho' only of the finances of the city *Industria*, and not of the Emperor under whom he lived. But the greatest difficulty arises from PET. ALIM. where our authors suppose the Engraver may have left out the stop between the letter P and the letters ET; so that we should read it *Quæstoris Ærarii publici & Alimentorum*; and then we have two different dignities of *Lucius Pompeius*; that of *quæstor* of the public taxes, and that of *quæstor* of the provisions, both regarding the city of *Industria*. Several other inscriptions are also produced, to prove the office of *quæstor alimentorum*; and a great deal of reading is introduced, to shew, that the *quæstor alimentorum* was sometimes understood to be an officer having the care of the public allowance for bringing up children; and that at other times his office was understood to be the procuring all sorts of provisions for the use of the Emperor's troops.

Passing by his office of *Ædilis* and *Duumvir*, we find he presided likewise over the receipt of the taxes, by this address to him,

CVRATORI
KALENDARIORVM.REI.P.

The days fixed for payment of the taxes and debts were registered in the public *calendars*; and creditors usually demanded their interest on

the *kalends*, or first day of every month: whence the register of the debtors, and the sums due, or the tribute to be paid by particulars to the public, and indeed the general state of the debts, and credit of every community came to be called *calendarium*.

COLLEGIVM PASTOPHORORVM

The college of priests called *pastophori*, a name taken, as some suppose, from a very rich and ornamental upper garment termed *pastos*. As *pastophorus* was a name given to *Venus*, these priests may have belonged to her, or else to the goddess *Isis*, whose chief priests, as *Lucius Apuleius* informs us, were called *pastophori*, by way of pre-eminence, *Unus — cætu pastophororum, quod sacrosancti collegii nomen est, velut in concionem vocato, indidem de sublimo suggestu — renunciat, &c. ** He says also, that the god *Osiris* had a college of them. His words are, *Osiris — in collegium me pastophororum suorum, imo inter ipsos decurionum quinquennales elegit*. This body of priests had various offices, one whereof might probably be the conferring honours on persons of great merit, as we find from our inscription those of *Industria* had done on *Lucius Pompeius*.

INDVSTRIENSIVM

PATRONO

OB. MERITA

Hereby the city *Industria* acknowledges *Lucius Pompeius* as it's patron or protector, and shews it's gratitude for singular benefits received. Patrons and clients were in the earliest times of the commonwealth; but, under the Emperors, inscriptions shew us frequently, that cities and nations chose for their patron some eminent *Roman* citizen in favour with the prince, on whom they often conferred great honours.

T. GRAE. TROPHIMVS. IND. FAC.

These words on the cornice our authors wondered to find, as it was unusual for any but the most eminent Painters and Sculptors to put their names, and that only to the most famous and perfect of their works. Whence they conjecture, that this *Titus Graecus Trophimus*, of *Industria*, might be not only the Engraver of the inscription, but likewise the Sculptor of some image to which this may have been the pedestal.

These Gentlemen, who are authors of the *Marmora Taurinensia*, went to this village of *Monteu* in the autumn of the year 1743, where they found many inscriptions, with the names of various magistrates both civil and ecclesiastical; which were certain proofs that some considera-

* Lib. xi. *Metam.*

ble city had been in that place formerly : and returning thither the autumn following, they found a broken stone ; whereon, by putting the pieces together, they could plainly read, that there had been decreed to a person named *Cocceia*, at the expence of the public, a statue AB. IND. which they interpret *Ab Industriensibus*, and suppose to mean the citizens of *Industria*.

..... COCCEIÆ

HA . . . EC . . AB. IND.

..... FVNERE. PV.

ET. STATVAM.

Some peasants about the same time, digging in the plain between the hills near the *Po*, discovered the vestiges of an ancient fabrick, with some medals ; and, in the middle of the following *February*, found the traces of a large room, other medals, and some pieces of wrought brass ; and in *March* they discovered this plate and *tripas*. Our authors mention also the discovery of many medals, a mosaic pavement, the remains of an ancient temple, *basso-relievo's*, little images, ruins of edifices, and inscriptions found here ; and give two passages from *Pliny*, one whereof is *, *Ab altero (Apennini) latere, ad Padum amnem Italiae ditissimum, omnia nobilibus oppidis nitent ; Libarnia, Dertonia, Colonia, Iria, Bardarate, INDUSTRIA* : in the other his words are †, *Metrodorus dicit, quoniam circa fontem arbor multa sit picea, quæ Pades Gallice vocetur, Padum hoc nomen accepisse ; Ligurum quidem linguâ amnem ipsum Bodinicum vocari, quod significet fundo carentem. Cui argumento adest oppidum INDUSTRIA, vetusto nomine Bodincomagus, ubi præcipua altitudo incipit.*

In the first of these quotations the city *Industria* is spoken of, as one of the noble cities that flourished in it's time along the banks of the river *Po*, a little way to the south side of the *Apennines*. In the other he explains himself more fully, describing it to be near the *Po*, where that river begins to acquire a greater depth ; and, as a confirmation, gives it's name still ancients than that of *Industria*, viz. *Bodincomagus*, signifying in the *Ligustine* tongue the river's being deeper at that place. And our authors affirm, that, even at this day, the *Po*, above and near *Turin*, is hardly navigable ; but at *Monteu*, after having received not only the *Dora*, but the *Stura*, the *Orco*, the *Mallone*, and the *Dora Balteo*, it becomes much larger both in depth and width. They also take notice, that the hill near the plain of *Monteu* is called *Mondicoi*, which they suppose a corrupted remain of the ancient word *Bodincomagus*. They find likewise, in the bulls of this parish, that the parochial church is called *Sancti Joannis Baptiste de Lustria* ; which, they conjecture, may, by length of time, have been formed from the ancient name *Industria*.

* *Pliny, lib. 3. cap. 5.*

† *Ibid. lib. 3. cap. 16.*

Account of a Tripos and Inscription found near Turin.

From all these circumstances put together, they seem confident of their having discovered the real spot where this ancient city stood; and bring several reasons to prove, that *Casal* cannot possibly be the place, as some writers have imagined; and in order to shew more fully the grandeur, magnificence, and antiquity of this ancient city, they add the following inscriptions found at the same place.

A. HOSTILIO. A F.

PAP. PATRONO.

C. AVILIO. L. F.

P. GAVIANO.

FLAMINI. DIVI

CAESARIS

PERPETVO

PATRONO. MVNICIPI.

TRIB. MILIT. LEG. III.

GALLICÆ

D. D.

QVO. HONORE. CONTENVS

IMPENSAM. REMISIT.

N. MINIO. A. F. POL

..... ANNIVS

PRIM. ... IRO. PRÆF. EQ.

COH. VI. Q.

HONORIS. CAVSSA. LOCVS

EX. D. D. DATVS. V. F.

SIBI.

ET.

MINIÆ. ANN. F. ET. TVLLIÆ

VXSORI.

C. LOLLIO

C. LIB. PAL.

AGRAVLO.

COLLEG.

CENTONAR.

Q. H. C. I. R.

L. FVLFENIVS. T. F. SIBI

ET. L. FVLFENIO. L. F. SECVNDO

FILIIS. SVIS. V. F.

T. SIBI
EIO.P.F.NIGRIN.
V. F.

IMP.CÆSARO
AVGVSTO
D. D.

The last inscription proves the great antiquity of this city.

III. 1. Lately we have discovered (within 4 miles of this place *) many foundations in a plowed field, which have lain buried for many hundred years; and there are no records or tradition of it: it was discovered by one Mr *Hudson*, a Farmer at *Millington*, as he formerly tended his sheep on one side of the hill, and on the opposite side had perceived in the corn a difference in colour for some years before; which led him this summer to dig; and, happening on the foundations, it encouraged him, Dr *Burton*, and myself, to dig likewise in several places; and in one part was discovered a circular foundation five feet wide, and the plan within 45 feet diameter; by which I imagine it was a temple dedicated to *Diana*, said to have been at *Goodmanham*; but I could never, by many trials, find the least appearance of it there. The distance from *Goodmanham* to *Millington* is about 5 miles; and there were likewise many other foundations which had great quantities of *Roman* pavements within them; by which I imagine, after the dissolution of the temple it became a *Roman* station, then called *Delgovicia*; which has been so uncertainly fixed at *Goodmanham*, *Londesbrough*, *Hayton*, &c.: yet not the least remains ever appearing at any of those places, so as to satisfy an indifferent inquirer; but, in this just now discovered, the ruins and foundations are a demonstration of the once grandeur of the place; and no doubt it was the above-mentioned *Delgovicia*. The foundations lay about 18 inches below the surface, and to the depth of 4 or 5 feet within the ground.

Concerning
the situation
of the ancient
town Delgo-
vicia; in a
letter from
Mr Tho.
Knowlton to
Mr Mark
Catesby,
F. R. S. N^o.
479 p. 100.
Mar. & Apr.
1746. Read
March 6.
1745-6.

N. B. It is on the *Wolds* hills, within two miles of *Pocklington*.

Within a mile and half of *Kilham* is a place called *Danes Graves*, near which, it is supposed, was fought a great battle, in which infinite numbers fell; and so were laid in heaps, and covered with the chalky soil in little *tumuli*, of the quantity of two or three square yards; in which, if opened, one may find great quantities of human bones: though, at this distance of time, I believe there is not less than an acre of ground covered over with them, joining close to each other; and it is one of the greatest curiosities of antiquity, in my opinion, I have ever seen:

* *Londisburgh*.

I am determined one day to go and number them, and to measure the quantity of land they cover.

About five miles from thence, in *Rudstone* church-yard, there stands up, on the N. E. end of the church, a large stone, 30 feet above-ground, and what depth within is not known (tho' I intend next winter to dig and see the depth): neither can I tell upon what account such a mighty obelisk was brought over land so far as it must have been; because we



have no quarries of stone nearer than twenty miles from the place; all the wolds are barren of such materials: it is 5 yards about, like the figure in the margin. Dr *Childersley*, in his *Baconica Britannica*, takes no notice of any of these antiquities, in which I shall enter a short memorandum of what is remarkable.

These two last articles were not seen by me till *Midsummer-day*, when last I went to *Flambrough head*, which made them in my way.

A dissertation on the situation of the antient Roman station of Delgovitia in Yorkshire; by John Burton, of York, M. D. N^o. 483. p. 541. Mar. &c. 1747 Read May 28. 1747.

2. The learned Antiquarians have hitherto been greatly at a loss to find the place where the *Delgovitia* of the Romans really stood; some supposing it at one place, and some at another.

My worthy and learned friend Mr *Francis Drake*, in his excellent *History and Antiquities of York*, has given us every thing which has hitherto been written in support of the claim made by each place to the honour of rising out of the ruins of that antient town; together with his reasons for fixing that station at *Londesburgh*; all which I beg leave to recapitulate in as few words as possible, and to make some few remarks thereupon, before I proceed to shew where I think *Delgovitia* really was.

There are three places where the site of *Delgovitia* has been fixed at; viz. *Weigh-ton*, *Godmanham*, and *Londesburgh*. See the Map Fig. 2

The reasons offered for fixing it at *Weigh-ton* are 3; 1. From the supposed derivation of the name. 2. From something like a *tumulus* being at the east end of the town. And, 3. Because the distance from *Derventio* agrees with the Itinerary.

As to the first, the learned *Cambden* would have *Delgovitia* to be derived from the *British* word *Delgwe*, which signifies the statues or images of the heathen gods; therefore, as some persons would have *Weigh-ton* to be derived from something of the like cause, upon no other foundation, than that *Weigh-telbergh* in Germany is noted by *Conrad Celtes*, says Dr *Gale*, as a remarkable town in those parts.

Another person* says, that *Delgovitia* was called *Devovicia*, or *Delvovicia*; from whence take *Vic*, and add the *Saxon* termination *ton*, there is something like *Wigh-ton*; especially when we consider that the *Saxon* U and W were founded alike.

Both these (supposed) proofs for *Weigh-ton* will drop, when we consider, that an easier and much more probable derivation of it may be

* In the *Chorography of Britain*.

found out; the name being intirely Saxon, and is plainly derived from the Saxon word þeg, or þæg, *via, stratum*, a road or street; and from the verb þegan, *ire, transire*, to travel: the termination *ton* is obvious to all; so the Belgic or High-Dutch *Wech, Wegh, Weghe*, are the same with our *Way*, and signify the same thing. This is most likely, because *Weighton* now stands at the conjuncture of several roads, which here meet, and run from thence by *Kexby-Bridge* to *York*, and thence may be called *Weighton*, or *Way-Town*. The Roman military ways, both from *Prætorium* and from *Lindum*, took a different course, and went by *Londesburgh*, as I shall shew in the sequel; and the old road being turned this way, a new town sprung up, which took it's name from the occasion of altering the road.

The second reason offered to prove *Weighton* to have been *Delgovitia* is, because Mr *Horseley* observed something like a *tumulus* at the west end of the town. This alone is so weak an argument to prove this place a Roman station, that I shall take up no time in endeavouring to confute it. I must also remark, that there are not the least remains of any Roman road leading to *Weighton*; which there always are to any known Roman station.

The third reason offered, is, because this place agrees so near with the distance from *Derventio*, as mentioned in the *Itinerary*; but this argument must fall, when I shall shew in the sequel, that it is not true in fact, having had the road measured.

There is no other reason offered, why *Delgovitia* was where *Godmanham* now is, than that this latter place was said, by venerable *Bede*, to be *Locus Idolorum*, or a place of idols. Mr *Burton*, in his *Itin. Anton.* seems to lay a stress on the *quondam Idolorum Locus*, and says, It may allude, as well to Roman idols as Saxon: but this is too far strained; and we may justly enough conclude, that this was a temple neither of Roman structure nor worship, but a place dedicated to Saxon idolatry; such a one as is described in *Verstegan*, inclosed with a hedge instead of a wall: for we find the curious Mr *Drake* made a very strict enquiry, and could not perceive the least remains of any ruins; meeting with nothing but holes and hills, where chalk or lime-pits had been made.

There being not sufficient proof offered to fix *Delgovitia* here, I shall now proceed to examine what Mr *Drake* brings, to prove that *Londesburgh* may claim this honour, he being the first person who has attempted to shew it; and I must own, has brought much more substantial reasons to support his opinion, than have been mentioned in behalf of either *Weighton* or *Godmanham*. These are five in number; viz.

1. From the name of the place, *i. e.* *Burgh* or *Brough*.
2. The distance from *Eboracum* and *Derventio* will answer the calculated miles in the *Itinerary*, as well as *Weighton*.
3. Because the Roman road lay that way.
4. Because Roman coins are found there. And
5. Because there are repositories for the dead often found.

The situation of the antient town Delgovicia.

As to the first; That it might derive part of it's name, *viz. Burgh*, from a fortress on land, I agree to; and very likely there might have been some guards kept there, because it stands so high, that it commands the prospect of the whole country from thence to *Brough*, where the *Romans* used to ferry over to and from *Lincoln*; as appears by the military road on both sides the river. Here might, I say, have been a guard kept, to see that no enemy came on that side to surprize them in their station; and which indeed seems to be the more confirmed by the coins found here, as well as the number of human bones. Though this last is no certain proof; because a battle or skirmish might have been fought near or upon this place; which indeed seems to have been the case, so many bodies being found together; yet, when joined with other circumstances, it helps strongly to confirm his opinion. But I have not heard of any old foundations, ruins, or *Roman* pavements having been discovered in or near this place.

The second reason offered is, because it will answer the calculated miles in the *Itinerary*. This place comes nearer the *Itinerary* than *Weigh-ton*; but this point I shall wave discussing, till I come to treat of that place where I think *Delgovitia* stood.

The third reason is, because the military way led thither from *Brough*. This, tho' a proof that the *Romans* passed and repassed where that place now stands, yet is no proof of it's being a *Roman* station.

The fourth and fifth reasons I have already spoke to under the first head here offered; so I shall now proceed to prove, I hope, where *Delgovitia* really stood.

I shall not spend much time in speaking of the wisdom of the *Romans*, either in their military or political capacity: some few things however, tending that way, I am obliged to mention, in order to shew their motives for fixing a station in the place I hint at, and then I shall endeavour to bring my proofs that that station was their *Delgovitia*.

Among others, these were strong reasons for keeping the several stations; *viz.* to guard the passes, to keep the country in awe, and to have a considerable number of men together, to prevent a surprize from any foreign invasion, and sometimes to secure a supply of provision.

I don't know any part of the county of *York* that required so strong a guard as this place (strong it has been, is evident from the plan of the camp); because it is so situated, that *York*, for want of this station, might have been sooner surprized, either by any foreign enemies, who might have made a descent upon them, from either the *Humber*, or *German* ocean; or from an insurrection of the inhabitants of the *East-Riding*; both which are by this station sufficiently guarded against. It likewise gave them an opportunity of receiving and defending their recruits, either from *Rome*, or from any other of the southern parts of the island; who could either come by water, or cross the river from out of *Lincolnshire*.

As the guarding their passes was one strong reason for fixing stations, any person, by looking on and examining the annexed map, (*Fig. 2.*) and the draught of the camp, *Fig. 3.* will easily at one view, see why that place must be preferable for such a station; for it is just at the angle where 4 *Roman* military ways meet; so that, by guarding sufficiently that one pass, they secured all the four roads; which, had they been stationed either at *Weigh-ton*, *Godmanham*, or *Londesburgh*, they could not with the same men have done. This will be more evident, by drawing a line of the *Roman* road on the map of *Yorkshire* from *Brough Ferry* on the river *Humber* thro' *Londesburgh* park to *Malton*, and from *Stamford-Bridge* to *Pattrington* near *Spurnhead*; and then you will find that the roads cross each other betwixt or near *Millington* and *Wartre* priory.

Moreover the country itself, by nature, helped, with only little art, to make their camp at that time almost impregnable, the hills being from 60 to 90 yards perpendicular in height, and their sides very steep, which are very apparent in the plan.

From one part of this camp they could see a great way towards *Malton*, and all the way down the great vale of *York*, from near *Hambleton-Hills* to *Howden*, and from *Londesburgh* (where I doubt not but they kept a watch-guard) they could see all the way from *Howden* to *Brough-Ferry*. Hence they could not be surprized from the S. E. S. W. or N. W. quarters; therefore they had only chiefly the eastern sides to fortify; and how they have done that, the plan of their camp will better shew than I can describe.

Another reason for their fixing here is very evident; because, at the foot of the hill, not 100 yards from where the *Roman* pavement was, there are two springs of fine clear water, which, united, form what they call the *Beck*. These springs never fail, even in the hottest and driest summers (a rare thing to be met with upon the *Wolds*) and there is not another spring within two miles of that place, but what is either quite dried up,, or greatly diminished in a dry season; insomuch that at this day, in some seasons, the people are obliged to drive their cattle several miles hither for water.

From this situation their army could never want provisions, having a free communication either by land or water, with the southern parts of the island.

All that part of the plan of the camp marked *a. a. a. a.* describes deep vallies; from the bottom of which to the top of the hills are in general from 60 to 90 yards in perpendicular height, and the sides are very steep. All along the hills, from vale to vale are *Roman* works, represented in the plan at *b. b. b. b.*; so that nothing could pass that way, without the knowledge and consent of the guards. It must also be observed, that, of all the works, those guarding the parts toward *Bridlington* are the strongest; they being from 4 to 6 ditches in breadth, each of which are 10 or 12 yards broad.

The situation of the antient town Delgovicia.

At the places marked *c. c. c. c.* in the vallies, were watch-guards, kept to prevent any surprize, by the enemy attempting to get at the station that way.

All these works inclose 4185 acres of ground; whence it is evident here must have been a large army. You see in several places where their *tumuli* or barrows were, represented by little green hills.

Having shewn the fortifications and out-works of the camp, I will now prove the part within these on which *Delgovitia* stood.

About half a mile north-east of *Millington*, on the S. side of a gently sloping hill, were found several stone foundations of buildings of different sizes, and of different shapes; among which were found several fragments of *Roman* pavements, *Roman* tiles, flues, and two *Roman* coins, all or the chief of which are represented in their respective colours, by the direction of the strokes according to the rules of Heraldry, in *Fig.*

Fig. 3, 4, 5. 3, 4, and 5. These are all proofs of the buildings having been *Roman*. There was likewise dug up a piece of a large stone pillar, of about 6 feet in length, but of no regular order; which, notwithstanding, might yet be *Roman*; for we cannot suppose those military people so well skilled in architecture as the Artists at *Rome*.

If *Delgovitia* (as *Cambden* hints) be derived from the *British* word *Delgwe*, which signifies statues or heathen gods, this place may lay claim to a title on that account, much sooner than either *Weighton* or *Godmanham*; for here was dug up a circular foundation resembling a temple in all appearance; being within of 45 feet diameter, and the foundation was near 5 feet thick.

Near to this circular building, but S. of it, were the foundations of two oblong square buildings, but with a strait entrance, not 2 feet wide, wherein I apprehend they put in the fuel and fire for their sacrifices; there being evident marks of burning upon the stones, they being almost burnt through: moreover, in digging in the middle of these two buildings, we found about $\frac{1}{2}$ a yard thick of ashes, wherein were some few small pieces of wood, fuel, and pieces of brute-bones, chiefly burnt, and a great part of an horn of a large deer, which I now have by me.

East of these were laid open the foundations of another square building, wherein we found the pavements, coins, &c. represented in *Fig. 3, &c.*

The situation of these buildings was very strong, being guarded on the S. E. and N. E. by a deep vale, the sides of which are very steep.

From what has been said, I think there is nothing wanting now to prove this to have been the *Delgovitia*, but to reconcile the distance as mentioned in the *Itinerary*.

From *Eboracum* to *Derventio* M. P. VII.

From *Derventio* to *Delgovitia* M. P. XIII.

Total M. P. XX.

I had the road measured from *York* to the circular building or temple, Fig. 6. and the particulars were as follows; viz.

| | Miles. | Yards. |
|---|------------------|--------|
| From <i>York</i> to <i>Stamford-Bridge</i> , or <i>Derventio</i> , were | 7 | 154 |
| From <i>Stamford-Bridge</i> to the first beginning of the Roman works — — — — — | 7 | 209 |
| From the first barrier to the circular foundation | 3 | 132 |
| From the temple to the east side of the works is | 2 $\frac{1}{4}$ | 0 |
| Total | 17 $\frac{1}{4}$ | 55 |

This, tho' not exactly the same distance mentioned in the *Itinerary*, is yet nearer it than either *Londesburgh*, *Weighton*, or *Godmanham*; for *Londesburgh* is 5 computed miles farther than this place; and if we add 1 mile and a half more (they in general measuring $\frac{1}{3}$ more than computed at), then it will be about $6\frac{1}{2}$ miles; which, added to $17\frac{1}{4}$ miles, will make *Londesburgh* to be $23\frac{3}{4}$ miles from *York*; which differs more from the *Itinerary* than the place where I suppose the *Delgovitia* to have been. Both *Godmanham* and *Weighton* are still farther off, the last being 3 computed miles from *Londesburgh*; and if it measures $\frac{1}{3}$ more, then it will amount to $4\frac{1}{2}$ miles; which, added to $23\frac{3}{4}$ miles, will make the distance betwixt *Eboracum* and *Weighton*, by *Derventio* and *Londesburgh*, to be $28\frac{1}{2}$ miles.

I think this little variation from the *Itinerary* not to be an argument strong enough to prove this place not to have been the *Delgovitia*, when put in competition with the situation, &c. which at one view both of the map and plan will appear; considering at the same time, that the proportion betwixt the *Roman Mille Passuum* and our miles is nearly as 19 to 21.

From what I have said, I think it is evident, that neither *Weighton*, *Godmanham*, nor *Londesburgh*, stand where *Delgovitia* was. I have in the first place shewn the probability of this place near *Millington* being the station, from the known prudence of the *Romans*, because one set of men could defend the whole 4 passes; which could not have been done, had they been placed at *Weighton*, *Godmanham*, or *Londesburgh*.

2. I have shewn, that, from the very situation and nature of the country, there required but little art to make their camp, at that time, almost impregnable; the vallies in general being from 60 to 90 yards deep, and the sides thereof very steep.

3. That from this camp and *Londesburgh* they might see the whole country from the *Humber* on the S. E. up the vale of *York* on the W. towards the N. W. side; so that no army could surprize them that way.

4. That they could always have a sufficient quantity of provisions, and never want water, even in the hottest summers. And,

5. That there has been a *Roman* station here, as is evident from the *Roman* pavement, coins, tiles, and foundations of the ruins: and if the *Romans* had had a station at *Weigh-ton*, *Godmanham*, or *Londesburgh*, they would scarce have had one so near the other.

All these things concur in proving this to be the site of *Delgovitia*; and there is or can be no argument brought against it; except that, by the *Itinerary*, the distance from *Eboracum* by *Derventio*, is set down at XX *M. P.* and by our measure the distance from *York* to the circular foundation, in the camp, is only 17 miles, one quarter, and 55 yards; so that there is above 2 measured miles difference. In answer to this, I say, May not the *Itinerary* be as wrong here as in some other places; which is very evident in several instances? And as it is wrong in some others, I doubt not but it may be so in this: besides, the *Romans* might calculate from the center of *York*; and this mensuration only goes from the Bar at *Walmgate* to the circular foundation in the *Roman* camp. But supposing the *Itinerary* to be exactly right, yet, when the difference betwixt the *Roman Mil. Pass.* and our miles is calculated, I believe it will end all disputes on that score.

An Appendix
to the fore-
going paper;
by Mr Fr.
Drake, F. R. S.
Ibid. p. 553.

3. Time, which subverts and destroys the greatest works of mankind, hath an equal property of bringing things to light. The *DELGOVITIA* of the *Romans* in this country, so long sought after by *Cambden*, and other writers, as well as myself, is at length discovered so far, that there is no need of any more conjecture about it.

Being informed, in the year 1745, of some *Roman* curiosities found in a field near *Millington*, on the *Wolds*, Dr *Burton* of *York* and myself set out to survey them. On our coming to the place, an intelligent countryman and his father conducted us to a large plain field, on the S. side of *Millington* wood, where we were shewed several foundations of buildings under ground, on the very stones of which the apparent marks of fire may be traced. Two bases of pillars, of an irregular order, and a large piece of a column, were also discovered; several pieces of tessellated pavements, *Roman* bricks, tiles, &c. were dug up. The father told us, that, about 40 years before, he saw the foundations of a circular building, about fifteen yards diameter, dug up in this place; which must have been the vestiges of some *circus* or temple: that it had been the custom for the inhabitants of their village, time out of mind, to dig for stones in this ground when they wanted; and that they must often do, in a country almost clear of such materials. The church of *Millington* itself seems to have been built out of the ruins of this antient *Roman* station.

That this was really the *DELGOVITIA* so long sought after, I think, is beyond contraction. The distance from *York* coincides very justly with the *Itinerary*; 19 or 20 *Italian* miles agrees pretty well with our present computation; and at the same time points out the true military way from the *Humber* to *York*. Instead of forcing a road thro' the vale, the *Romans* very wisely chose to mount the hills as soon as possible;

possible; and therefore directed their *stratum* from *York* to the ford, over the river *Derwent* at *Stainfordburgh*; and from thence in a direct line to *Garrowby* hill; which I take to be corrupted from *Barrowby*, many of those *tumuli* or *barrows* being near this place. On the top of this mountain, as I may well call it, though the road turns up it by an easy ascent, begins a series of such enormous works for fortification, as the like is not to be met with in the whole island.

This road on the summit of the hill in a strait line points directly for *Sureby* or *Burlington-Bay*, the *SINUS SALUTARIS* of *Ptolemy*. But another road to the right takes a different course, and comes down to the ruins I have before mentioned. From thence the road leads directly to *Londesburgh*, the place which I once thought the station sought for: it passes thro' Lord *Burlington's* park, where more of it was laid open last year than I had before seen, in widening the large and noble canal in that inclosure. This place was before a morass, and the *Romans* were obliged to force a way through it, which is eight yards broad, and laid with stone edgeways to a great depth. The road passed up the hill on the other side this marshy place, and divided into two branches on the top of it; one way pointing through *Weighton* to *Brough* on the *Humber*, and the other by the east end of *Godmondham* directly for *Beverley*; which now I am convinced also was the *PETVARIA* of *Ptolemy*. From which last station it must have gone out directly for *Patrington* or *Spurn-head*; one of which was certainly the *Roman PRAETORIUM*, mentioned as the last stage in the first itinerary route of *Antoninus*.

This sea-port must be very commodious to touch at, either going or returning from *Gaul*, or the *Belgic* coasts, and bringing military stores, &c. from thence, either to *York* or *Malton*; to which last place the *CAMOLODVNUM* of *Ptolemy*, another road branches out, apparently from the conjunction on the top of *Garrowby-Hill*, and leads directly to it. But to return to our *DELGOVITIA*.

The situation of this place is admirable, and the stupendous works about it, thrown up for a defence to this station, and the several grand roads near it, are not to be described. The town itself was placed on a declivity of a hill, almost full S.; and very near it's ruins arise some rapid springs of excellent water; and so copious, as, when joined in one stream, turns a mill; from which I suppose the name of *Millington* has proceeded. There was also lately discovered a well above a mile E. & S. from these springs, dug thro' the solid rock, 26 dards deep, which must have been a *Roman* work.

To the S. W. there are no ramparts thrown up; but to the E. N. E. and due N. the whole country is full of them. The vales are all of them guarded by small encampments at their angles; the vestiges of the barracks, now visible, are called by the country people the *camps*. These were to prevent any sudden surprize that way. On the hills, from vale to vale, some of which are from 60 to 90 yards deep, and prodigious steep, are thrown up works, as ramparts, 12 yards broad, and proportionably

portionably high, which join in right angles with the vallies, and serve as a strong barrier every-where. Whether they had any palisadoes upon them is uncertain: but without them they are capable of stopping an enemy; which they had cause to be apprehensive of, by it's neighbourhood to the *German Ocean*, from whence the *Saxons* gave them many alarms.

Dr *Burton* was at the expence to have the whole of these works measured and planned out; a map of which has been exhibited to the *Society*; but, upon my shewing a correct copy of the draught to Lord *Burlington*, his Lordship ordered me to get the survey of the road leading from thence through his park at *Londesburgh* to the division into two added to the former plan; by which a course of some miles, and many thousands more acres on both sides the road are given.

An account
of a Dissertation published
in Latin by
Dr Weidler,
F. R. S. in
1727, concerning the
vulgar Numeral Figures.
As also some
remarks upon
an inscription,
cut formerly
in a window
belonging to
the parish
church of
Rumsey in
Hampshire:
by John Ward,
F. R. S. Rhet.
Prof. Gresham. N^o.
474 p. 79.
June &c. 1744.
Read June 7.
1744.

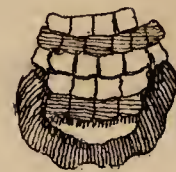
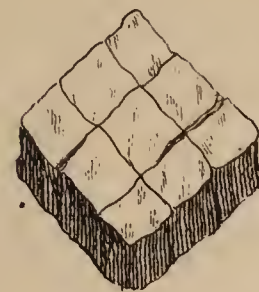
IV. 1. Some years since I had the honour to lay before an assembly of this learned body two papers, concerning the antiquity and use of the *Arabian* or *Indian* figures, and more especially in relation to *England*. And those papers, being afterwards published in the *Philosophical Transactions* *, occasioned the learned Dr *Weidler*, Professor of the Mathematics at *Witemberg*, and a Member of this *Society*, to transmit to Dr *Mortimer* a *Dissertation* he had formerly printed upon that subject †. Which discourse coming before the *Society*, they were pleased to refer it to my perusal and consideration; of which the following is a brief account.

The author begins his discourse by observing the great inconveniences, that the *Antients* laboured under in their arithmetical computations, which were usually made with the letters of their several languages, differently applied in different countries. And he thinks it very strange, that, when it was always the custom to distinguish their numbers by *decades*, they should not more early have fallen into the method of using only ten different characters, by means of which the largest sums are now computed with so much ease and expedition. But the *Romans*, as he observes, had some assistance from their *abacus*, or *counting-table*; a description of which, with the use of it, he has given from *Velfer*, by whom it was first published, and afterwards by *Gruter*, and others ‡. And a draught of the table may be seen also in the *Philos. Transf.* Number 180. I would therefore only beg leave to make a short observation or two concerning it. And first I imagine, that the \ominus , which is placed between the two series of rings on the right-hand, may stand for the Greek word $\frac{\sigma}{\rho}\alpha\acute{\sigma}\mu\alpha\tau\alpha$, *fractions*; as that order of rings denotes *ounces* or *parts* of the several following *decades* towards the left hand, which

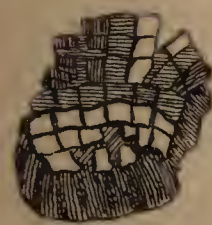
* See Vol. IX. Part iv. Chap. i. Art. x.

† *De characteribus numerorum vulgaribus, & eorum ætatibus, veterum monumentorum fide illustratis, Dissertatio mathematico-critica, a Joan. Frid. Weidlero, J. U. D. & Mathes P. P. &c. Witemb. 1727. Quarto.*

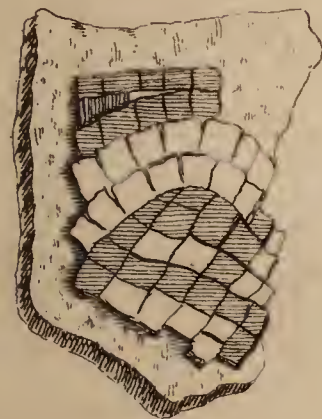
‡ *Grut. Inscript. antiq. ccxxiv. Pignor. De Servis, p. 344, ed. 1674. Octavo.*



Fragments of the



Mosaic



Pavements.

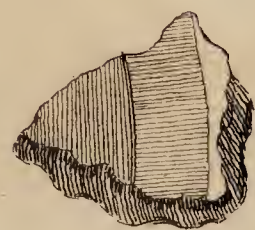


Fig. 5.

from York to
Stanford Burgh



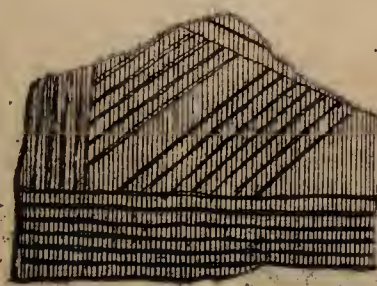
Part of a Flue



Fig. 4.

A Scale of Inches
for the Pavements

and
Tyles.



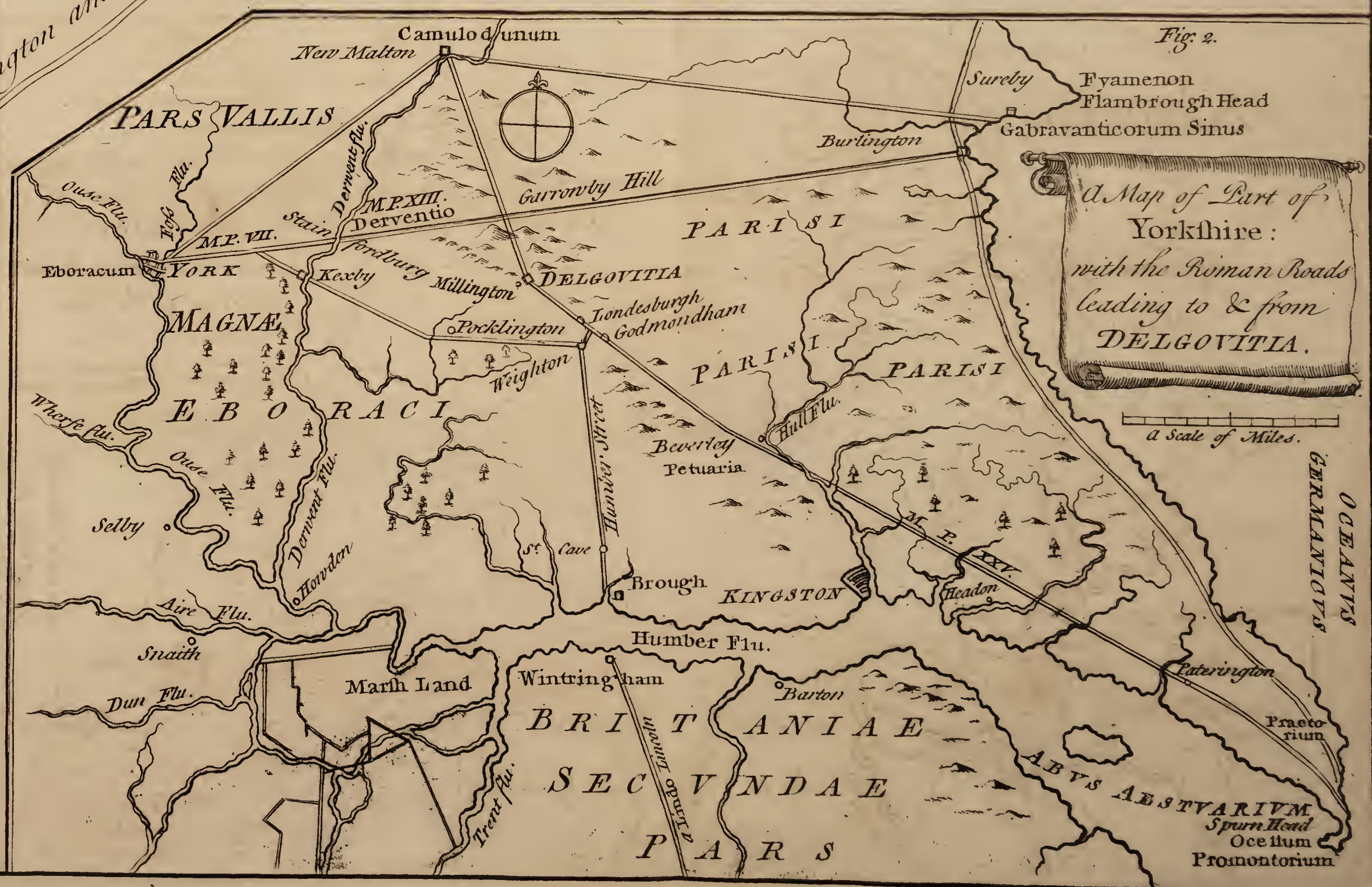
YORK



Bridlington and Flamborough Head

Fig. 6.

A Scale of Miles.



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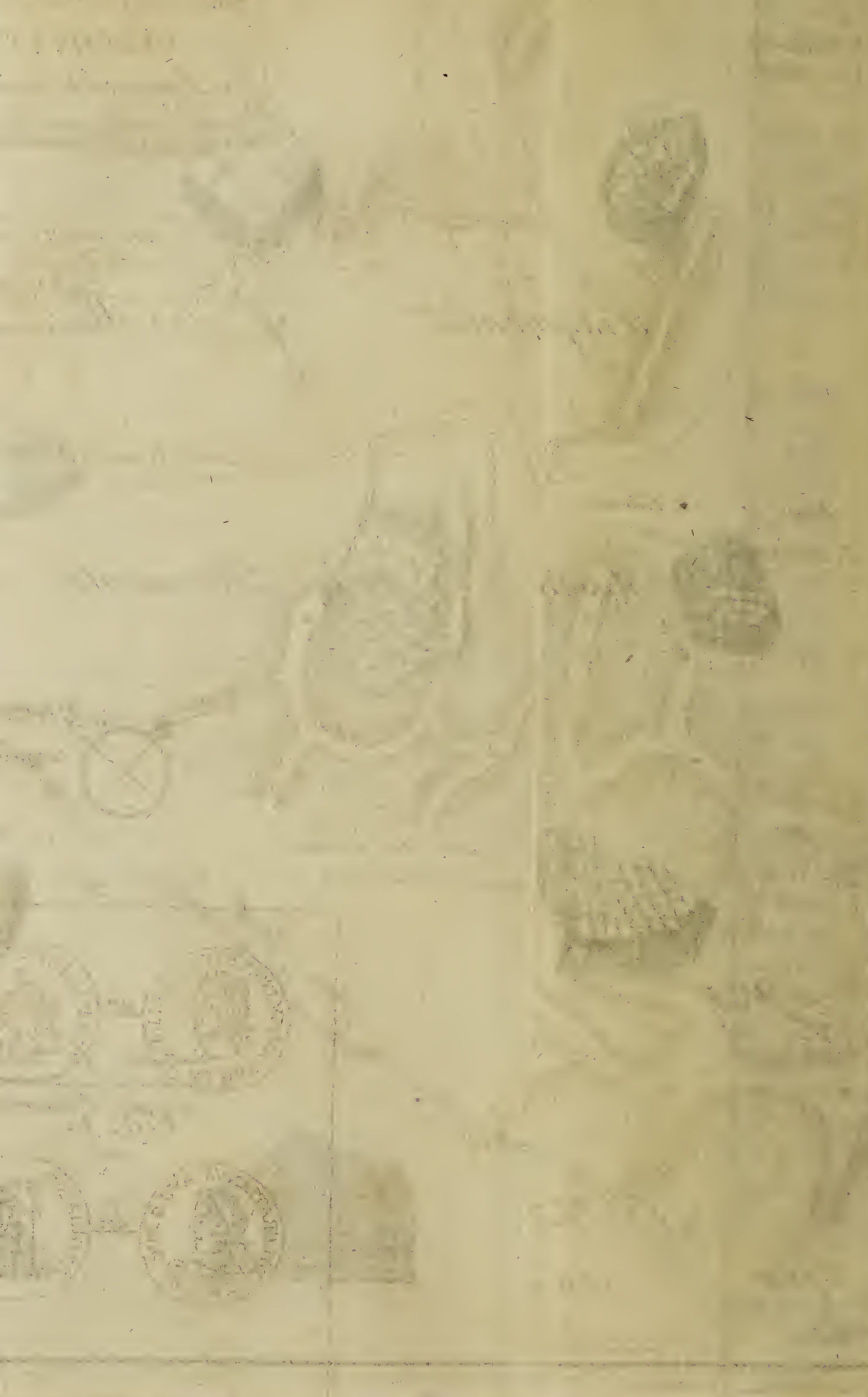


Fig. 3.

Part of the general Vale of York.

A Survey of the
Roman Works in Yorkshire
where the Ancient Town of
DELGOVITIA
is Supposed to have stood.

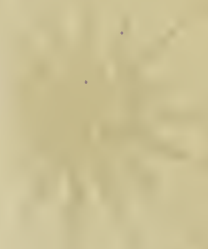
aaaa. Vallies markt in the
Original with Green.
bbbb. Barriers markt Yellow.
cccc. Watch Guards markt red.



Fig. 3.



Explorations of the



The following is a list of the names of the persons who accompanied the expedition, and a description of the route taken, and the results of the journey.

The names of the persons who accompanied the expedition are as follows:

1. The name of the first person is [illegible].

2. The name of the second person is [illegible].

3. The name of the third person is [illegible].

4. The name of the fourth person is [illegible].

5. The name of the fifth person is [illegible].

6. The name of the sixth person is [illegible].

7. The name of the seventh person is [illegible].

8. The name of the eighth person is [illegible].

9. The name of the ninth person is [illegible].

10. The name of the tenth person is [illegible].

The route taken by the expedition was as follows:

1. The expedition started at [illegible].

2. They then traveled to [illegible].

3. From there, they went to [illegible].

4. They then traveled to [illegible].

5. Finally, they reached [illegible].

The results of the journey were as follows:

1. The expedition discovered [illegible].

2. They also found [illegible].

3. The results of the journey were [illegible].

are all *asses* or *integrals*. Nor was it unusual with the *Romans* to make use of single *Greek* characters on some occasions; as we use the *Latin* letters, *l. s. d.* for *pounds, shillings, and pence*. I would further remark, that the four rings, placed by themselves on the right-hand of the former, are doubtless parts of the ounce, as *Velfer* explained them. But, whereas *Peireskius* (as our author observes from *Gassendus*) thought *Velfer* was mistaken in calling the two lowermost of them *duellas*, or *thirds of an ounce*; which he rather took to be *sextulas*, or *sixths*: I cannot but differ from both those opinions, since they make this order of rings to disagree with the rest of the table. For in each of the other orders the several rings, differently disposed, are suited to express any number of parts contained under it; and all of them together make one short of the whole. Thus it is in the several *decades*; and the rings for ounces may be so placed separately, as to express any number under 11; and all of them united will make that number, which falls short of the ounce by one. But in these parts of the ounce, if the two undermost rings are taken for *thirds*, they will not apart express either the number one or two, nor by any union the number 5; and, if considered as *sixths*, they will no way denote the number one. And besides, in either case, the whole number together will exceed 11; that is, one short of the parts, into which the ounce was divided: which being an integral to these, as the *as* was to the ounce, such parts of it were doubtless designed to be given here, as would correspond with the rest of the table, in the manner already explained. I apprehend therefore, that the two lowest rings were intended for what *Volusius Maecianus* calls *dimidias sextulas*, and *duodecimas* *; that is, the *twelfths of an ounce*; which, with the other two above them, will express any part of the ounce from 1 to 11, and so render the whole table consistent with itself.

But I return to our author, who employs the remaining part of his Discourse in treating of the antiquity and use of the *Arabian* or *Indian* figures. And here he has given a very particular and accurate account of the different opinions of several writers upon this subject, but more especially of what *Kircher* and Dr *Wallis* have said concerning it. The former of whom, as he observes, ventures to fix the precise time, when the *Europeans* learned them of the *Arabians*; which was occasioned by the assembly called together by *Alphonfus* King of *Castile*, for settling the *astronomical tables*, at which some *Moors* or *Arabians* were present. Now in those tables, which were finished and published in the year 1252, the numbers are expressed in these characters. *Kircher* thinks likewise, that the *Arabians* first borrowed them from the *Indians* about the year 900; when, having subdued *Persia*, *Carmania*, and the coast of *India*, they opened a commerce with that country. On the contrary, Dr *Wallis*, as our author remarks, has shewn, that these figures were known to the *Europeans*, and used by them in books of *Astronomy* and *Arithmetick*,

* See Gronov. *De Sestertiis*, p. 397.

long before the time assigned by *Kircher*. But, as *Dr Wallis* suspects, that the characters found in some old editions of *Boëthius de Geometria*, very like the *Arabian* figures, are different from the original, or other antient manuscripts of that work; our author acquaints us, that he himself saw in the public library of the university at *Altorf* a copy of it, which by the form of the letters appeared to him to have been written in the eighth or ninth century; and that both the shape and situation of the numeral characters were the same, as in the first edition printed at *Venice* in 1492. He thinks therefore, that they might be the same, as in the original of *Boëthius*; and endeavours to shew, that they were then used in much the same manner, as the *Arabian* figures now are, in sums of multiplication and division. And from thence he concludes, that such characters must have been known in *Europe*, as early as the beginning of the sixth century; since *Boëthius* was put to death by *Theodoricus* King of the *Goths*, in the year 524. As to the objection, which may be made to this opinion, from the silence of writers concerning it for several ages after *Boëthius*; he observes, that the same has happened in other instances of a like nature. Though he supposes, that both the characters themselves, and the use of them, was a secret at that time, known only to Philosophers and men of learning, and not introduced into the common affairs of life; and that the first invention of them was owing to the eastern nations, from whence they came to the *Greeks*, among whom the *Pythagoreans* were particularly remarkable for concealing their knowledge from the vulgar, and imparting it only to their followers.

For the illustration of his Discourse, the author has prefixed to it a table of numeral characters, taken from writers of different ages and countries; together with the *Helmdon* date, in the explication of which he follows *Dr Wallis*: but he offers nothing further, so far as I could observe, in relation to the common use of them, more early than what I have remarked in my former papers. With this learned *Dissertation*, he transmitted likewise to *Dr Mortimer* a small brass quadrant, with the numbers engraven upon it in *Arabian* figures, and the date when it was made, namely, 1306. In this quadrant all the figures agree with those of *Johannes de Sacro Bosco*, except the 2; which in him is inverted thus, 2*, but on the quadrant has the present form.

I had long since delivered in this paper; but that a reverend and learned Gentleman having communicated to the Society “An account of an antient date in *Arabian* figures, upon the north front of the parish church of *Rumsey* in *Hampshire*” †, I was desirous to get the best information I could concerning it, in order to lay my thoughts of it before them at the same time, to prevent the giving a double trouble. But upon a very strict inquiry I could not, for a great while, learn any thing further about it. And *Daniel Wray*, Esq; a worthy

* See Vol. IX. Part iv. Chap. i. Art. x.

† See Ibid. Art. xii.

Member of this *Society*, was pleased to inform me, that being at *Rumsey* he had examined all the parts of the church carefully, but could find no such date, nor any thing that resembled the draught, which accompanied the account. However, at length having by the favour of the Reverend Mr *Richard Newcome*, Rector of *Bishops Stoke* in that county, procured the model of a window, containing an inscription not unlike that in the draught, I now take leave to communicate the same, together with a drawing of it, as in *Fig. 7.* It was found at *Rumsey*, where *Fig. 7.* it served to stop up the window of a stable in an empty inn, to which place it had probably been long before conveyed; since none of the inhabitants remembered it's being taken out of the church, and nothing could then be discovered there, which bore the least resemblance to it. Upon shewing it to a very skilful architect, he immediately said, it was the model of a church window; and that it was the custom formerly to have such models made for the use of the masons. Some time after I desired Mr *Peter Newcome*, now a Member of this *Society*, to convey a copy of the draught above mentioned to his brother, the Reverend Mr *Benjamin Newcome*, who was then in that neighbourhood, and from whom he soon received the following account: "There was a window
" in the north front, that fell down thirty years since, which, the sex-
" ton tells me, he thinks something resembled the draught (for I shew-
" ed it to him) as well as he can remember. That window was brick-
" ed up for 23 years, but about 7 years ago was repaired and glazed." But, in the same letter, speaking of the present state of it, he says:
" I viewed the north front, and could not see any building or win-
" dow, either without or on the inside, that in the least resembled the
" draught." From the sexton's account therefore it seems to me not improbable, that there was formerly a window somewhat in that form, but larger, in the north front, and that the draught sent to the *Society* contains only part of it; which being considered barely as a date expressing the year 1000, the rest was not attended to. But, upon comparing it with the model, I could not enter into that sentiment; tho' I had not drawn up my thoughts upon it, when I first found it printed in the *Transactions* *. And I hope, what I now offer, will be esteemed only as an inquiry after truth; which in things of this nature can often rise no higher than probability, wherein every one is at liberty to determine, as he sees cause.

I suppose therefore, that formerly there was a window in the church made after this model; though the place, where it stood, cannot now be determined, there having been 40 windows stopped up in that church, as Mr *Newcome* informs his brother in another letter. And more than one window might be built at first by this model, or with a little variation from it. Wherefore the characters being, as I apprehend, the same both in the model and printed draught, the same interpretation may

* Ibid.

serve for both. Accordingly, I take the upper ones to stand for the letters *ih̄s*, with a stroke cross the top of the *h̄*, being a contraction of *ihesus*, as that name was antiently written in *English*. And the characters below these, which have been taken for figures, when put together, make the name *ion*, as it was likewise formerly written, without an *h̄*. Both which words are so spelt in *Wiclif's English* version of the *New Testament*, published by the Rev. Mr *Lewis*. And doubtless, had the last character been designed to express the number *ii*, the two strokes would have been kept separate, as they appear in the printed draught; and not been joined both at the top and bottom, in the form of the letter *ii*, as we find them in the model. This reading agrees very well, with what Mr *Newcome* mentions in his first letter; that he heard some persons at *Rumsey* say, the church had been dedicated to St *John*. It seems therefore not improbable, that the pictures both of *Christ* and St *John* were formerly painted either in that window, or near it. And this opinion I am the more confirmed in from Mr *Newcome's* account in the same letter, that there are three crucifixes yet remaining about the church, notwithstanding the many windows now stopt up, and other great alterations made in it at different times. One of these is painted on a window behind the communion-table, and represents *Christ* bearing his cross. The second is on the south wall, the figure of which is 5 feet 3 inches high, and represents him in the usual manner upon the cross, with his arms extended, and a hand above pointing downward. It stands near the ground, and appears very antient. The third, which is very rudely drawn, is on the outside of the church, near the top of the west front, in the same posture as the second, accompanied with the six following figures. On the top of the cross are two angels. On the sides are two other figures in long garments, designed probably to represent his mother, and St *John*, to whose care he committed her, as we find recorded in his Gospel*. Below are two soldiers, one on the left side holding up a reed with a sponge at the top; and the other on the right piercing his side with a spear, which latter circumstance is likewise mentioned only in the Gospel of St *John*†. As the model is cut out of an oaken board, it may probably, notwithstanding its thinness, be old enough to consist with the time of that spelling; but that it can be so antient as the year *1011*, some experienced workmen, to whom I shewed it, think it wholly incredible.

The explication here given may be further confirmed by the symbolical figures underneath, which seem to be designed as an emblem of the Trinity joined with the cross. And it is well known, that St *John* has treated more largely upon the Divinity of *Christ*, than any of the other Evangelists. *Plutarch* informs us, that *Xenocrates* the Philosopher resembled the Deity to an equilateral triangle, the Genii to an

* Chap. xix. 26, 27.

† Chap. xix. 34.

isosceles, and men to a scalenum (a). And a triangular figure has been since applied by Christians to represent the Trinity, sometimes singly, and at other times with additional lines expressing a cross, as in this model. So we find them variously combined upon the medals of the Popes published by Bonanni (b). And nothing was more frequent formerly with Printers, than to place these complex figures in the front of their books, at first doubtless with a religious intent, till at length by common use, and being joined with other devices, they became only press-marks, and badges of distinction among the trade; as they now are with merchants, who mark their goods with them both here and abroad.


A like event has happened to another character, made up of the Greek letters X and P joined in this manner  which we first meet with in some large brass coins of the Ptolemies, Kings of Egypt, where it was placed on a civil account. Some writers have taken it for a date, and others for the initial letters of a proper name (c). But as no reasons are assigned for either of those conjectures, I would rather suppose it an abbreviation of the word XPHMA, money, impressed on those pieces to denote their currency as money. Which might be thought proper, as they have not the heads of the Kings stamped upon them, like their silver and gold coins; but always that of Jupiter on the front, and an eagle perched on a thunder-bolt upon the reverse (d). And in that respect they seem to agree with such brass medalions of the Romans, as we find stamped with the letters SC; which in the opinion of a judicious Antiquary were designed to intimate, that after they had been dispersed as largesses to the populace, they were to pass for money, like the common brass coins marked with those letters by the authority of the Senate (e). An impression of one of those Greek coins above mentioned is here annexed Fig. 8. with which I was favoured by our honoured Pre-
sident. And it is well known, that the monogram, which appears on it, was afterwards applied to a very different purpose by the Emperor Constantine the Great, who made use of it to denote the name XPICTOC, and placed it both on his coins and military ensigns; wherein he was followed not only by some of the succeeding Emperors, but also by private persons, who out of devotion put it on their lamps and other utensils (f). But afterwards it came to be used merely as a critical note, to point out remarkable passages in manuscripts; as in later times an hand, with the fore-finger extended, has been placed for the like

Fig. 8.

(a) Παράδειγμα ὃ τῷ λόγῳ Ξενοκράτης μὲν ὁ Πλάτωνος ἐταῖρος ἐποίησατο τὸ τ' τετραγώνον, δείω μὲν ἐπικύσας τὸ ἰσόπλευρον, δητῶ δὲ τὸ σκαληνόν, τὸ δὲ ἰσοσκελὲς δαιμονίῳ· τὸ μὲν γὰρ ἴσον πάντῃ· τὸ δὲ ἀνίσον πάντῃ· τὸ δὲ πῇ μὲν ἴσον, πῇ δ' ἀνίσον, ὥσπερ ἡ δαιμόνων φύσις ἔχουσα καὶ πάθος συντεῖ καὶ θεὰ δύναμιν. De defectu oraculorum, edit. H. Steph. 1572. Vol. I. p. 740.

(b) Numismata Pont Rom. ed. Rom. 1699. fol.

(c) Pignorius, Epist. 24.

(d) See Suidas in the word χρημάτων.

(e) La Science des Medailles, Tom. I.

pag. 211. ed. 1739.

(f) Casalius, de sacris Christian. ritibus, p. 227. Pignorius, de Servis, p. 32. ed. 1674. οὐδ' αὖτο.

purpose in the margin of printed books. And then it stood for the initial letters of the Greek word XPHCIMON, *useful*, as we learn from *Isidore* *. Other instances might be produced of the like nature, wherein things have deviated from their original use, and served different purposes; but I forbear giving further trouble to this Assembly on such minute matters, and hope the nature of the subject will plead my excuse, for what has been said already.

I shall only beg leave to communicate the copies of two antient dates in *Arabian* figures, which were imparted to me by the Rev. Dr *William Warren*, senior Fellow of *Trinity-Hall* in *Cambridge*. They were both taken by himself, and are of the same size with the originals. One is cut on a beam running from the north-east corner of the steeple to the school in the church of *Ashford* in *Kent*, and expresses the year 1295. The other is cut in a beam (over a great passage) that is part of a very old house at *Cambridge*, called the *Half-Moon*, near *Magdalen-College*, and denotes the year 1332. The figures of both are very rude, agreeable to those times, being the oldest I have yet met with, except those at *Helmdon* †.

Fig. 9.

Fig. 10.

A brief inquiry, by John Ward, F.R.S. & Rhet. Prof. Gresh. into the reading of two dates in Arabian figures, cut upon stones which were found in Ireland; communicated to the R. S. Nov. 10. 1743. and Dec. 6. 1744. N°. 475. p. 283. Jan. &c. 1745. Read Feb 28. 1744-5. Fig. 11.

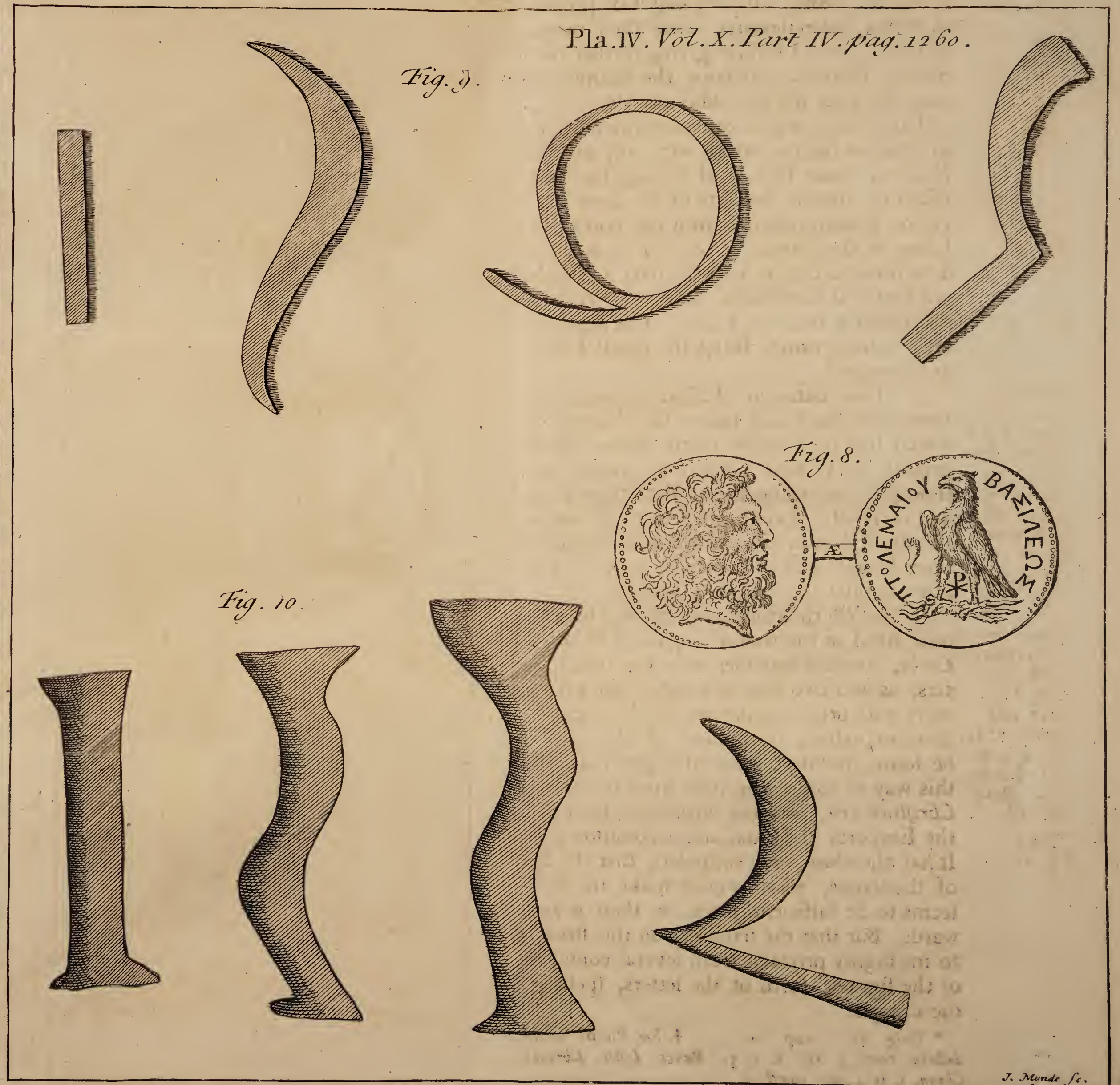
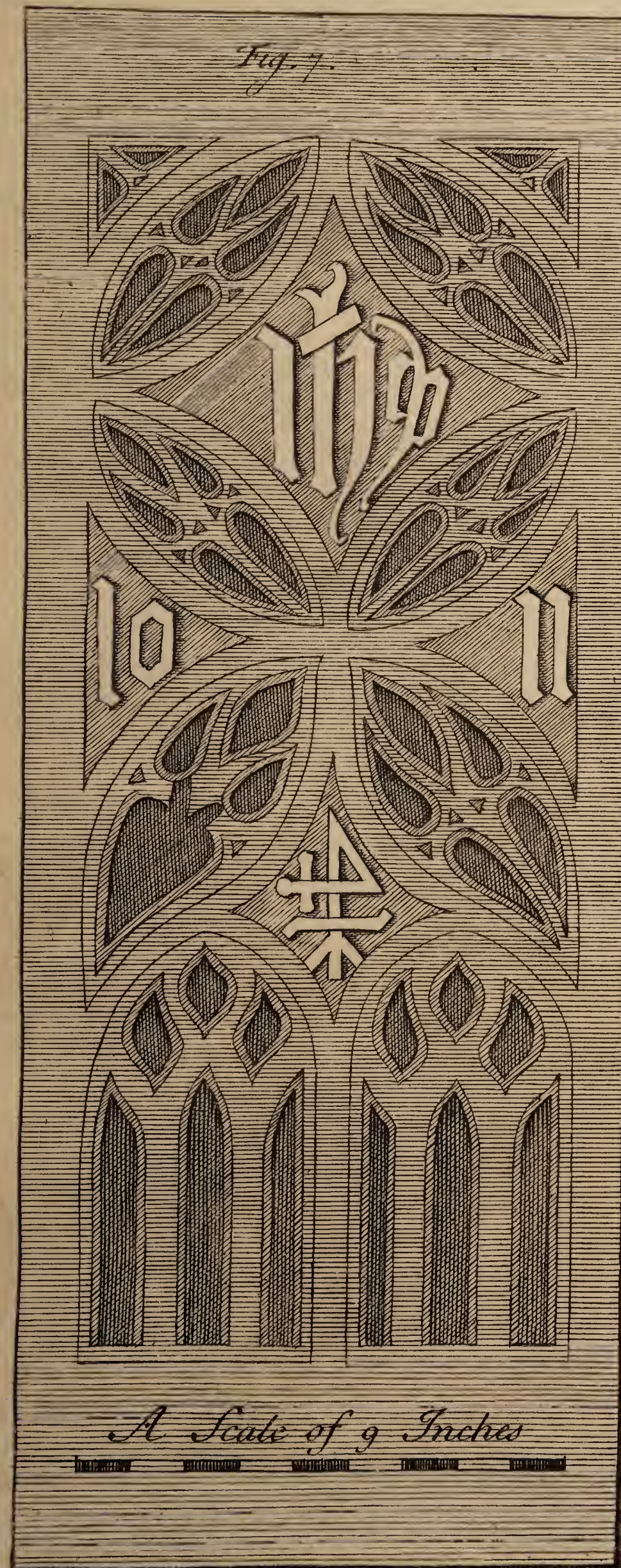
2. Two dates in *Arabian* figures, transmitted from *Ireland*, were some time since laid before this learned Society. But as the reading, which had been given them, seemed doubtful to the Gentlemen then present, they were pleased to honour me with desiring my further thoughts concerning them. Both those dates are said to be cut on stones, and in relief. I considered them as carefully, as I was able, and after the strictest examination could see no sufficient reason to think either of them so old, as had been represented. For which opinion I now take leave to offer the following reasons.

I. One of the stones, which was found in the Friars Abbey, and is now fixed in the wall of a garden belonging to Alderman *Baldwin* in *Corke*, contains together with the date several words cut in capital letters, as also two human images (one a trumpeter and the other a drummer) with other ornaments and decorations. (See Fig. 11.) The date, as there expressed, runs thus: *A D. 158*. But it is evident, here must be some mistake; since it is generally allowed by Chronologers, that this way of computing time from the birth of *Christ*, which is called the *Christian æra*, was not introduced till the 6th century, in the reign of the Emperor *Justinian*, and is commonly ascribed to *Dionysius Exiguus* ‖. It has therefore been supposed, that the figure 1 is omitted in the place of thousands, which would make the date 1158; and for which there seems to be sufficient place, as there is not for any other figure afterwards. But that the sculpture on this stone cannot be so antient, appears to me highly probable from several considerations, taken from the shape of the figures, form of the letters, spelling of the words, and dress of the images.

* Orig. lib. 1. cap. 20.
doctrin. temp. l. xii. c. 2, 3.
Chron. l. iv. c. 40. quest. 4.

† See Philos. Transf. n. 439.

‖ See Petav. de
Bever. Instit. Chronol. l. ii. c. 10. Strauch. Breviar.



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As to the figures, I have never met with the 5 any thing like the shape of it upon this stone (which comes pretty near the modern form) till the 14th century; except in one single instance of a date 1295, * which I had the honour to communicate to this Society, June 7. last. In the *table of characters* prefixed to N^o. 439, of the *Philosophical Transactions* the figure Five is given from 3 writers of the 13th century, in two forms both very different from that upon this stone. One was taken from *Maximus Planudes*, a Greek writer, which is like the β of that language inverted in this manner Ϸ; and the other from *Johannes de Sacro Bosco* and *Roger Bacon*, which is made thus 9. The latter of these continued in use till the beginning of the 16th century, as appears from a manuscript preserved in the *Inner Temple* †, bearing date the XXI Yere of King Henry the VII, and the Yere of our Lorde 1509; to which is prefixed a *calendar*, wherein all the figures are like those of *Roger Bacon*.

The letters in this sculpture are mixed, being partly *Roman*, and partly *Saxon*; as we often find, that the workmen took great liberties in varying and mixing their letters. Those of the latter sort are G, D, N, and O, the shape of which may be seen in the draught of this sculpture. The first of them occurs in several coins of *Henry VIII*. *Edward VI*. and *Queen Mary*. And the 3 last are found together, cut in relief over the eastern gate of *Lincolns-Inn*, in the following date, ANNO DNI 1518, the form both of the letters and figures agreeing likewise with those of the sculpture. (See Fig. 12.) I shall only add, Fig. 12. what our celebrated Antiquary, Mr *Camden*, has observed, that the *Saxon* characters were used in *Ireland* in his time ‖. Nothing therefore appears in the letters, which can necessarily carry the antiquity of them higher than the 16th century.

Nor is there any thing in the spelling of the words, but what agrees with the manner of writing in that century, more especially while the orthography of the *English* language continued so various and uncertain, as it did for the greatest part of it. The only words, that call for any remarks are *fere* for *fear*, or *feare* with *e* final, *dow* for *do* or *doe*, and *shall* and *well* with a double *ll*. As to the first of these, we then often find the *a* omitted in words of that form; as *clene*, *clere* *clerely*, *nere*, *yere* *yerely*, and the like. And as to *dow* for *do* or *doe*, I meet with *dowthe* for *doeth* or *dotb*, and *gowlde* for *golde* or *gold* §. And such monosyllables, as *shall* and *well*, were in the former part of that century

* See *Phil. Transf.* N^o. 474. p. 91.

† This they call their *Grace Book*, because it contains, among many other things, graces to be used before and after meals.

‖ *Britann.* pag. 730. edit. 1607.

§ See Sir *Richard Gresham's* Petition to King *Henry VIII*. *Cotton Libr. Cleop. E. 4.* Sir *Tho. Gresham's* Memorial to *Queen Mary*. *Ibid. Otho, E. X. 3.* His Memorial to *Queen Elizabeth* in 1558, found among Lord *Burghley's* papers, and now in the hands of *James West*, Esquire.


more commonly written with a single *l* than double *ll*, but afterwards promiscuously, as may be seen by the Bibles printed in those times.

With regard to the images, King *Philip* is the first, whom I have seen dressed with a high-crowned hat and feather. Nor does it seem improbable, that he might introduce that fashion here in *England*. Ruffs do not appear among us till pretty late in the reign of *Henry VIII.* and were small at first; but afterwards they increased in their size gradually, till they became very large under Queen *Elizabeth*, and so continued thro' the next reign, and part of the following, when they were succeeded by broad laced bands. It is true indeed, that we find ruffs upon the images of some of our princes, or other great persons, placed on their funeral monuments, and elsewhere, which seem to exceed the fashion then in use. But as these images were made after their death, so the habits are suited not to their times, but those when the artists lived, and the monuments were erected, where they are found; which circumstance, if not attended to, will be apt to mislead us in several other things relating to our antiquities. The close striped jackets, shaped to the body, with small buttons, and striped breeches, came in about the same time with the ruffs. The ingenious artist, and diligent searcher into our *English* antiquities, Mr *George Vertue*, shewed me a portrait of *William Herbert*, the first Earl of *Pembroke*, so habited in the reign of *Edward VI.*; which, he said, was the oldest picture, that he remembered to have seen in that habit. And he had likewise another of *Edward Clinton*, Earl of *Lincoln*, and Lord Admiral in Queen *Elizabeth's* reign, dressed with a cap and feather, a striped jacket, small buttons, and a ruff.

These several considerations induce me to think, that this sculpture was made in the 16th century, and probably not before the reign of Queen *Elizabeth*, considering the largeness of the ruffs, and some other circumstances already mentioned, which seem not to suit with more early times. And from thence I would conclude, that the figure omitted in the date must have been a decimal. Some figure is evidently wanting, which, had it been a 1 in the place of thousands, might easily have been supplied, tho' the sculpture is in relief, by fixing in a small piece of stone in the form of that figure. And that this mistake was owing to the workman may be presumed from hence, that the word *you* is likewise omitted in the sentence below.

II. The other stone was found in an old castle since pulled down, which was called *Kilbritain*, in *Irish* the *Church* or *Cell*, or rather the *Burial Place of the Britons*. It is now at the house of Mr *Stowel* at *Kilbritain* near *Corke*, and contains some imperfect remains of an inscription, besides the date. (See Fig. 13.) Two copies of it have been transmitted hither, one of which being taken by laying a clean sheet of white paper over the stone, and tracing out the whole inscription with a blacklead pencil, must therefore be the more exact. But so small a fragment only of the stone has been preserved, that no certain judgment

can thence be made of the whole ; and not one perfect word remains upon it, unless perhaps the last. And as to the date, which has been read 1035, I cannot come into that sentiment for these reasons.

The same objection lies here against the shape of the figure Five, as in the other date, for the reason there given. And what has been taken for a cipher in the place of hundreds, I apprehend to be only an imperfect Four, which wants the oblique stroke, descending on each side transversely from the circular part in this manner  ; for which there appears to be room at the bottom of the circle, which does not come so low as the other figures before and after it, tho' it equals them in height at the top. There is a sample of this figure both in *Johannes de Sacro Bosco* and *Roger Bacon*, as also in the manuscript of the *Inner Temple* cited above, which shews that it remained in use till the 16th century. As this inscription is cut in relief, the extreme parts of that figure might as well be broken off, as the much greater parts of the mutilated letters in the lines above it. And I would further observe, that the probability of this opinion seems to be not a little confirmed by a parallel instance of the same figure, which formerly came before this Society, in a date found at *Colchester* ; the figures of which were at first thought to express the year 1090, that in the place of hundreds being taken for a cipher, as in the present case, by not attending to the side strokes, which were pretty near defaced ; till upon a more accurate view of the original by a very worthy Member, and skilful Antiquary, that mistake was discovered, and the date found to be 1490. For a further account of which, together with a draught of the date, I must beg leave to refer to N^o. 439. of the *Transactions* mentioned above. But as this manner of writing the Four appears as antient as the time of *Johannes de Sacro Bosco*, who died in the year 1256, the age of this *Irish* date cannot so well be determined by that figure, as by the form of the Five, which follows it, and so exactly agrees with the modern shape, that it cannot, I think, from any instance I have yet met with, be justly deemed much older than the 16th century.

Upon the best judgment therefore, which I can form of these two dates, from such arguments as the nature of the subject admits of, it appears to me highly probable, that neither of them can be more antient, than the times I have here assigned them.

3. It is impressed in releivo upon a brick, near the top of a large and high chimney, on the outside of a farmhouse belonging to *William Wollascot, Esq;* of *Woolhampton* in that neighbourhood. This date had always hitherto been read 1182, the two first figures, as they are seen from the ground, having both the appearance of a One ; with this difference only, that the second seems pretty much thicker than the first. And this led me upon viewing it in that situation to suspect it might be a 3, like that in the *Cambridge* date *. And accordingly having by the

A brief account of an ancient date in Arabian figures, at Shalford Farm (adjoining to Wasing) in the parish of Brimpton,

* Art. iv. 1.

near Alder-
marston in
Berkshire; by
the same N^o.
490. p. 613
Dec. 1748.

Fig. 14.

means of a long ladder an opportunity of going up to it, I found upon a near inspection, that it was really so, as I had apprehended. For the small curves in the second figure being filled up with moss gave it the appearance of a broad and strait line, when seen at a considerable distance. I took an exact draught of the whole, by first impressing a paper upon the brick, with the several figures contained in it; and then delineating each of them carefully with a pencil. And the copy, which is here exhibited, is drawn by a scale of $\frac{1}{3}$ of the original, *Fig. 14*. The house, where this date remains, is by tradition said to have belonged antiently to a Knight Templar; but however that might be, the date must have been placed there long afterwards; as that order of Knights was destroyed *Jan. 7. MCCCVII*.

*An explication
of a Roman
inscription
found not long
since on a stone
at Silchester
in Hampshire;
by the same.
N^o. 474. P.
200. June &c.
1744. Read
Dec. 13.
1744.*

V. 1. The draught, which accompanies this paper, contains an exact copy of a *Roman* inscription, lately communicated to me by Dr *John Collet*, Physician at *Newbury*. The account, which he gives of it, is this: the original stone, in which it is cut, was found at *Silchester*, within the antient market-place, about 4 feet under ground; and is now in the possession of Mr *John Stair* of *Aldermarston*, who took this copy of it by pressing the paper into each letter and mark in the stone, so that every part is exactly of the same size and form with the original. The two stops in the shape of leaves are not cut so deep as the letters, or the triangular points. The stone is one inch $\frac{5}{8}$ in thickness; and those parts of it, which are wanting both at the end of the lines, and at the bottom, were broken off and lost, before it was dug out of the ground.

Fig. 15.

With that larger draught I have also sent a lesser, by a scale of $\frac{1}{4}$ of it *. (*See Fig. 15*). And from the usual form of such votive inscriptions, and the manner of expressing them, I apprehend, there are not many letters lost at the end of the lines, and but one line wanting at the bottom; so that if all the words were written at length, and the line, which is wanting, supplied, the whole would run in the following manner.

Deo Herculi Segontiacorum Titus Tammonius, Saenius Tammonius Vitalis, cornicularius, honoris causa dedicarunt vel fieri curarunt.

We find no less than six altars dedicated to *Hercules* in Mr *Horsley's Britannia Romana*, two of which have the title *DEO* prefixed to the name *HERCVLI* †, as in this inscription. But as the thinness of the stone shews, that it could not be part of an altar, it might probably belong to some public building erected to his honour at this place. Mr *Stair*, as Dr *Collet* informs me, has drawn a plan of the antient town, the traces of which are sometimes visible in summer; as likewise the ruins of an amphitheatre, without the wall, not far from the eastern gate. This would have led me to imagine, that this stone might for-

* It is here reduced to near $\frac{1}{6}$

† Northumb. num. LXXXI XCIV.

merly have been removed from thence ; but that I cannot meet with any instance of an amphitheatre dedicated to *Hercules*, tho' many temples and other buildings were erected in honour of him, as may be seen in *Gruter*.

The word SEGON. in the second line, must, I think, be read SEGONTIACORUM, as referring to the name of the people *Segontiaci*. Thus we have in Mr *Horsley* DEAE NYMPHAE BRIGANTVM (a), and MOGVNTI CADENORVM (b); and in Mr *Camden* DEO MOVNO CADENORVM (c); denoting the topical deities of those people. For as to the town *Segontium*, notwithstanding the affinity of it's name with the *Segontiaci*, it was at a great distance from them, as appears by *Antonine's Itinerary* (d), being situated on the western coast over against the isle of *Anglesea*, where *Gaernarven* now stands; and therefore it could have no relation to this inscription.

The 3 following lines contain the names of the two persons, who caused this dedication to be made in honour of *Hercules*, that is, TITVS and SAENIVS TAMMONIVS, that of VITALIS in the fifth line being a *cognomen* of the latter, which often occurs as such in *Gruter*.

The two imperfect letters at the beginning of the sixth line I take for OR, which with C before them, now broken off, making the syllable COR might stand for an abbreviation of the word CORNICVLARIVS. We find CORNICVL. for CORNICVLARIVS both in Mr *Horsley* (e) and *Gruter* (f); and *Reinesius* reads K. LG. XIII. *cornicularius legionis decimæ tertiæ* (g), K being put for C, which was not uncommon. And I believe it would not be easy to find any other word, that would suit the reading in this place. *Cornicularius* was a general title for a Clerk or Secretary to a military corps; or some superior officer, military or civil. Hence we meet with *cornicularius cohortis, legionis; tribuni, præfæcti, consulis*; as also *tribuni plebis* (h). And in the *Notitia dignitatum imperii Romani*, published by *Labbe*, this is one of the officers said to belong to the court of the *comitis littoris Saxonici per Britanniam* (i), who commanded this part of the island. *Saenius Tammonius* therefore might possibly be that officer, and choose in this manner to join with the other *Tammonius* in shewing a regard to the tutelar deity of the country, where they resided.

As there can be no doubt, but the other word in the same line, when perfect, was HONORIS; whoever considers the common forms of such votive inscriptions, will readily supply the word CAVSA with DD. or F.C. for DEDICARVNT or FIERI CVRARVNT in a following line, now broken off, as necessary to complete the whole.

Had this inscription been sooner discovered, it would have saved our Antiquaries much trouble in fixing the situation and limits of the

(a) Pag. 269, 315.
1607, (d) Iter xi.

(g) Class 1. num. 192.

(i) Sect. LII.

(b) Northumb. num. LXXX.

(e) Northumb. LXIV. 6.

(h) See Pancirol. Comment. in Notit. Dignitat. p. 11.

(c) Brit. p. 663, edit.

(f) Pag. DXLV. num. 1.

Segontiaci ; about which they have been greatly at a loss, and led into different opinions. Those people are first mentioned by *Cæsar* ; who in the account of his second expedition into *Britain* says, that the *Trinobantes* having submitted to him, the *Cenimagni*, *Segontiaci*, *Ancalites*, *Bibroci*, and *Cassi*, followed their example (*k*). The *Trinobantes* are placed by Mr *Camden* in *Middlesex* and *Essex* (*l*), and the rest in the neighbouring counties on each side the *Thames* ; the *Segontiaci* particularly in the north part of *Hampshire*, in *Holeshot* hundred (*m*). And he rightly takes *Vindonum* or *Vindomis*, as it is called by *Antonine* (*n*), now *Silchester*, to have been their principal town. But tho' Dr *Gale* agrees with *Camden* in making *Silchester* the same as the antient *Vindomis* ; yet he thinks, that town did not belong to the *Segontiaci*. His words are these: *Segontiaci oram maritimam circa Cicestriam, olim Caer Cei, longe infra hanc urbem tenuerunt ; Et errant, qui credunt Vindonim eorum fuisse civitatem* (*o*). Mr *Horsley* differs from them both ; and neither admits *Silchester* to be the antient *Vindomis*, not to lie within the bounds of the *Segontiaci* ; but takes it for *Calleva Atrebatum* (*p*), mentioned likewise in the *Itinerary* (*q*). From the difficulty therefore of fixing the situation of the *Segontiaci*, Dr *Clarke* contents himself with only placing the word *incertum* against their name (*r*). But had this short inscription, as imperfect as it is, offered itself to these learned writers, none of them could have been at any further doubt, either in placing *Vindomis*, and not *Calleva* (which belonged to the *Atrebates*) where *Silchester* now stands ; or including this town within the limits of the *Segontiaci*. The want whereof has likewise occasioned them no less to differ in settling some other neighbouring stations, which by this help might have been fixed with much more agreement and certainty.

I would beg leave further to observe, with regard to the persons concerned in this dedication, that Mr *Camden* has published the following inscription found long ago at *Silchester*, MEMORIAE FL. VICTORINAE T. TAM. VICTOR CONIVX POSVIT (*s*) ; wherein the abbreviated names T. TAM. are read by Mr *Horsley* TITVS TAMPHILVS (*t*) ; the latter of which occurs indeed in the *Fasti Consulares*, but as a *Cognomen*, M. BAEBIVS TAMPHILVS. Wherefore I am more inclined to think, it should be read TAMMONIVS, as it has there the place of a family name ; and that probably he was the TITVS TAMMONIVS mentioned in this other inscription. It is true, that he has the *cognomen* VICTOR given him in the former, which does not appear in this ; but either he might not have gotten that till afterwards, or the initial letter V might have stood at the end of the third line after TAMMONIVS, which, as several writers observe, was an-

(*k*) B. G. Lib. v. c. 20, 21. (*l*) Pag. 298. edit. 1607. (*m*) Ibid. p. 194.
 (*n*) Iter XII. xv. (*o*) Comment. in Antonin. Iter. Brit. p. 135. (*p*) Brit. Rom. p. 457.
 (*q*) It. VII. XII. (*r*) Ind. Propior. nom. apud Caes.
 (*s*) Britann. p. 196. edit. 1607. (*t*) Brit. Rom. p. 332.

Fig. 12.

ANNO DOMINI MDCCLXXXII

Fig. 14.

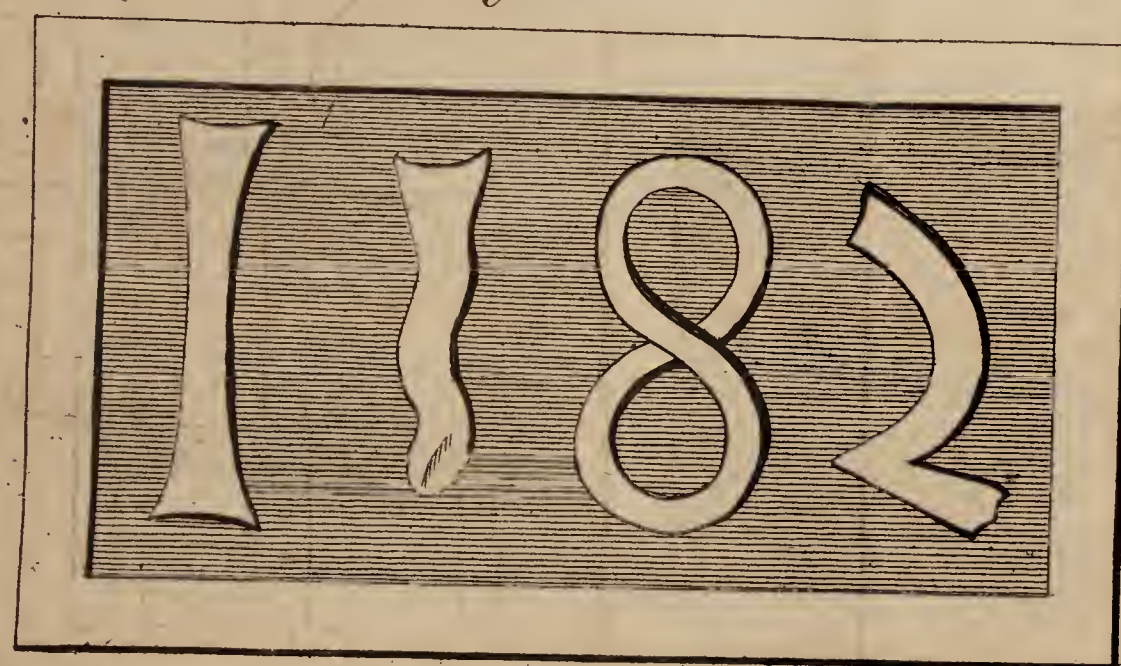
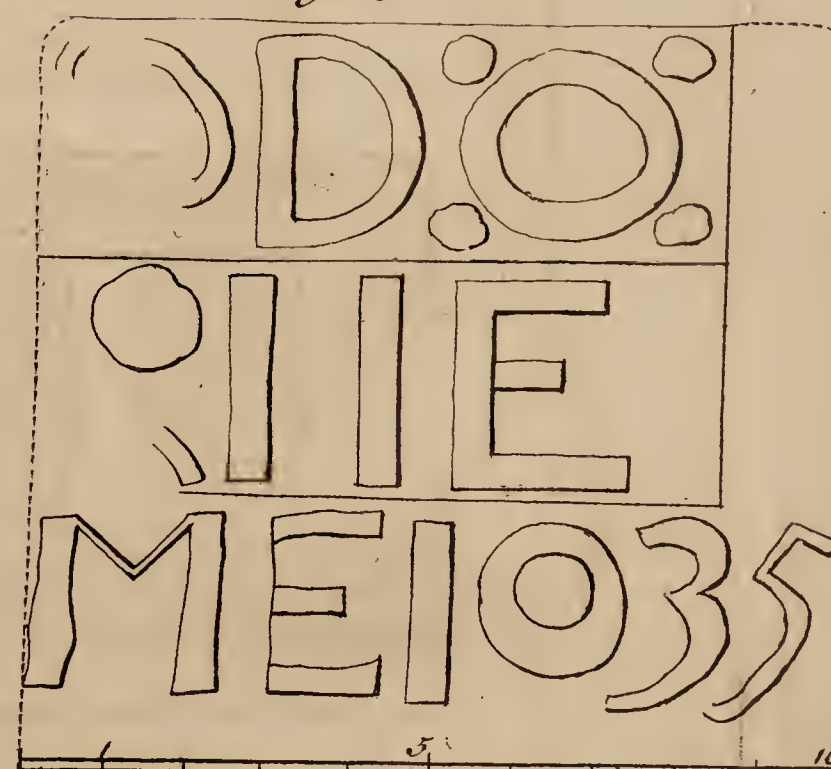


Fig. 13.



A Scale of Inches.

Fig. 15.

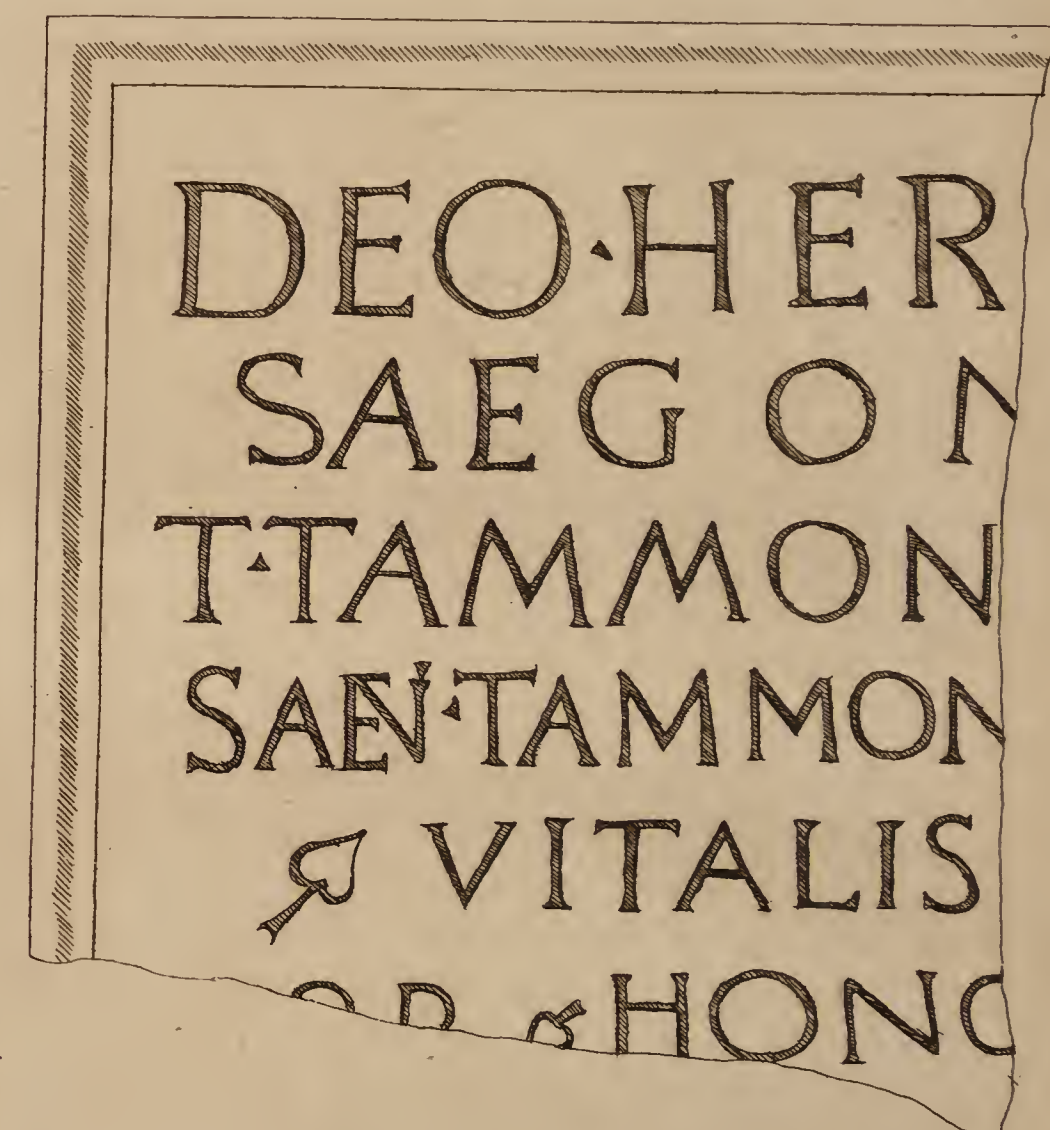
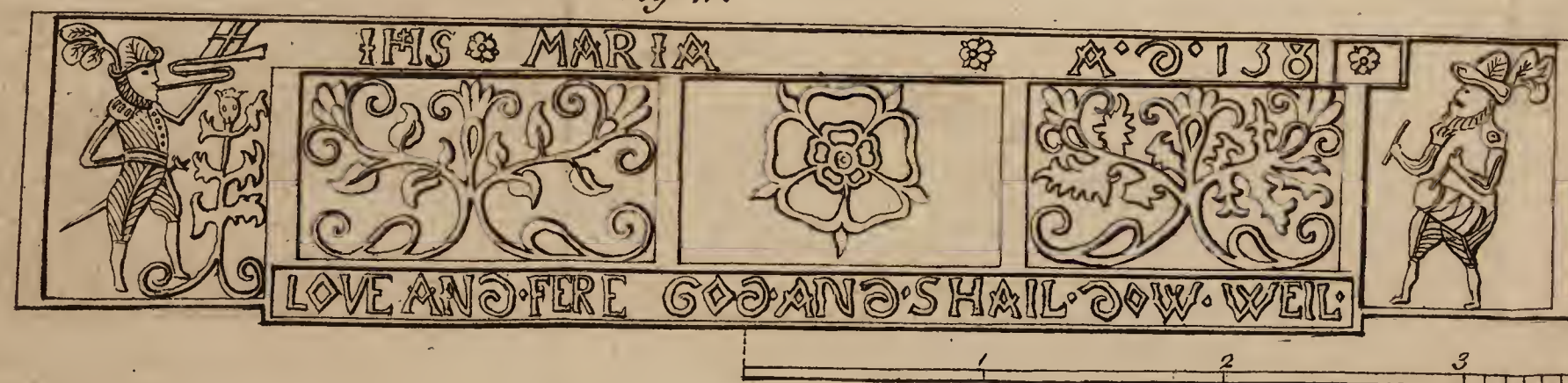


Fig. 11.



A Scale of Feet

tiently put for VICTOR. And it is very remarkable, that Gruter has given us a like votive inscription, erected by two persons, having both the same family name, with the *cognomen* VICTOR added to one, and VITALIS to the other, like those in the instance before us. Gruter's inscription runs thus: APOLLINI. SACRVM. EX. VOTO. C. VIRIVS. VICTOR. ET. L. VIRIVS. VITALIS. S. L. M (u). It is not improbable therefore, that in both cases the two persons were either brothers, or nearly related to each other.

2. In a former paper (x) I attempted to explain a Roman inscription cut in a stone, then lately found at *Silchester* in *Hampshire*; by which it not only appeared, that this town was the antient *Vindomis*, but likewise that it was situated within the limits of the *Segontiaci*: as to both which circumstances our best Antiquaries have been at an uncertainty, and differed in their sentiments concerning them. I took notice likewise at the same time, that the traces of the antient town are yet, as I had been informed, often visible in the summer; and that the ruins of an amphitheatre still remain without the wall. But being since in that country, I had an opportunity of visiting the place myself, and getting a more perfect account of it than I expected, by the assistance of two persons in the neighbourhood, the late Mr *John Wright*, jun. and Mr *John Stair*, jun. who were both well acquainted with it, and accompanied me thither. The former, who was an experienced surveyor, measured the whole circuit of the wall, with the height of it in several places, as also the dimensions of the amphitheatre, while we were on the spot. And the other traced out the several streets, and other parts of the town, to a considerable exactness. But as only a rough draught of the plan could then be taken, I left it with them to revise, and transmit to me an accurate copy; which has been since done, and now accompanies this description.

A Description of the town of Silchester in its present state; by the same.
N^o. 490. p. 603. Dec. 1748. Read Dec. 22. 1748.

The circuit of the wall on the outside, as therein given by the scale, Fig. 16, contains near one *English* mile and $\frac{1}{2}$; and the several parcels of land contained within it amount together to 100 acres, or upwards. Indeed *Leland* says, that the compass of the wall is *about two miles, and conteyneth 80 acres* (y). And *Camden* says the same, except that he calls them *Italian miles* (z). But neither of them acquaint us, from whom they had their measurement. The wall consists of 9 sides, but very unequal; which might perhaps be occasioned by the different situation of the ground, which in some parts is uneven.

The materials, that compose the wall, are large flints, and rough stones of different sorts, cemented together with very strong mortar. And as to the manner of building it, the foundation is generally made of a row or two of stones laid flatwise; and over them 4 or 5 rows of flints; then usually a double row of stones, sometimes three rows, and at

(u) Pag. xxxviii. num. 17. vol. vi. p. 48, edit. 1744.

(x) See the preceding Article.

(y) *Itinerary*,

(z) *Britann.* p. 196. edit. 1607.

other times one only, laid in the same position; over these a like number of rows of flints, as before; and so alternately upwards. And a little to the westward of the south gate are yet to be seen 7 of these ranges of stone, with six of flint between them; where the height of the wall measured on the outside about 18 feet. And about 50 yards eastward of the same gate are 6 ranges of stone, with 5 of flint between them; where a small part of the facing seems yet to be near entire. But there is no appearance either of copings, or battlements, on any part of the wall. Tho' the ranges of stone in the front of the wall are placed horizontally, yet those within it often stand edgewise and somewhat obliquely, like the wall of *Severus* in the north of *England* *. And at the south gate the thickness of the wall measured about 5 yards. From this account therefore it seems not improbable, that in the passage of *Leland*, given us by Mr *Hearne* from *Stowe's transcript*, where it is said, *the wall without is in some place 6 or 7 fote highe* †, for the numbers 6 or 7 should be read 16 or 17.

The wall is not any where entirely demolished, except that two breaches have been made of late years on the N. W. side, to open a passage for waggon. And the ditch without the wall is in some places 10 or 12 yards over, but in others at present not visible; where probably it may have been filled up by the earth thrown into it from the *vallum*, that encompassed the city between that and the wall, and which is yet in several places of a considerable height about the ditch. Each of these may be seen in the plan, || marked with the letters A, B, C. There is little appearance of the *vallum*, or military way, within the circuit of the wall, the ground being now more generally raised pretty near the top of the wall, on which grow many large oaks, and other timber trees. From the south gate towards Winchester has lain a military road, which when broken up appears to have been pitched with flints.

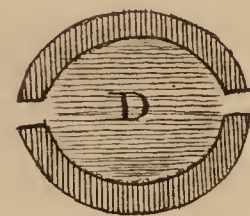
The amphitheatre stands without the wall, at the north-east corner, and distant from it upwards of 100 yards; as appears in the plan, where it is marked by the letter D. Both the wall and seats, which are made in it, consist of a mixture of clay and gravel. The wall is about 20 yards thick at the bottom below the seats, and decreases gradually to the thickness of about 4 yards at the top. There are 5 ranges of seats above one another, at the distance of about 6 feet on the slope. It has two passages into it, one towards the town, and the other opposite to it. The diameter of the area is 50 yards by 40, and the area itself now serves for a pond to a Farmer's yard. The design of this amphitheatre might possibly be for the baiting of wild beasts, or other athletic diversions, agreeable to the customs of those times. Tho' at present no appearance of a cavern, or any other place proper for the reception of such animals, is to be discovered.

* See Horsley's *Britann. Rom.* p. 123.

† *Ubi supra.*

|| See TAB. III.

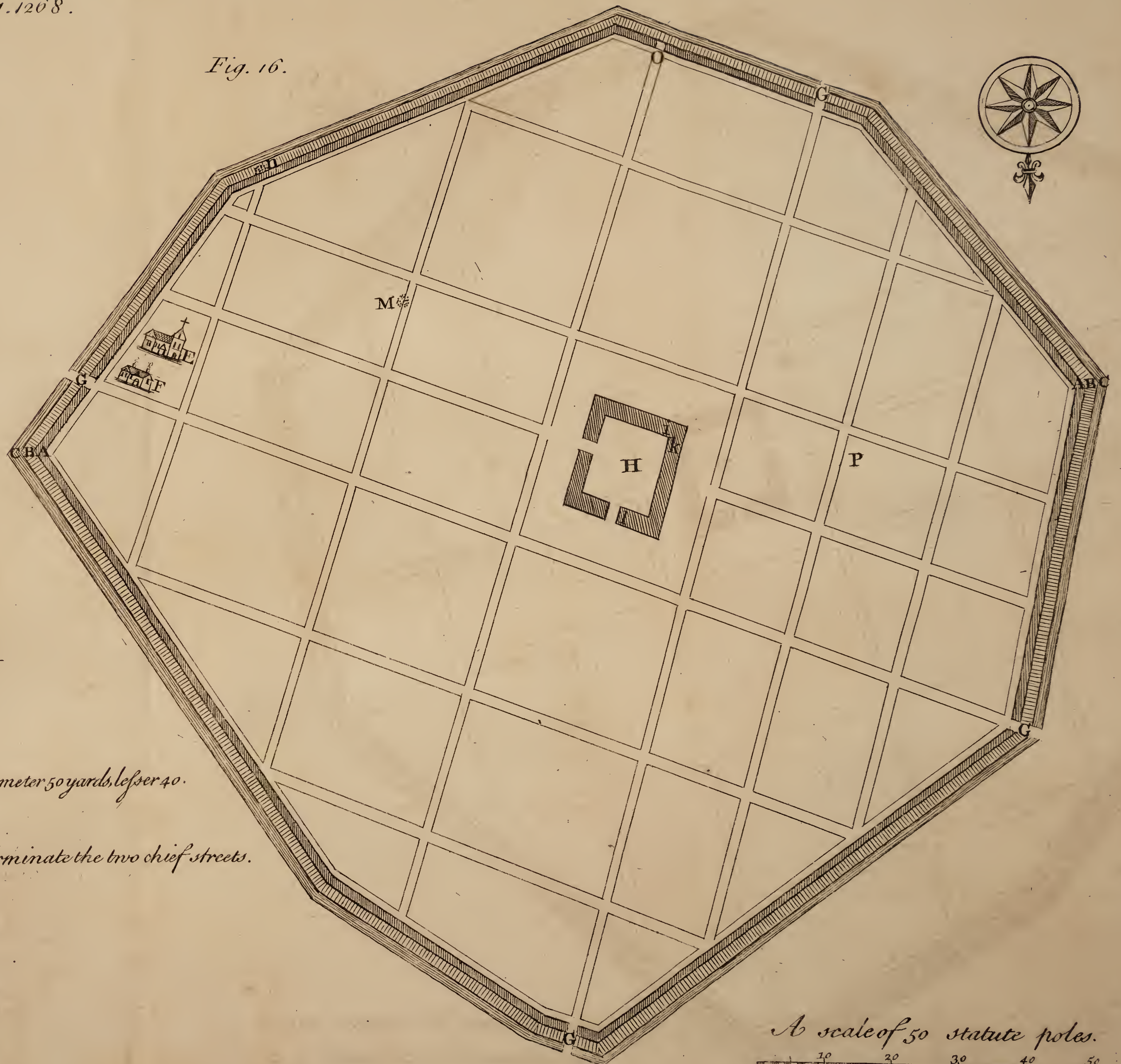
A PLAN of
the ancient city of
SILCHESTER
in Hampshire,
taken by
JOHN WRIGHT
Surveyor,
MDCCLV.



The Explanation

- A. The Wall.
- B. The Vallum.
- C. The Ditch.
- D. The Amphitheatre, greater diameter 50 yards, lesser 40.
- E. The Church.
- F. The Farmhouse.
- G. The four City gates, which terminate the two chief streets.
- H. The Forum.
- ik. A Temple.
- 1. The Roman inscription.
- Mn. A Fountain.
- O. Onion hole.
- P. Silver hill.

Fig. 16.



A scale of 50 statute poles.



The area of the town within the walls contains at present only corn-fields, except a small quantity of meadow land, with an antient church, and farm house, near the east gate, both which are marked in the plan with the letters E and F. The method taken by Mr *Stair*, in order to discover where the streets formerly lay, was by observing for several years before harvest those places, in which the corn was stunted, and did not flourish as in other parts. These were very easily distinguished in a dry summer, and run in strait lines crossing one another, as they are drawn in the plan. Moreover, by spitting the ground, and often digging it up, he found a great deal of rubbish, with the plain ruins and foundations of houses on each side of these tracts. Whereas in the middle of the squares nothing of that nature appeared, and the corn usually flourished very well. The ploughmen also confirmed the same, who found the earth harder,, and more difficult to be turned up, in these tracts and near them, than elsewhere. And it is further observable, that two of these streets, which seemed rather wider than the rest, lead to the 4 gates of the city, one of them running in a direct line from the north to the south gate, and the other from the east to the west, which latter measured at least 8 yards across. The 4 gates are marked with the letter G in the plan.

By digging likewise in different places Mr *Stair* at length discovered the ruins of a number of buildings in the form of a long square, which in the plan is marked with the letter H. The foundations of some of these buildings were still pretty entire, and the depth of them from wall to wall was found to be about 27 feet, and the breadth about 16, which it is not improbable may be the remains of the antient *forum*. But between the letters *i* and *k* there appeared the foundation of some larger structure, consisting of free stone three feet in thickness. And at *k* there seemed to be the pedestal or foundation of an altar, by the great quantity of ashes and wood coals burnt, that lay round about it. What remained was about 3 feet in height, 4 in length, and 3 in breadth. It consisted of large *Roman* bricks, one of which dug up entire, and communicated to me by Dr *Collet*, is $17 \frac{1}{2}$ inches long, $12 \frac{1}{2}$ broad, and $2 \frac{1}{2}$ thick; which accompanies this paper.

At the letter *l* was found the stone with the inscription upon it mentioned above. And upon further search Mr *Stair* has since dug up within 2 feet of the same place, and about 4 feet under ground, a square copper frame, composed of several mouldings, and it's sides soldered together, 3 of which are yet entire, but part of the 4th is broken. This frame inclosed a border of the same metal, one side of which is still preserved. The weight of them together is 47 pounds; but the thickness of the frame varies in different parts from of an inch to much less, and the border is more than $\frac{1}{8}$ of an inch thick. Each side of the frame at the outer edge is about 33 inches long. And from the size of the inner edge of the border, each side whereof is 20 inches in length, it is supposed that the stone, which contains the inscription, was at first placed

Fig. 17.

placed behind it, and supported by it. And as that inscription was erected in honour of *Hercules*, it might originally belong to the stone building, whose foundation is yet visible, and which might have been a temple consecrated to that deity by the persons named in the inscription; as was conjectured in the former account given of it. A draught of this frame and border in their present state, taken by a scale of $\frac{1}{6}$ of the original, now in the possession of Dr *Mead*, is prefixed to this Discourse, *Fig. 17*. There was likewise a considerable number of brass *Roman* coins found near the same place.

At the letter *m* in the plan was antiently a fountain, which at *n* discharged itself under the wall. Some of the stones, with which this fountain was inclosed, are still to be seen.

And at the letter *o* is a large breach under the wall, 2 yards in length, and 2 feet in height; though in the memory of some persons yet living it was considerably higher. It is called *onion hole*, from an imaginary giant of that name, who is said once to have inhabited this city; and from whom likewise the *Roman* coins found there have been called *onion penies*. The breadth of the wall thro' this hole, where it is pretty much broken, measured now but 10 feet; and being upon high ground could not, I think, have been a common sewer, as some have imagined; but might originally perhaps have been designed for a private passage upon some necessary occasions.

The most valuable coin, which has been discovered in the ruins of this antient *Roman* town, is a gold one of *Allectus* in fine preservation, and very remarkable for a peculiar attribute of the deity on the reverse. The front side represents the head of *Allectus* crowned with laurel, round which is this legend, IMP C ALLECTVS P F AVG. On the reverse is placed the figure of *Apollo* with a radiated crown; his left hand, which holds a globe, has over it a whip; his right arm is raised in a forbidding posture, and supports a *chlamys*, which crossing his breast descends on both his sides; at his feet sit two captives, whose hands are tied behind them; and the legend round it is ORIENS AVG, with ML in the exergue *. These several attributes may be found in some or other of the imperial coins between the time of *Gordian* the younger and the *Constantines*, or later. But the singularity of the coin, I am now describing, is this; that in the figure of *Apollo* the eyes seem plainly to be covered by a fillet, which goes cross the forehead; the reason of which

* Some very skillful Antiquaries have thought, that those letters on the reverse of many coins of the lower Emperours, which are put at the bottom, often denote the place, where those coins were struck. And therefore, as I meet with ML by themselves upon no others, but those of *Carausius* and *Allectus*, who both ruled in *Britain*; it seems not improbable, that they may stand for *moneta Londinensis*, or *Londini*, supplying *signata* or *incusa*. As upon some coins of *Constantinus Magnus*, who was first proclaimed Emperour in *Britain*, we find MSL and MLI, which may also be so interpreted. Indeed the letters MLS and MLP occur upon the coins of some other Emperours, where they have been read, and perhaps justly enough, *moneta Lugduni signata* and *percussa*.

I cannot undertake to account for with certainty. But we are told by *Suetonius*, that among other reflections thrown upon *Augustus* for a secret entertainment made by him, at which the persons present were dressed in the habit of deities, and this at a time of great scarcity in *Rome*, *Acclamatum est postridie frumentum omne deos commedis; et Caesarem esse plane Apollinem, sed tortorem*. To which *Historian* adds, *Quo cognomine is deus quadam in parte urbis colebatur* *; which being the place where criminals were punished, is thus described by *Martial*,

Cruenta pendent qua flagella tortorum †.

It was not unusual for the Antients to apply the attributes of one deity to another on particular occasions. From whence one might be led to interpret this representation of *Apollo*, or the Sun, with a whip, and a bandage over his eyes (the emblems of *Justice*) together with the two captives, as descriptive of the punishment denounced against all, who should attempt to oppose the government of *Allectus*. And as the *Roman* coins do generally contain on their reverse some devise relating to the times, in which they were made; it seems not improbable from hence, as also from the legend, *ORIENS AVGVSTI*, that this was struck upon *Allectus*'s first assuming the purple, after he had murdered *Caracausius*: since upon others of his coins we meet with *SPES AVGVSTI*, *VIRTVS AVGVSTI*, *PAX AVGVSTI*, *SALVS AVGVSTI*, and the like, as denoting the gradual success of his affairs afterwards. An impression of this curious coin, which is now in the *Museum* of Dr *Mead*, is likewise given here. The place, where it was found, is marked in the plan by the letter P; which of late years has gained the name of the *Silver-bill*, because more silver coins have been found there, than in any other part of the city. And by the remaining ruins, which discover themselves upon turning up the ground, it is supposed, that some large building stood antiently upon that spot. But great numbers of coins in all metals, and of all sizes, have likewise been found in several other places; so that Mr *Stair* is now possessed of several hundred, which have been all collected from this *Roman* settlement; among which are the Emperours *Valentinian* and *Arcadius* in gold; with most of the imperial coins from *Augustus* to that time, either in silver or brass; many of which are exceedingly well preserved.

VI. A transcript of the first inscription, sent to me six months ago, *An antient Roman inscription at Rochester in Northumberland, and two* was copied by a person of too narrow judgment to be depended upon; which obliged me to take a journey to *Rochester*, to be exact in every circumstance relating to the same: the stone was found erected upon two supporters, and discovered last year in the midst of a heap of rub-

* In *vit. August.* cap. 70.

† *Lib. II. epigr. 17.*

others at Rishingham; by Mr Christopher Hunter. N^o. 474. p. 159. June &c. 1744. Read Nov. 8. 1744. Fig. 19. Fig. 20, 21.

bish, and the inscription intire to the last line, where the name of the *Præfetus* is visibly obliterated, in some ensuing reign.

The two following I took out of the earth at *Risingham*, another Roman fort, 8 miles S. from *Rocheſter* upon the banks of *Reed's* water mentioned by Mr *Camden*: they are both altars broken, the first falling from the top of the highest wall of a dwelling-house deserted above 50 years; the 2 initial letters of the third and fourth lines broken off; as IVLIUS, LIVIVS, LIBIVS, LICIVS, or LIDIVS. The other is upon the margin of Mr *Warburton's* map of *Northumberland*, I fear, omitted by Mr *Horsley* in *Britannia Romana* among his sculptures; but find it in pag. 240. of his *Observations*, so quite different from my copy.

The inscription upon a Roman altar found near Stanhope in the bishoprick of Durham; communicated to the R. S. by the Rev. Mr Tho. Birch, F. R. S. N^o. 486. p. 173. Feb. & Mar. 1748. Read Feb. 18. 1747-8.

VII. SILVANO INVICTO SACRVM
CTETIVSVETVRIVSMICIA
NVSPREFAIÆSEBOSIAÆ:
NÆOBAPRAMEXIMIAE
FORMÆCAPTVMQVEM
MULTIANTECESSO
RESEIVSPRAEDARI
NONPOTVERVNTVSLP

Silvano invicto sacrum
C. Tetius Veturius Micia-
nus, Præf. Alæ Sebosia-
næ, ob Aprum eximiae
formæ captum quem
multi Antecesso-
res ejus prædari
non potuerunt Votum solvens lubens posuit.

A Roman inscription found at Bath, communicated to the R. S. by the Rev. Will. Stukely, M.D. Fellow of the Coll. of Phys. F. R. S. and Rector of St. George the Martyr, Lond. N^o. 488. p. 409. June &c. 1748. Read June 30. 1748.

VIII. LVVITELLIVS MAXIMI
NIAI FVT ANCINVS
CIVES HISP CAVRIESIS
EQ ALAE VETTONVM CR
ANN XXXXVI STIP XXVI
H S L

Thus to be read. *Lucius Vitellius Maximiniani filius Titus Ancinus, civis Hispanus Cauriensis* † *equitum alæ Vettonum curator, annos XLVI, stipendii XXVI. hic sepultus est.*

* Like *nubes*, *labes*, *sepes*.

† Of the city of *Ceria* in *Spain*.

IX. This

Fig. 17.

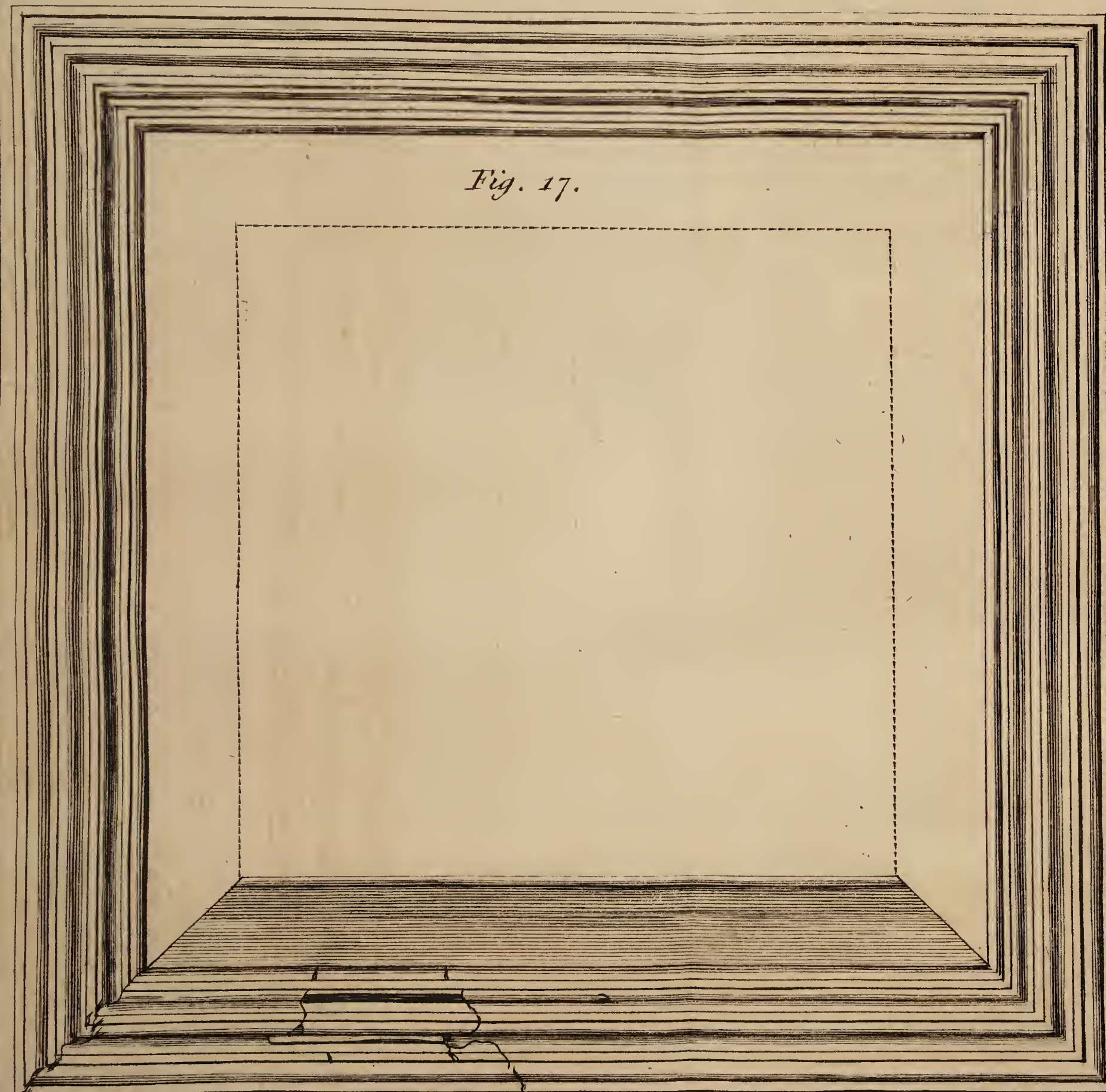


Fig. 19.

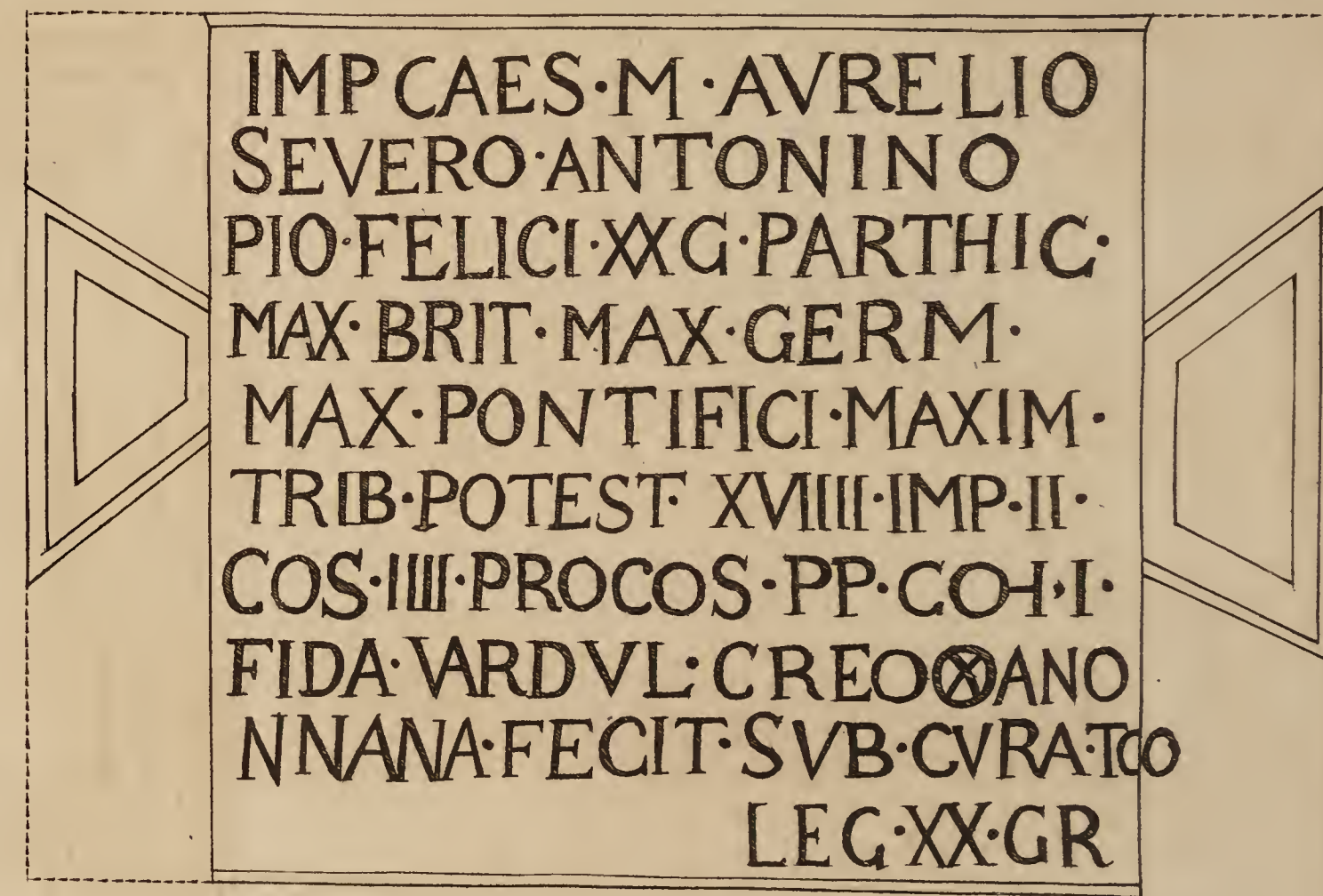


Fig. 18.



Fig. 20.

MARTI
VICTORI
VLIVS LI
IVS TRIB
V·S·L·M

Fig. 21.

FORTVNAE
AVG
A E T
PROCVLINA
V S

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IX. This inscription is cut in a small brass plate, which together with a draught of the same dimensions accompany this paper. See *Fig. 22.* The words of the inscription, as they stand on the plate, with some account how and where it was found, were formerly published by *Fabretti (a)*. But as what he says is very short, I shall take leave to transcribe the whole of it, which is as follows :

FLORAE
TI PLAVTIVS DROSVS
MAG. II
V. S. L. M.

Remarks upon an antient Roman Inscription, found in that part of Italy, which formerly belonged to the Sabines ; and now in the possession of Richard Rawlinson, LL.D. F.R.S. by Mr John Ward, Prof. Rhetor. Gresh. & F. R. S. N^o. 494. p. 293. Jan. &c. 1750. Read Jan. 11. 1749-50. Fig. 22.

Minucius Felix (b), *Laetantiusque (c)*, Floram inter propudiosa gentilitatis numina collocant. Vindicat autem hanc infamiam Ouzelius ad Minucium, pag. 233, ubi ex Varrone et Ennio probat Romulo antiquiorem fuisse Florae cultum, et flamines ei datos. Vestigium igitur hic remanet in Sabinis primum Floram coli coeptam ; cum e Sabino agro in lamella aenea inscriptio haec nuper eruta fuerit apud Licentiam, Ursinorum oppidum, quod Digentia rivo alluitur. Licentia quippe a Digentia detortum nomen facile quis credet.

The plate now before us, and the inscription upon it, so exactly agree with this account of *Fabretti*, as to leave no doubt of their being the same with those described by him. The present possessor of the plate purchased it at Rome, in January 1720. N. S. At which time a small brass label was fixed to it, containing the following words cut in capital letters, EX REGIIS CHRISTINÆ THESAURIS ; which is also here exhibited, with a draught of the same form and size subjoined to that of the inscription. It is probable therefore, that this plate might have been deposited among the other curiosities of that Princess, soon after it was dug up ; and afterwards have fallen into other hands, who affixed that label to it ; till at length being exposed to sale, it was bought by the Doctor at the time abovementioned. For Queen *Christina* died at Rome, April the 19, 1689, where she had resided from the year 1658, after her return thither from France (d). The year following a catalogue of her Greek and Latin medals was printed at Rome (e), and *Fabretti* published his large and elaborate collection of *antient inscriptions* there in 1699. In which he says, that this plate, at the time he wrote his account of it, *nuper eruta fuerit*. But as he wrote this very probably some years before the publication of his book, the plate might have been found before the death of that

(a) *Inscript. Antiq. cap. x. pag. 742.*

(b) *Pag. 28. edit. Ouzel. quarto.*

(c) *Instit. lib. 1. cap. 20.*

(d) *Moreri, Diction. Histor.*

(e) *Nummi antiqui in*

thesauro Christinae reginae Suecorum, Romae asservati, a Francisco Camelo, ejusdem Ma-iestatis antiquario. Romae 1690.

Princess, and been in her possession; and after her decease come into such hands, as might occasion the disposal of it, at the time, and in the manner, here recited.

But I shall now proceed to consider the words of the inscription; which may, I presume, be read at length, with the proper supplements, in the following manner:

Florae Tiberius Plautius Drosus, pagi magister anni secundi, votum solvit libens merito.

The goddess FLORA was thought by the Romans to preside over fields and trees, and therefore they addressed to her to favour them with prosperous and fruitful seasons. Hence Varro says: *Invoco Rubigum et Floram, quibus propitiis, neque rubigo frumenta atque arbores corrumpit, neque non tempestive florent. Itaque publicae Robigo feriae Robigalia, Florae ludi Floralia sunt instituti* (f). But it appears from other passages of that writer referred to by Fabretti, that she was first a Sabine deity, and introduced at Rome by King Tatius in the time of Romulus, many ages before the institution of the Floralia. For that festival was not observed till the year of the city 513, when the expense of it was ordered to be paid out of the fines levied on those persons, who had converted the public lands to their own use, for feeding their cattle (g). The memory of which is yet preserved on a coin struck that year, having on one side the Genius of Rome: and on the other a sheep, as an emblem of the manner, by which that money was raised; with the legend M PUBLICIVS MALEOLVS, the name of one of the aediles, who had the care of the coinage (h). And the same year also he with his brother Lucius Publicius, the other aedile, erected a temple in honour of the goddess Flora, near to the great circus (i). However the Floralia were not afterwards constantly observed, but only when the season was bad, and seemed to threaten a scarcity of provisions. But in the year 580 (k), by a decree of the senate, they were ordered to be celebrated annually, upon the fourth of the kalends of May, and the three following days (l), which ended with the first of that month.

TI, the two first letters of the praenomen of the person mentioned in the inscription, are the usual abbreviation of Tiberius; as a single T is of Titus (m).

PLAUVS, which follows, denotes the family name, and often occurs in Roman writers, as also upon coins, where it is sometimes written Plotius, and at other times Plutius (n).

(f) De R. R. Lib. i. cap. i. (g) Ovid. Fast. Lib. v. v. 279. (h) See Vaillant Fam. Rom. in Publicia, num. 1. (i) Tacit. Annal. Lib. ii. cap. 49. (k) See Pighii Annales ad ann. 513, 580. (l) Ovid. Fast. Lib. v. v. 327, 183. (m) Valer. Prob. de not. Roman. (n) See Vaillant Fam. Roman. in Plautia.

DROSVS, the *cognomen*, I do not remember to have seen so spelt elsewhere; but I make no doubt of it's being the same as *Drusus*, which we frequently meet with. For thus, as was just now observed, his family name is written three several ways, *Plautius*, *Plotius*, and *Plutius*. And *Suetonius*, speaking of the *Claudian* family, says: *Notissimum est Claudios omnes, excepto P. Clodio, optimates assertoresque dignitatis ac potentiae patriciorum semper fuisse* (o). So likewise *Poblicius*, the name of the aedile abovementioned, is sometimes spelt *Publicius*. These instances indeed all relate to the family name; but the same is true likewise with regard to the *cognomen*. So the name *Poblicola* first given to *Publius Valerius*, the colleague of *Brutus*, but retained afterwards by the family, is generally written *Publicola* (p). And as to *Drusus*, *Suetonius* acquaints us with the original of that name. *Drusus*, says he, *hostium duce Drauso cominus trucidato, sibi posterisque suis cognomen invenit* (q). His other names were *Marcus Livius*, and the action here referred to was in the year of the city 470 (r). It therefore seems very probable, that this *cognomen* being so antient, might by some of the descendents of that family be written *Drosus*, and afterwards more generally *Drusus*. For as the Romans endeavoured gradually to improve their language, and soften it's pronunciation, they changed *au* into *o*, and again *o* into *u*, for the more agreeable sound. This appears not only from the examples given already, but likewise from many others, which might be produced, as well of common words as proper names. Of the former sort are *codex* from *caudex*, *corus* from *caurus*, *lotus* from *lautus*, and *forex* from *saurex*; and of the latter, *multus* from *moltus*, *rubigo* from *robigo*, *vulgus* from *volgus*, *Vulcanus* from *Volcanus*, and the like.

MAG. II. according to the explication given above, are an abbreviation of the words *magister secundi*, which stand for *pagi magister anni secundi*, was the whole to be expressed at length. The word *pagus* signifies a division or large portion of land, not much unlike what we call a *shire* or *county*. Hence *Caesar* in his history of the *Gallic war* says: *Omnis civitas Helvetia in quatuor pagos divisa est* (s). And again speaking of the *Suevi*, who were a very large nation: *Hi centum pagos habere dicuntur* (t); tho' *pagus* is sometimes used by the poets in a more restrained sense, for a single *village*. So *Mandela* is described by *Horace*, as *rugosus frigore pagus* (u). And *Virgil* represents the rural sports, as performed *pagos et compita circum* (x). As to the title *magister*, it always denotes some preheminance and authority, and is used in a variety of senses, as *master* is with us. So *Festus* says: *Magistri non solum doctores artium; sed etiam pagorum, societatum, vicorum, collegiorum,*

(o) In *Tiber. cap. 2.* (p) *Liv. ii. 8. vii. 32.* (q) In *Tiber. cap. 3.*
 (r) See *Pighii Annales, ad an. 470, 471.* (s) *Lib. i. cap. 12.* (t) *Lib. iv. cap. i.*
 (u) *Epist. Lib. i. xviii. 103.* (x) *Georg. Lib. ii. 382.*

equitum dicuntur ; quia omnes hi magis ceteris possunt (y). Of these the *magister pagi* was a very antient officer among the Romans, appointed by *Numa Pompilius*, the immediate successor of *Romulus*, as we learn from *Dionysius of Halicarnassus*. “ For *Numa* (as he saies) first divided
 “ the whole country into such portions, as they call *πάγες* ; over each
 “ of which he appointed an officer, whom he calls *ἀρχοντα, ἐπίσκοπον,*
 “ and *περίπολον*, that is, a President or *Master*, Inspector, and Surveyor.
 “ His business was to survey the lands, and take an account in writing,
 “ whether they were well or ill cultivated ; which accounts he laid be-
 “ fore the King : who commended those husbandmen, who had been
 “ diligent, and treated them courteously ; but reproved and fined the
 “ indolent, in order to excite them to greater diligence (z).” The
 Roman territories at that time were confined within very narrow
 bounds ; but in after-ages, as they enlarged their dominions by con-
 quests, many of the countries, which they subdued, were given to the
 soldiers, as a reward for their service. And as a certain quantity of land
 was allotted to each soldier, the distribution was made by persons sent
 with them for that purpose ; and those settlements, or colonies, were
 divided into several districts, or *pagi*, which were considered as distinct
 communities. And because differences would sometimes happen among
 them afterwards about the limits of their possessions, the *magistri pago-*
rum were impowered to adjust them, and prevent all incroachments of
 that sort. For which end also public processions were made at certain
 times, accompanied with sacrifices and other religious ceremonies (a),
 in honour of the tutelar deity of the place, the expense of which was
 born by contributions of the inhabitants ; as is shewn in another paper
 (b). These officers had likewise the care of the roads, which led from
 one town or village to another, within their respective jurisdictions, to
 keep them in good repair (c). And another part of their duty was to
 provide accommodations for the armies in their march, as also for the
 governors of provinces, and other great persons, in passing thro’ their
 several districts upon public affairs (d). Horace seems to refer to this,
 when he saies :

*Huc venturus erat Maecenas, optimus atque
 Cocceius, missi magnis de rebus uterque
 Legati ; aversos soliti componere amicos.*

And soon after :

*Proxima Campano ponti quæ villula tectum
 Praebuit ; et parochi, quæ debent, ligna salemque* (e).

Where by *parochi* (so named from *παρέχειν* to provide) may be meant
 an inferior sort of officers, appointed by the *master* to provide all ne-
 cessaries for strangers, included here in the words *ligna salemque* (f).

(y) In voc. *Magisterare*. (z) *Antiq. Rom. Lib. ii. cap. 76.* (a) *Siculus*
Flaccus, pag. 9, 25, edit. Goef. (b) See Art. xxi. (c) *Sicul. Flacc. ubi*
supra. Frag. Magon. et Veg. pag. 255. (d) *Sic. Flac. pag. 25.* (e) *Serm.*
Lib. i. Eclog. v. v. 27, 45. (f) See *Budæi Annot. in Pandect. p. 262.*

But as the title *magister* was common to so many officers of different characters, it may be expected, that some reason should be alleged for assigning it to the *magister pagi* in this inscription, where no other word is added to determine the particular sense of it. And indeed I cannot pretend to offer any direct proof for this; but the reasons, which induce me to think so, are these which follow. Both the design and form of the inscription seem to suit him best. For the purport of it being the performance of a vow to the goddess *Flora*, very probably for a prosperous and fruitful season, as the effect of their addresses to her; by whom could that so properly be done, as by this officer, who presided over the religious as well as civil affairs of the inhabitants? Besides, I cannot but think from the name *Drosus*, which elsewhere is always spelt *Drusus*, that this inscription was written before several other officers were appointed, who afterwards bore the title of *magister*; which might render any explanatory word unnecessary at that time, especially in the place where he had his residence. There are indeed two inscriptions published, one by *Gruter* (g), and the other by *Reinesius* (h), in which the word PAGI is added to MAGISTER; in the same manner as we meet with *magister collegii*, *vici*, and the like. But the former of these was as late as the reign of *Tiberius*; and the time of the latter uncertain, so that nothing can be concluded from it. And there are likewise several inscriptions, in which MAGISTER and MAGISTRI stand alone, without the addition of any explanatory word. But then it appears either from the design of those inscriptions, the number of persons mentioned, or some other circumstance relating to them, that their office was of a different nature; some few instances of which I shall presently have occasion to mention. From these considerations therefore I have been led to conclude, that the sense here assigned to the word *magister*, as it respects this inscription, is the most probable.

That the characters II. stand for *anni secundi*, the date of the time, during which this *Drosus* had then held that office, is confirmed by several inscriptions published by *Gruter*. In one of which we have MAG. ANNI. V (i); in another MAGISTRI. ANNI. VI. (k); and in two others MAG. ANNI. PRIMI. (l), where the word denoting the time is expressed at length. As these different ways therefore of expressing the time relate to persons, who all bore that title, tho' not the same office, as appears by the inscriptions; they plainly shew in what sense those characters are to be taken here.

The concluding letters V. S. L. M. which stand for *votum solvit libens merito*, contain the usual form of dedicating votive monuments.

But the thing dedicated is not mentioned here, which was most probably a statue or an altar; and I rather think the latter, from the number of such inscriptions in *Gruter*, and other collectors of antient monu-

(g) Pag. xxvi. num. 9.

(h) Class. sext. num. LXXII.

(i) Pag. LIV. num. 1.

(k) Pag. xxxvi. num. 7.

(l) Pag. xxxiii. num. 10. xxxv. 5.

ments, taken from altars. Whereas we hear of few images of any sort relating to this deity. *Pliny* indeed mentions a statue of *Flora* at *Rome*, which was made by *Praxiteles* (m). And *Montfaucon* has given the draughts of three or four others, which have been thought to represent that goddess (n). Her head also is found upon two coins, one of the *Claudian* (o), and the other of the *Servilian* family (p). However these are but very few, comparatively with what we meet with of other deities. Besides, the age of this inscription (which seems further confirmed by it's brevity and simplicity) as likewise the place where it was discovered, suit much better with an altar; for altars were erected in all parts of the country, and very early. I suppose therefore, that this plate was fixed into the front of some altar consecrated to *Flora*; for which purpose both the shape of it, and the angles at each end, seem to be very well adapted. The town, where it was dug up, is by *Fabretti* called *Licentia*; which he supposes to have taken it's name from the river *Digentia*, that runs by it. *Horace's* farm therefore was probably not very far from thence, since he speaks of it as near that river, when he says:

Me quotiens reficit gelidus Digentia rivus ;
Quem Mandela bibit, rugosus frigore pagus :
Quid sentire putas, quid credis, amice, precari (q) ?

The antiquity of this inscription, together with it's singularity, made me think it not unworthy the notice of this learned Society. For tho' we meet with two inscriptions, one in *Gruter*, (r) and the other in *Reinesius* (s), to the honour of *Flora* jointly with other deities; yet this is the only one I can find, which is addressed to her alone. And as to the manner of explaining the several parts of it, that is left to every one to judge of, as he pleases.

An attempt to explain an antient Greek inscription, ingraven upon a curious bronze cup with two handles, and published with a draught of the cup by Dr Pococke, in his Description of the East, Vol. II. Part 2. pag. 207. By the same. N^o. 495. p.

X. The diameter of the cup on the inside is about $13\frac{1}{2}$ inches, as near as I could take it from a copy of the inscription, which is placed round the upper side of the rim. A draught of which is hereunto annexed, reduced to about $\frac{2}{3}$ of the size, and may also be seen in *Dr Pococke*, with only this difference, that the characters on the cup are not cut in continued lines, as in the doctor's plate, but consist of separate points placed in the form of letters, as here represented. Letters expressed in this manner appear upon several antient coins; and may likewise be seen in the silver plates, found not long since in *Hertfordshire*, and published in these *Transactions* (t). As to the circular form of the inscription, we read in *Pausanias* of an instance not very much unlike this. *Iphitus* King of *Elis* is said to have restored the Olympic games,

(m) *Nat. Hist. Lib. xxxvi. cap. v. § 5.*

chap. 30.

(o) See *Vaillant Fam. Rom. num. 13.*

(q) *Epist. Lib. i. xviii. 103.*

num. CCXXXIII.

(t) See Art. xiv.

(n) *L. Antiq. expliq. Tom. i. par. 2.*

(p) *Ibid. num. 77.*

(s) *Class. prim.*

during which all hostilities ceased among the several states of *Peloponnesus*. Throwing the *discus* or *quoit* was one of the exercises performed in those games, and the *discus* of *Iphitus* was deposited in the temple of *Juno* at *Olympia*; upon which the cessation of arms, always observed at that solemnity, being ingraved was then publicly read. Which inscription, as the Historian observes, was not cut in strait lines, but in the form of a circle. Tho whether the letters were placed on the face of the *discus*, or upon the edge, like some modern coins, is not expressly said. The words are these: Ταύτην [ἐκ χειρὶ] ἔκ ἐς εὐθὺς ἔχει γεγραμμένην, ἀλλὰ ἐς κύκλῳ γῆμα περίεσιν ἐπὶ τῷ δίσκῳ τὰ γράμματα. That is: *Hæc [induciae] non recto versuum ordine sunt inscriptæ, sed literis in disco orbem ambientibus (u).*

488. May &c.
1750. Read
June 28. 1750.
Fig. 23.

But I return to the inscription on the cup, which, as I apprehend, may be thus read in the common Greek characters.

M N Δ
ΒΑΣΙΛΕΥΣ ΜΙΘΡΑΔΑΤΗΣ ΕΥΠΑΤΩΡ
ΤΟΙΣ ΕΝΤΟΣ ΤΟΥ ΓΥΜΝΑΣΙΟΥ ΕΥΠΑΤΟΡΙΣΤΑΙΖ
ΓΥΦΑ vel ΓΟΥΦΑ ΔΙΑΞΩΖΕ.

In Latin thus :

*Monumentum dedit
Rex Mitradates Eupator Eupatoridis
in gymnasio [vel intra gymnasium]
Gypba [vel Gupba] servavit.*

I. The letters M N Δ stand by themselves over the rest, which are placed below them in the form of a circle; which circle is made up of the four segments here put one under another, when united in one orbicular line. And the situation of these three letters in the annexed draught shews, over what words of the circular part they are placed. All the words from ΒΑΣΙΛΕΥΣ to ΕΥΠΑΤΟΡΙΣΤΑΙΖ take in above half the circumference of the rim of the cup; and the division of this last word was occasioned, as I suppose, by one of the handles being fixed there. The two last words stand at a considerable distance from the rest, on the remaining part of the rim.

II. If those three letters above the rest are not a date, they may, as I conjecture, stand for *μνημόσυνον δῶκε* for *ἔδωκε*, without the augment. Where the word *μνημόσυνον* seems to be used in the same sense, as we meet with it in *Catullus*, when he says, *Verum est μνημόσυνον mei sodalis (x).*

III. In the circular line *Μιθραδάτης* is put for *Μιθριδάτης*, as his name is usually written. And so it is spelt in two other inscriptions, both found by *Spon* in the isle of *Delos*, at a considerable distance from each

(u) *Pausanias*, Lib. v. c. 20. p. 427. ed. Lips. 1626. Where *Pet. Faber* for *ἐς εὐθὺς* reads *ἐς εὐθὺ*, *Agonist. Lib. II. c. 26.*

(x) *Epigram. ad Asinium.*

other,

other, and published in his *Voyage d'Italie etc* (y). The like spelling may also be seen both on the coins of this prince (z), and in the edition of *Tacitus* published by *Lipsius*. These inscriptions were cut on the remaining pedestals of two statues, which had formerly been placed there, one in honour of *Mithridates Euergetes* the father of this Prince, and the other of himself. I shall here transcribe them both, as they will serve to illustrate that on the cup. The former runs thus :

ΒΑΣΙΛΕΩΣ ΜΙΘΡΑΔΑΤΟΥ
ΕΥΕΡΓΕΤΟΥ ΣΕΛΕΥΚΟΣ
ΜΑΡΡΑΘΩΝΙΟΣ ΓΥΜΝΑΣΙΑΡΧΩΝ.

In Latin :

*Regis Mithradatis Euergetae Seleucus
Marathonius gymnasio praefectus
statuam posuit.*

The latter is as follows :

ΒΑΣΙΛΕΩΣ ΜΙΘΡΑΔΑΤΟΥ ΕΥ
ΠΑΤΟΡΟΣ ΕΥΤΥΧ ΤΟΥ
ΜΙΘΡΑΔΑΤΟΥ ΕΥΕΡΓΕΤΟΥ
ΔΙΟΝΥΣΙΟΣ ΝΕΣ ΝΟΣ
ΑΘΗΝΑΙΟΣ ΓΥΜΝΑΣΙΑΡΧΗ
ΣΑΣ ΑΝΕΘΗΚΕΝ.

In Latin :

*Regis Mithradatis Eupatoris Eutych . . filii Mithradatis Euergetae
Dionysius Nes . . nus Atheniensis gymnasio praefectus posuit statuam.*

It appears highly probable by these inscriptions, that there was a *gymnasium* (a), or college, at that time in *Delos*. And the same writer observes, that near to the former he found eleven pillars standing without chapiters, and others lying on the ground. At which place, as he says, the inhabitants of the neighbouring isles (for *Delos* was then uninhabited) have a tradition, that a college antiently stood ; and he further adds, that the corsairs still continue to call this island by the name of *The schools* (b). And therefore it is not improbable, that this cup, which some years since was found in the port of *Antium*, and (as Dr *Pococke* says) is now placed among the antiquities of the capitol at Rome, might

(y) *Tom.* 111. pag. 86.

(z) *Numism. Pembrock. Par.* 11. tab. 66.

(a) By the word *gymnasium* among the Greeks was meant a number of contiguous buildings, with portico's and open walks, so disposed as to be fitted, some for bodily exercises, and others for the study of the liberal arts and sciences.

(b) *Tom.* I. pag. 177.

formerly have been brought from that island: and the name *Eupatoridæ* given to those of the college, in honour of this King *Mithridates*, their benefactor; who seems to have been pleased with the name *Eupator*, ascribed to him in the latter inscription, since he built a city in *Paphlagonia*, which he called *Eupatoria*, as we are told by *Pliny* (c) and *Appian* (d). By the latter of whom we learn also, that he was fond of fine ornaments, and elegant furniture; and had made a vast collection of them, of which vases were always esteemed a considerable part (e). The imperfect word *ERTRX* which follows *Eupatoris* in the same inscription, may perhaps stand for *ERTRXOYΣ*, *Eutycheis*, or *Felicis*; as that word occurs at length in another inscription discovered by *Spon* in the same island, of which the two names *ΔΙΟΝΥΣΙΟΥ ΕΡΤΥΧΟΥ* then only remained (f). Tho indeed I have not met with that name elsewhere ascribed to *Mithridates*; notwithstanding *Cicero* mentions a large catalogue of extravagant titles, which had been heaped upon him by his flatterers. *Mithridatem*, sais he, *Deum, illum Patrem, illum Conservatorem Asiae, illum Euium, Nysium, Bacchum, Liberum nominabant* (g). But yet it is possible, that the title *Eutycheis* might have been given him, on account of his great success in war for many years. As *Sylla* afterwards assumed that of *Felix*, for the like reason; and upon his triumph at *Rome*, for his victory over that powerful Prince, had also his other titles of *Conservator* and *Pater* ascribed to him, as we are told by *Plutarch* (h). These inscriptions give us also the names of two Presidents of that college, the former of whom is called *Seleucus* of *Marathon*; and the latter *Dionysius* of *Athens*, who might possibly have taken the name *Dionysius* from his patron *Mithridates*, who besides the name *Eupator* assumed also that of *Dionysus* as the same author relates (i). But the second name of this latter President *Nes nus* being imperfect, I am at a loss how to fill it up.

IV. The letters next following *Ευπάτωρ* upon the cup I take to stand for *τοῖς ἐνὸς*; tho they are strangely confused, by reason of a mistake, which seems to have been made at first in writing the inscription. In the word *γυμνασίᾳ* the two letters *ν* *α* are united in one character. And as to the expression *ἐνὸς γυμνασίᾳ*, which here refers to place, and not to time, as it more usually does; *Hesychius* has, *ἐντὸς τῆς σώματος*; *Phavorinus*, *ἐντὸς οἰκίας*; and *Apollonius Rhodius*, as cited by *H. Steph.* *ἐντὸς ὁσσεως*. And in the word *Ευπαλορίσταις*, the letters *στ* are put for *δ*, and *ζ* for *ς*.

V. Of the two last words upon the cup, which (as has been observed) stand separate from the rest, the former is a proper name; but whether the first character in it was designed only for a *Γ*, or a combination of the two letters *ΓΟ*, I am not certain. In the following word *διάζωζε*

(c) *Nat. Hist. Lib. vi. cap. 2.* (d) *Rom. Hist. pag. 251. ed. Steph.* (e) *Rom. Hist. pag. 251. ed. Steph.* (f) *Tom. iii. pag. 87.* (g) *Pro Flacc. c. 25.* (h) *Vit. Syll. Tom. v. p. 865 ed. Steph.* See likewise *Pighii Annal. ad A. U. 672.* (i) *Ibid pag. 250.* See also *Plut. Sympos. l. 1. q. 6.*

without the augment, for διέσωσε, ξ is put for σ; and likewise ζ, as in the preceding word 'Ευπαλορίσταιζ. I should have been led to imagine, that *Gypba*, or *Gupba*, might be the name of the Sculptor, and διάξωζε stand for διέξωσε, *exsculpsit* or *persculpsit*; it being a common practice for artists to set their own names to their works; had it not appeared an objection to this, that the inscription is no better executed; and likewise that neither the form, nor combination, of the letters seems to suit with the age of *Mithridates*. I am therefore inclined to think, that the cup did not come into the hands of this person till a considerable time afterwards; who seems to have been no great scribe, but was willing however to preserve both the memory of so curious a vase, and his own as the possessor of it.

As the several variations from the common manner of spelling, which occur in this inscription, are to be met with in *Greek* writers; I shall trouble this assembly with no further remarks upon them, than just to observe, that *Lucian* in his humorous discourse, intitled *Judicium vocalium*, complains, that among many other innovations, which had then crept into the language, τ had invaded the place of δ, and both ξ and ζ that of σ.

Thus I have attempted to offer my thoughts upon this intricate inscription, and explain it in such a manner, as appeared to me the most probable, from a copy of it, communicated to me some years since by *Smart Letbieullier*, Esq; *F. R. S.* who had it from *Father Revillas* at *Rome*.

P. S. *Spon* was at *Delos* in *Aug.* 1675, when he copied the two inscriptions mentioned above. But it seems to be owing to the smallness of his page, that the former of them is printed in 4 lines; for *Wheeler*, who took it at the same time, brings it into 3, in the following manner:

ΒΑΣΙΛΕΟΣ (k) ΜΙΘΡΑΔΑΤΟΥ
ΕΥΕΡΓΕΤΟΥ ΣΕΛΕΥΚΟΣ
ΜΑΡΑΘΟΝΙΟΣ ΓΥΜΝΑΣΙΑΡΧΩΝ (l).

Tournefort, who was there in *October* 1700, confirms this draught of *Wheeler*, as to the form of it; and sais, the inscription was cut upon a square pedestal, 2 feet 5 inches high, and 2 feet 1 inch broad. And as part of it was defaced at the beginning of the lines, I have here transcribed the copy published by him, of what then remained.

..... Ι ΜΙΘΡΑΔΑΤΟΥ
..... ΣΕΛΕΥΚΟΣ
..... Σ ΓΥΜΝΑΣΙΑΡΧΩΝ (m).

(k) The O in this word seems to be a mistake in the print for Ω.

(l) *Travels*, pag. 56.

(m) *Voyage du Levant*, Tom. I pag. 297. a Paris 1717. qu.

But Dr *Anthony Askew*, F. R. S. who was there in Oct. 1747, tho he found the pedestall pretty much broken, in the manner described by *Tournefort* ; yet observed more letters of the inscription then remaining, than are given us by him. The difference will appear by the following copy, with which the Doctor was pleased to oblige me out of his large and curious collection of inscriptions, and other valuable antiquities :

..... Σ ΜΙΘΡΑΔΑΤΟΥ
..... ΟΥ ΣΕΛΕΥΚΟΣ
..... Σ ΓΥΜΝΑΣΙΑΡΧΩΝ.

The latter inscription mentioned above, as erected in honour of *Mithridates Eupator*, was found, as *Spon* informs us, upon a round basis, at a great distance from the other. It is likewise taken notice of by *Wheeler*, tho he has given no draught of it. But *Tournefort* supposes it to have been gon, before he was there ; because, upon searching for it, he could not find it. Which indeed is not to be wondered at ; since we are told by *Spon*, that *Delos* being intirely in ruins, and without any inhabitants, the antient remains, being of *Parian* marble, are frequently carried away by strangers, either for curiosity or use.

However *Tournefort* discovered part of another pedestall, in the shape of a cylinder, not far from the former of the two already mentioned. It lay half buried in the sand, but being opened and washed, an imperfect inscription was found upon it. The remaining words of which are these :

ΒΑΣΙΛΕΩΣ ΝΙΚΟΜΗΔ . .
ΤΟΥ ΕΥΓΟΝΟΥ (n) ΒΑΣΙΛΕΩΣ
ΝΙΚΟΜΕΔΟΥ (o) ΕΠΙΦΑΝΟΥ
..... ΚΟΥΡΙΔΗΣ (p) ΔΙΟΣΚΟΡΙΔΟΥ
ΡΑΜΝΟΥΣΙΟΣ ΓΥΜΝΑΣΙΑΡΧΟ . .

In Latin thus :

*Regis Nicomedis nepotis regis Nicomedis Epiphanis Dioscorides Dioscoridis
Ramnusius praefectus gymnasio statuam posuit.*

King *Nicomedes*, to whose honour this inscription was erected, is here called εγγονος, that is *grandson*, of *Nicomedes Epiphanes* King of *Bitynia*. Which seems to confirm the account of *Appian*, who gives the name *Nicomedes* to the three last *Bitynian* Kings. The first of whom

(n) I am obliged likewise to Dr *Askew* for a copy of this inscription, taken by himself ; by which it appears, that Υ in this word is a mistake for Γ, as the Doctor has written it.

(o) The Ε in this word is also a mistake for Η, as it is written in the first line here, and by Dr *Askew* in both places.

(p) I take this to be the same name as the following ; the middle syllable of which is usually written with Ο, but sometimes with ΟΥ. See *Grut. pag. LXXXVI. num. 2.*

being, as he ſais, the ſon of *Prusias* (and here ſtilled *Epiphanes*) was ſucceeded firſt by his ſon *Nicomedes Philopator*, and then by his grandſon, whom he calls barely *Nicomedes*, without ſubjoining any *cognomen*, agreeably to this inſcription. And he further adds, that it was this laſt, who left his kingdom to the Roman people by his will (*q*). Which bequeſt was made in the year of the city 679, and was ſoon followed by their third and laſt war with *Mithridates Eupator* (*r*). Some have ſuppoſed this account of *Appian* to be erroneous, through a miſtake in the copies; becauſe other writers generally mention but one *Bithynian* King, as ſucceſſor to *Nicomedes Epiphanes*, who was alſo named *Nicomedes*, made the Roman ſtate his heir, and has been taken for his ſon (*s*). But the inſcription, by calling this latter the *grandſon* of the former, as *Appian* does, corroborates his account; who likewiſe ſais expreſſly, that his ſon *Philopator* was confirmed in his father's kingdom by a decree of the Roman ſenate (*t*).

I thought it proper to take notice of this inſcription, not only as it ſeems to clear up the ſucceſſion of the *Bithynian* Kings; but likewiſe as it contains the name of a third Preſident of the college in *Delos*, and ſhews the regard that was paid to it by the princes of different countries in *Aſia*.

An explanation of an ancient inſcription diſcovered at Rutcheſter, the laſt ſtation in England, upon the Roman Wall, 1744; by John Taylor, LL D. Chancellor of Lincoln, and Reſiſter of the University of Cambridge. N°. 482. p. 344. Jan. & Feb. 1747. Read Jan. 15. 1746-7.

| | | |
|----------|-----|---------------------------------|
| | XI. | IMP. CAES. M. AVRELIO. |
| | | SEVERO. ANTONINO. |
| AV. | | PIO. FELICI. X. PARTHIC. |
| i. e. | | MAX. BRIT. MAX. GERM. |
| Auguſto. | | MAX. PONTIFICI MAXIM. |
| | | TRIB. POTEST. XVIII. IMP. II. |
| | | COS. IIII. PROCOS. P. P. CO. I. |
| | | FIDA. VARDVL. CREO \oplus ANO |
| | | NNANA. FECIT. SVB. CVRA. TCO |
| | | LEG. XX. GR. |

According to the copy given me by Dr *Hunter* of *Durham*, who copied the inſcription this laſt ſummer.

This inſcription addreſſed to *Caracalla* has nothing in it very ſingular, except the title of the cohort that dedicated it, namely,

FIDA. VARDVL. CREO \oplus ANONNANA.

concerning which I ſhall offer theſe few conjectures.

The

(*q*) *Rom Hiſt.* pag. 175. ed *Steph.*
lant, Achemened. imper. Tom II. pag 345. 354.

(*r*) *Pigh. Annal Rom.*

(*s*) See *Vail-*

(*t*) *Vaillant* has given us the impreſſion of an head upon a ſilver tetradrachm, with this title placed over it, *Nicomedes Epiphanes rex Nicomediae*; whom he takes to have been the grandfather of *Prusias*: *Achemened. imp. Tom. II. p. 304.* And Dr *Mead* has ſuch a coin in his collection, with theſe words on the reverſe, ΒΑΣΙΛΕΩΣ ΕΠΙΦΑ-
 ΝΟΣ

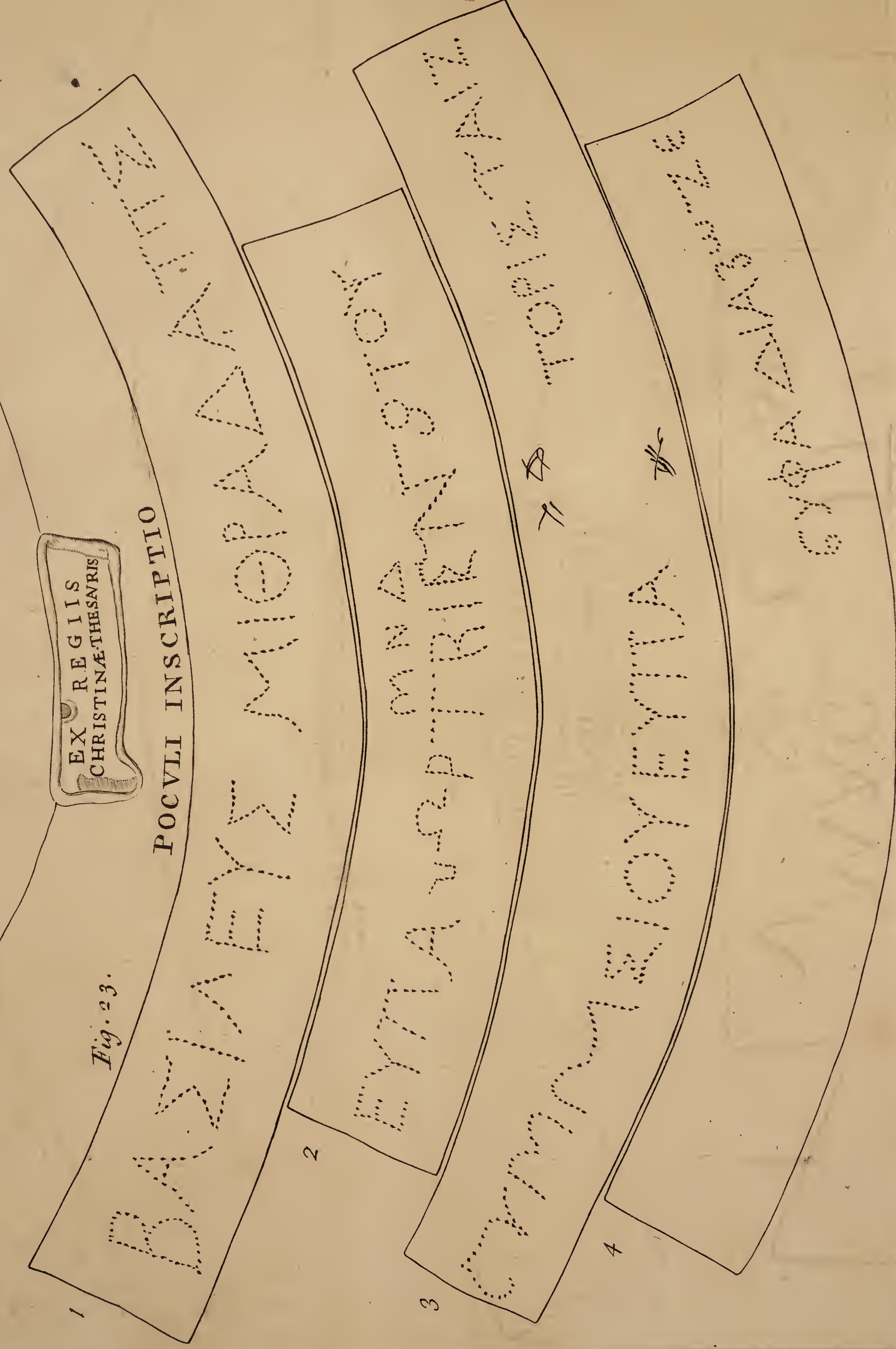
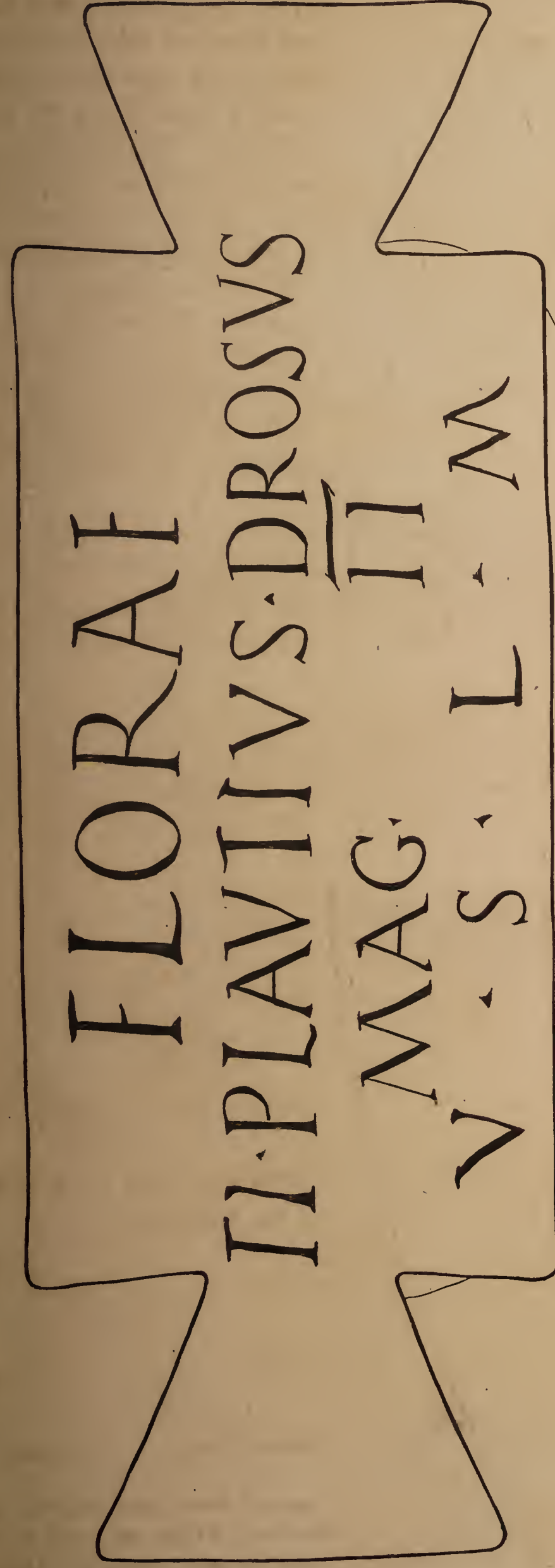


Fig. 23.

The *Varduli* were a people of *Hispania citerior*, mentioned by *Pomponius Mela* and others; and are recorded now, in no leſs than three inſcriptions, to have ſerved in *Britain* as auxiliaries. The two other are printed in *Horsley's Britannia Romana*, *Northumb.* N^o. xciv^s. *Durb.* xxvi. We find troops of ſeveral nations to have been here upon the ſame occaſion, as *Cohors prima, ſecunda, &c. Batavorum, Dacorum, Nerviorum, Tungrorum, Delmatarum, Thracum, &c.*

This cohort of the *Varduli* is intituled FIDA, a very common appellation, and moreover CREOΘANONNANA; the laſt letters of which I ſeparate, and read without inserting a ſingle letter ANTONINIANA, thus, ANTONNANA. Nothing is ſo frequent in inſcriptions, as this compendiary way of writing ANTONINVS, and it's derivatives. Thus in *Horsley's* inſcriptions,

M.AVR.ANTONNO.PIO.

North. cxiii.

M.AVRELI.ANTONINI.PII.FELIC.

North. xciv^s.

COH. IIII. BR.

ANTONNIA.

North. lxxvi.

Upon this hint therefore I am perſuaded, that when the ſtone is next inſpected, theſe little *apices* will appear, which are eaſily overlooked, when this brief manner of writing is not expected or attended to.

In regard to the appellation *Antoniniana*, it is obſervable of the ancient militia, that ſeveral of their cohorts and legions, as well *Roman* as provincial, complimented themſelves with the imperial ſurname, of which I ſhall produce ſome inſtances.

DIS. MANIBVS.

C. ANTONI. C. F.

OVF. AEGRILLI.

MIL. LEG. VII. CLAVDIAE.

PIAE. FELIC. ET. FIDEL.

VIX. ANN. XXXIIX.

M. VII. D. VIII.

M. AEGRILIUS. FRATRI

DVLCISSIMO. F. C.

Gruter, dxxi. 4.

ΝΟΥΣ ΝΙΚΟΜΗΔΟΥ. Where *Epiphanis* the *cognomen* ſtands firſt, as it ſometimes does in antient writers: *Cic. Orat. c.* 27. An impreſſion alſo of a ſimilar coin, with the head on one ſide, and the ſame legend on the other, may be ſeen among the *Numismata Pembroke. Par. II tab.* 69. But *Vaillant* ſays, that he had not met with any coin, which had on it the head of *Nicomedes Epiphanes* the ſon of *Prusias*, to whom the inſcription refers, which is cut on the ſtone. *Ubi ſupra,* pag. 355.

I. O. M.
 COH. I. AEL.
 DA. GORD
 .ANA.

Horſt. Cumberl. VIII.

i. e.

Iovi Optimo Maximo COHors I^{ma}. AELia DACorum GORDIANA.

NVMINI FOR
 TIS.FORTVNE.
 M. AVRELIVS.
 MARINVS. MIL.
 COH.VII. PR. P.V.
 SEVERIANAÆ.

Gruter. MXIII. IO.

i. e.

COHortis VII^{mæ} PRætoriae Pia Viêtricis SEVERIANAÆ.

But the imperial addition ANTONINIANA is perhaps the most frequent of any, as the name of *Antoninus* was assumed by a long series of Emperors.

DEAE. EPONAE. M. OPILIVS. RESTIO.
 MILES. LEG. XXII. ANTONINIANAÆ. P. P. F.

Gruter. LXXXVII. 4.

H. D. D.
 DEABVS. MATR.
 IVLIVS. REGVLVS. MI
 LES. LEGIONIS. VI.
 ANTONINIANE.

Gruter. XCII. 2.

D. M.

D. M.
MARINA. SECVN
DINA. VIX. ANN. LIII.
L. AVRELIA. SATVR.
VIX. ANN. XXI.
L. AVRELIVS. INGENVVS.
MILES. LEG. XIII. GEM.
ANTONINIANAE.
MATRI. ET. SO
RORI. POSVIT.

Gruter. DXXVIII. 8.

D
ET. MEMORIAE.
AETERNAE. VE
GETINIAE. RO
MANAE. MEM
MIVS. RVSTICVS.
MILES. LEG. V.
VICTRICIS. AN
TONINIAN. CON
IVGI. INNOCENTISSI
MAE. LOCO. PERE
GRINO. DEFVNCT.
P. C. ET. SVB. ASCIA. D.

Reinesf. VIII. 68.

But more particularly the two following, as they both bear date under *Caracalla* ; and one of them the very year of his fourth consulate, when he was collegued with *Balbinus*, and of consequence nearly coæval with ours. This is to be seen in *Gruter*, XLIV. 2. and runs thus :

P. AEL. VICTORINVS. ROMANI. VIVENI. F.
AVRELIANA. POPA. MIL. COH. V. PR. ANTONINIA
NE. P. V. 7. VERI. HERCVLI. DONV. POSVIT. LIBI
ENS. VOTO. VT. GRATIAS. AGO. DEDICAVIT.
III. NON. NOVEMBRES.
IMP. ANTONINO. AVG. N. IIII. ET. D.
CAEILIO. BALBINO. II. COS.

The other is somewhat earlier, and bears date between his second and third consulate.

IMP.

IMP. CAES. M. AVRELIO.
 ANTONINO. AVG. SEVERI. F.
 TRIB. POT. VIIII. COS. II.
 COH. I. VIG. ANTONINIANA.
 CN. RUSTICVS. RVFINVS. PRAEF. VIG.
 TIB. CLAVDIVS. MARCELLINVS. TRIB.

etc.

Reinesf. III. ult.

There is yet a third, which, besides the title *Antoniniana*, and the correspondency of date (for it was erected in the consulate we have been speaking of) is likewise remarkable for the same *scriptura compendiaria*. I shall give it whole from *Gruter*, CVIII. I.

IN. H.

D.D.

GENO. SANC.
 TO. M. AVREL.
 CL. POMPEIAN
 VS. MIL. LEG. VIIII.
 ANTONINAE
 AVG. B. F. COS. K.
 IANVAR. IMP. D. N.
 ANTONINO. IIIII. ET.
 BALBINO II. COS.

What remains to be accounted for is CREO⊕. Which letters if any one should compare with *Horsley's Durham* inscription, N^o. XXVI. where mention is made of the same *Varduli*, he will find a very strong resemblance; and be apt to conclude, that what explains the one, will bid very fair to explain the other. That inscription runs thus,

. O. M.

.... ATI. COH.

VARDVLOR.

CREQ∞

V. S. LL. M.

CR then I take to be distinct marks, and expressive of *Civium Romanorum*. And of this I find little room to doubt, when I observe the same marks applied to several corps, who were as strictly provincial as our *Varduli*; such as *Afri*, *Asturientes*, &c.

L.PRAESENTIO.L.FIL.
LEM.PAETO.
L.ATTIO.SEVERO.
PRAEF.CO.H.I. AFR.
C.R.EQ.IVDICI.SELECTO.EX.
V DEC.PR.AVXIMI.PAT.COL.
AEDILI.II.VIR.ANCONAE.

Gruter. CDLIX. 9.

SEXTL.CLASENO
. PRAEF.CO.H.
III.ASTVR.EQ.C.R. *etc.*

Gruter. CCCLXXXVIII. 3.

DIANAE
SACRAVIT
T.FL.
ITALICVS
PRAEF.
ALAE.I.VLP.
CONTAR.
∞.C.R.

Gruter. XL. 3.

DEIS DEABVS.
ALA.I.VLP.CONT.
∞. C.R.CVI.PRE.
Q. ER. INGENVVS.

Gruter. II. 6.

But still more exprefly,

. CEREALI.
... LEG. III. GALLICAE
... III.CO.H.GALLICAE.
civivm.ROMANORVM.

Gruter. CDXCIX. 3.

An antient Roman inscription at Rutcheſter.

And moreover what kind of citizens, we are told more than once;
viz. *Juris Italici*.

T.FL.BARSI.V
ETER.ALA.E.I.FL.
AVG.BRIT.
∞.C.R.IVRIS.ITALICI.
MEMOR. FR
ATRI.SVO.POSVIT.

Gruter. DXLI. 8.

T.F.VERECVND.
MAG.EQVES.ALA.E.
I.FLA.AVG.BRIT.∞.
C.R.IVR.ITALICI.

Gruter. DXLII. 7.

The freedom of the city had been for some time before this a regular reward for the fidelity of the provinces, or any other military or civil merit. The famous oration of the Emperor *Claudius*, or the act for incorporating the people of *Vienne* in *Gaul* (a large fragment of which is preserved in *Gruter*, p. DII.) is a remarkable instance of what I have advanced. QUID. ERGO. NON. ITALICVS. SENATOR. PROVINCIALI. POTIOR? EST. IAM. VOBIS. CVM. HANC. PARTEM. CENSURAE. MEAE. APPROBARE. COEPRO. QUID. DE. EA. RE. SENTIAM. REBVS. OSTENDAM. SED: NE. PROVINCIALES. QUIDEM. SI. MODO. ORNARE. CVRIAM. POTERINT. REICIENDOS. PVTO. For so the last words must be emended, as they have been restored by *Reinesius* and *Grævius*.

Afterwards, probably a little before the date of our inscription, which is near the end of the reign of *Caracalla*, came the general constitution of that Emperor; the memory of which being fresh might probably occasion the insertion of the words *Civium Romanorum* in this monument. *In orbe Romano qui sunt, ex Constitutione Imperatoris Antonini Cives Romani effecti sunt*, are the words of *Ulpian*, in the first book of the *Digest*. *Tit. de Statu Hominum*, Law XVII. That the *Antoninus* there mentioned was our *Caracalla*, is abundantly made good by *Baron Spanhem*, in his *Comment* upon that text. To which let me add the words of *Prudentius*, produced by the same learned writer upon the occasion.

*Hinc frenaturus rabiem Deus, undique gentes
Inclinare caput docuit sub gentibus isdem,
Romanosque omnes fieri, quos Rhenus et Ister,
Quos Tagus aurifluus, quos magnus inundat Hiberus.*

Corniger

An antient Roman inscription at Rutcheſter.

5021
1291

*Corniger. Heſperidum quos interlabitur, et quos
Ganges alit, tepidique lavant ſeptem oſtia Nili.
Jus fecit commune pares, et nomine eodem
Nexuit, et domitos fraterna in vincla redegit.*

What remains EO⊕, I would reſtore EQ⊕, according to the model of the *Durham* inſcription produced above:

COH. VARDVL. C. R. EQ.∞.

For the difference in ſtones is ſo minute in this particular, that I will venture to pronounce, tho' I never ſaw the inſcription, it may as well be one as the other. And I read the whole title of the cohort thus:

COhors Ima FIDA VARDVLorum Civium Romanorum Equitata
Milliaria ANTONINIANA.

Concerning which I have ſomething to offer.

The frequent mention of equeſtrian cohorts, or, to ſpeak more adequately, of *cohortes equitae*, in old inſcriptions, I obſerve to have been a great choque upon ſeveral Antiquaries, who have been taught to conſider the cohorts as appropriated to the foot ſervice, as the *alae* and *turmæ* were to the horſe. Mr *Horsley* in particular, p. 94. imagines, the *Cohors prima Claudia Equitata*, which he met with in the *Notitia*, was intended to intimate, that this cohort had been promoted from the foot to the horſe ſervice. But when that Gentleman was led, by the mark or monagram in the *Durham* inſcription referred to in theſe papers, to conſider that corps as conſiſting of a thouſand horſe, his difficulty is increaſed to that degree, that he does not know what to affirm upon it. Now of all this there is a very eaſy ſolution.

The auxiliary or provincial cohorts (for of them only I obſerve what follows) were either intirely or purely foot, like the legionary and ordinary cohorts; or elſe they had a mixture of both kinds of militia, and conſiſted of horſe and foot together.

IMP. CAESAR. DIVI. VESPASIANI. F. DOMITIA
NVS. AVGVSTVS. GERMANICVS. PONTIFEX. MAXI
MVS. TRIBVNIC. POTES. XII. IMP. XXII. COS. XVI.
CENSOR. PERPETVVS. P. P.
PEDITIBVS. ET. EQVITIBVS. QVI. MILITANT. IN. COHO
RTE. III. ALPINORVM. etc.

Gruter. DLXXIV. 5.

This latter ſort, as they could not properly be ranked under either denomination of horſe or foot, (for they were made up of both) ſeem to have appropriated to themſelves the diſtinguiſhing title of *COHORTES*

An antient Roman inscription at Rutchester.

EQUITATAE, corps of infantry with a mixture of horse. And of this term, so very significant, and so little understood, I find frequent mention.

L. BRVTIO. L. F.

PAL. CELERI.

EQVO. PVBLIC.

PRAEF. COH. III. AVG.

THRAC. EQVIT.

Gruter. DXXXIV. 2.

Q. CAECILIO. Q. F.

AN. OPTATINO.

PRAEF. COH. I. AQVI

TANOR. EQVIT.

PONTIA. T. F. SABI

NA. MATER.

FLAMINICA.

L. D. D. D.

Gruter. DXXXIV. 4.

P. LICINIO. P. F.

GAL. LICINIANO.

PRAEFECTO.

COHORTIS. VII.

PRAELECTORVM.

EQVITATE. IN.

GERMANIA.

TRIBVNÓ.

MILITVM. LEG.

VII.

EVIA. PRAEFECTO.

Gruter. DL. 4.

P. LICINIO.

P. LICINIO. P. F.
GAL. MAXIMO.
PRAEFECTO.
COHORTIS. II.
GALLORVM.
EQVITATE. IN.
DACIA. TRIBVN.
MIL. LEG. VII.
CLAVDIAE. PIAE.
FIDELIS. P. LICINIUS.
LICINIANVS.
FRATRI.

Gruter. DL. 5.

M. MAENIO. C. F. COR. AGRIP
PAE. ET. VSIDIO. CAMPESTRI.
HOSPITI. DIVI. HADRIANI.
PATRIS. SENATORIS. PRAEF. COH.
II. FL. BRITTON. EQVITAT.
ELECTO. A. DIVO. HADRIANO.
ET. MISSE. IN. EXPEDITIONEM. BRIT
TANNICAM. TRIB. COH. I. HISPA
NOR. EQVITAT. PRAE. ALAE. I.
GALLOR. ET. PANNONIOR. CATA
PHRACTAE. etc.

Reinesf. VI. 128.

Nor have we these testimonies only, but also a full and decisive proof of this denomination, and, what is yet behind, of their number also, in a writer very well versed in military affairs, *Hyginus*, who wrote a Treatise *de Castrametatione*, in the time of *Trajan*. From him we are informed, that these troops were called *milliariae*, as consisting of a thousand private men, part horse and part foot. The proportion of the former of these to the latter was nearly as one to three, viz. 240 to 760, instead of 250 to 750. Which little difference was possibly occasioned by the necessity of dividing them into *centuriae* and *turmae*. The author's words are :

Habet Cohors Equitata Milliaria pedites septingentos sexaginta, centurias decem, equites ducentos quadraginta, turmas decem.

And again ;

Meminerimus itaque ad computationem cohortis equitatae milliariae pedaturam ad mille trecentos sexaginta dari debere.

Which

Which is to be thus explained: the *pedatura* of this irregular corps, in their incampment, would not be the same as the *pedatura* of an uniform body of infantry, of the same number, but would exceed it by 360 feet; for the proportion of the room of one horseman to one foot-soldier he assigns as $2 \frac{1}{2}$ to one.

Omnis miles provincialis accipit pedaturam, pedem, adjecta quinta — eques autem duos semis, adjecta quinta. — Habebit itaque cohors equitata milliaria equites ducentos quadraginta, quos redigo ad peditem, ut pedem, quod accipit miles, ad duos semis, quod accipit eques. Fit dimidia sumpta ductum quinquies: sic tractabimus numerum equitum, fit centum viginti: ducemus quinquies, fiunt sexcenti: accedunt ex eadem cohorte milliaria, detractis equitibus, reliqui pedites septingenti sexaginta: fit cum superiori mille trecenti sexaginta.

For the mark \oplus I account thus: the usual note of a thousand is either I between CC's, thus CIO; or else X, thus CXO. The former figure, when closed at the top, exactly resembles an ancient M, thus \oplus ; and the latter, when shut up, a figure of eight inclined ∞ . Both which marks have been long used to express a thousand. The latter is the mark before us, the X between CCs, but closed in on all sides, thus, \oplus , if this be in reality the figure upon the stone. For Mr Gordon in his *Iter Septentrionale* copying an inscription, wherein there was the mark of four thousands, gave us the thousand inclosed on all sides, the very mark in our inscription; but upon Mr Horsley's inspection it turned out to be the second figure, the thousand inclosed only at both ends thus ∞ .

The last part therefore of the inscription is to be thus understood:

COHors I.^{ma} FIDA VARDVLorum Civium Romanorum EQuitata
Milliaria ANTONINIANA FECIT SVB CVRA T. CO.....
Legati, Tribuni, or Centurionis LEGionis XX.^{ma} Genio Romæ.

Which last words are to be applied to the Emperor, and contain a compliment at that time of day not unusual.

Concerning
two ancient
Camps in
Hampshire;
in a letter
from Mr Tho.
Wright to
James Theo-
bald, Esq;
F. R. S. N^o.
475. p. 273.
Jan. &c. 1745.
Read Feb. 7.
1744-5.

XII. Last summer, during my stay in the West of England, common report, and my own natural curiosity, led me to a place in Hampshire, called *Buckland Castle*, or, more vulgarly, the *Rings*; where I found two neighbouring camps about 3 furlongs asunder: the one very strong, with double ditches, and triple *vallums*, upon the top of an hill, three ways guarded by a natural ascent; the other upon a lower ground, close by a river (which defends it on one side), with a ditch and *vallum* half round, and a kind of morass on the other. The first contains about 10 acres; the latter about 7.; and the land lying betwixt them is, and has been time out of mind, called *Ambrose Farm*: besides an arm of the river,

river, or rather the sea (it being salt-water), running close up to the latter, is called *Ambrose Dock*.

From all which it appears not improbable, that this may have been a principal station belonging to *Aurelius Ambrosius*.

The inclosed is a true representation both of their form and situation (see Fig. 24, 25.): the large one is about a mile from *Limington*; and, Fig 24, 25. crosses the river opposite to the town, and facing the S. E. angle of it, is an artificial hill, known by the name of *Windmill Nap*; which I take to have been a beacon, by reason it commands almost all the *Isle of Wight*, the full passage of the *Needles*, and great part of the there level champagne, besides the camp itself.

Camden (tho' he takes no notice of these camps, yet mentions one of much less consequence, about twelve miles distance, called *Castle-Malwood*) says, "It is most certain, that, about the year 508, *Aurelius Ambrosius* had here many conflicts with the *Saxons*, with various success." But, again, the people of this country have a tradition, That, 3 miles to the W. of this camp, a famous battle was fought betwixt the *Saxons* and the *Danes*; in which so much blood was spilt, that a little river is said to have run blood, now called from thence *Danes Stream*.

XIII. The town of *Castor* is at present in a very low condition, containing no more than between 20 and 30 small cottages. It stands about 4 miles S. W. of *Norwich*, and by tradition, and some learned authors, is supposed to have been a considerable city, out of whose ruins *Norwich* took it's rise. *Skinner* says, in his *Etymologicon*, "*Castor in Com. Norf. olim VENTA ICENORUM: ex cujus Ruinis orta est Norwich civitas.*" However, at this day (excepting the camp) not the least trace or footstep of any thing remarkable is left remaining.

The camp itself lies near a furlong S. W. from the town of *Castor*, and leads you by a gentle descent down to the little river *Wentsum*, which swiftly glides close to the end thereof, and no doubt, at the first forming of the camp was designed to be part of the fortification on that side, as well as to supply the army with water, and to bring up such things as they wanted from the sea, if so be their communication by land should at any time be impeded. What confirms me in this opinion, is a large staple and ring of iron, which I myself have seen on the side of the tower that stands near the river; tho' now I find that curious monument of antiquity is taken away. However, on my supposition, this river must have been much larger at that time than it is at present, or it could have been of little use for water-carriage, from the smallness and shallowness of it's stream.

This river by some is called *Taus*, or *Tese*: but I imagine it did not formerly take that name until it approached the *Roman* camp at *Teseburgh*, 3 or 4 miles higher.

We are told by tradition, as well as by some learned authors, that the sea came up to this camp; and indeed every intelligent observer must

An account of the present condition of the Roman Camp at Castor in Norfolk, with a plan of it; in a letter from Mr Will. Arderon, F. R. S. to Mr Baker. N^o. 493. p. 196. Oct &c. 1749. Read Nov. 2 1749.

must confess, that the marine bodies found in every part of *Norfolk*, on the highest hills, as well as in the lowest pits and vallies, are indubitable proofs, that at some time or other the sea must have covered this whole county : but then we may be assured, by the present condition of this camp, that the sea hath not exceeded the level of it since it hath been in being, which, if we credit several of our antient Historians, it was upwards of 1700 years ago. It may therefore serve to prove, that the sea since that time has not exceeded these bounds, and that the fossils dug up above this level are more antient than it, tho' we have no proper *data* to discover how long before, the sea had passed this height.

I have been pretty exact in examining the situation of this camp, imagining the two sides had pointed due E. and W. and the 2 ends N. and S. : but I find they differ from it full 10° , after allowing for the variation of the needle : whence it is plain the meridian of the place must have altered better than $\frac{1}{2}$ a degree each century to the westward, provided the situation of the camp was placed due N. and S. when it was first formed. But possibly the *Romans* might not be exact as to the points of the compass, or perhaps this variation was at first dispensed with to accommodate the camp to the natural declivity of the ground. These difficulties however may better be cleared up, if some ingenious Gentlemen would take the trouble to examine some other *Roman* camps in different parts of this kingdom, to discover whether the *Romans* paid a strict regard to the disposing of their camps with their sides to the 4 cardinal points of the compass, which I think it will nearly amount to a demonstration that they did, if they are found to agree with this camp at *Castor* in it's variation from due N. and S.

The figure of the camp is not a square (as it is described by most authors who have writ upon it) but a parallelogram, whose two longest sides are each 440 yards, and it's ends or two shorter sides 360 yards each. These are it's dimensions without-side the rampart and ditch ; but within-side the said ditch and rampart the length is 392 yards, and the breadth 264. I measured the breadth of the fosse and rampart, which I found in some places, where it remains most perfect, to be 48 yards, tho' in others not above 30. And according to my computation the whole ground taken up, including the ditch and rampart, is 32 acres, 2 rood, and 36 pole ; or the area within the ditch and rampart 21 acres, 1 rood, 21 pole.

At about 3 or 4 furlongs N. W. of the camp rises a ridge of hills, appearing something like a second rampart, and descending gradually to the camp. These hills add greatly to the prospect, and must have been no little advantage to the safety of the place, as a constant watch might be kept thereon to prevent any surprize ; nor could an enemy advance nearer than the summit of these hills without being exposed to the view of the whole camp.

Three sides only of this camp have been fortified with a rampart, whose upper part was faced with a thick and strong wall made of lime
and

and flints, of which wall there are still remains in several places of the rampart, besides a very deep ditch that seems to have been most considerable on the E. and S. sides. The wall on the N. side appears to have been built at two different times; that is, it seems to have been raised higher than it was built at first, at some distance of time afterwards; for a parting may be observed at a certain height running from end to end.

The ruins of two old towers still remain, one of which stood on the N. side, and the other at the W. end; the last of which is at present the most considerable of the two. They were both built in a manner perhaps peculiar to the *Romans* at that time, and which it may not be improper to describe. They began first with a layer of bricks laid flat as in pavements; on that they placed a layer of clay and marle mixed together, and of the same thickness as the bricks; then a layer of bricks, afterwards of clay and marle, then of bricks again, making in the whole 3 layers of bricks and 2 of clay: over this were placed bricks and lime 29 inches, the outside being faced with bricks cut in squares (like the modern way of building in some parts of *Norfolk*), then bricks and clay again *stratum super stratum*, as high as the old ruins now remain standing.

The mortar is found extremely hard at this day: it is a composition of lime, sand, and ashes, and so compact that I could by no means break a piece of it of an inch diameter from the base of one of the towers at the E. gate, but on striking it with a sharp flint it flew off in dust.

The *Roman* bricks which I examined, were made of two different sorts of clay mixt; when burnt one appears red and the other white: at the time of my viewing them they were exceeding hard and solid, and far superior to any thing of the kind now made with us. Perhaps they are little worse than when they were first laid down. These bricks were made without the assistance or addition of sand, as is too much the practice at present here in *Norfolk*: for when sand enters the composition in any considerable proportion, it renders the bricks friable, soft, and rotten, subject to be broken or ground to pieces with the least motion or pressure. I took the exact dimensions of several of these bricks, and found their length to be $17 \frac{4}{10}$ inches, or a *Roman* foot and half; and their breadth $11 \frac{6}{10}$ inches, or precisely a *Roman* foot: which I think may serve as some proof that the *Roman* measures handed down to us by several authors are right, and may likewise inform us of the proportionable stature of man at that time. The thickness of these bricks is $1 \frac{3}{10}$ inch.

The great number of *Roman* medals that have been, and are still found in and about this camp, are to me a matter of great wonder. One Lady who lives near the place, has (I am credibly informed) picked up at least 100 with her own hands, and several are daily gathered up by boys, and sold to strangers who come to visit the place.

That these pieces have been used as money I think exceeding clear, from their different degrees of perfection, some being worn almost quite smooth, others having imperfect busts without letters, and others again having both the busts and inscriptions fair and legible, which could not happen, I think, but from their different wear as money. But then how such quantities of them should become scattered, as if sown, in this and other *Roman* stations, is a difficulty I must leave to those better versed in these matters to resolve.

Fig. 26.

I send herewith a plan of the camp in it's present condition, (*Fig. 26.*) that my description may the better be understood.

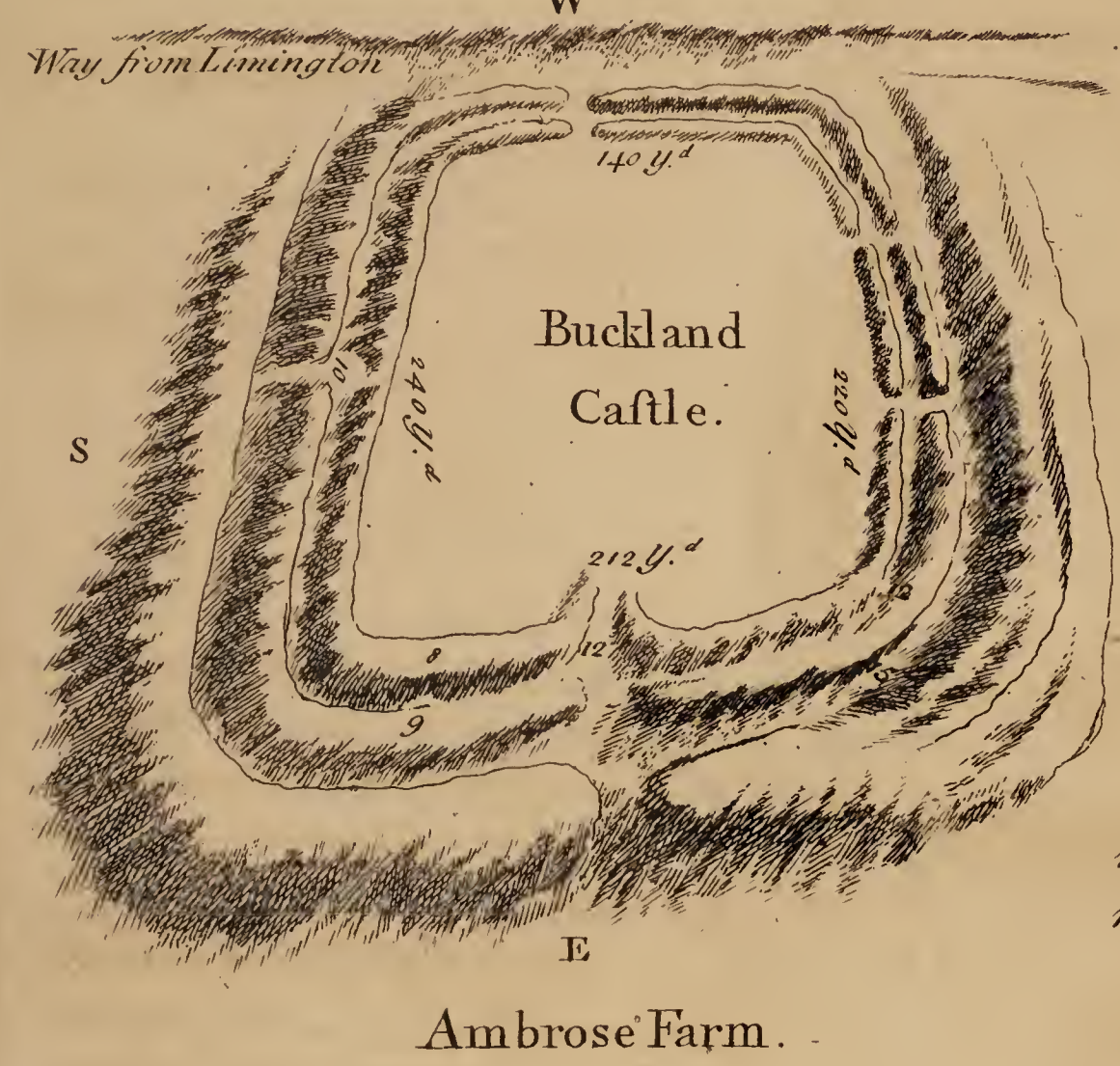
An attempt by John Ward, Rhet. Prof. Gresh. & F. R. S. to explain some remains of antiquity lately found in Hertfordshire; and communicated to the R. S. by Will. Freeman, Esq; F. R. S. Feb. 14. 1745. N^o. 476 p. 349. Apr. &c. 1745. Read April 4. 1745.

XIV. By the account, which that Gentleman delivered in with them, they are said to have been found in a chalk pit, near the side of *Rooky Wood*, in the parish of *Barkway* in *Hertfordshire*. A Farmer's man digging chalk there about 2 years since brought them with the chalk into his master's yard, and taking no notice of them mixed them with the dung. But very lately on throwing up the dung in that yard they were discovered by Mr *Raymond*, Steward to *George Jennings, Esq;* Lord of the manor. Upon which at the desire of *Pulter Forester, Esq;* an ingenious Gentleman in that neighbourhood, they were shewn to him; who had the curiosity to delineate those two plates, which have inscriptions upon them, and sent the draughts to Mr *Freeman*. And he soon after receiving the originals from Mr *Jennings* laid them, together with the two drawings, before this Society; when the form of the plates, and novelty of the work, occasioning the Assembly then present to think they might deserve some further consideration, it was their pleasure to desire me to give them my opinion, both as to the reading, and design of them. This I have indeavoured to do, in the best manner I could; and now beg leave to offer, what appears to me most probable, on a subject so intirely new.

Barkway lies not far from *Royston* in the same county, by which the *Roman* way called *Erming-street* passes to *Huntington*, as described by *Camden* (a). But as no *Roman* station has been discovered near either of those towns, it may be difficult to assign a reason, how these things should come to be lodged in the place, where they were found; tho several instances of the like nature have happened in diverse other parts of this kingdom. They consist of a small brass image, an oblong peice of brass, and 7 silver plates very thin; which have all suffered more or less by time, and other accidents. The figures and ornaments on the front of the plates are all in relief, and seem to have been made by a stamp impressed on the other side. Two of them have inscriptions in a compartment, writen with the point of a style, and the letters flatted behind. And one of these with 3 others of the remaining 5 have the figure of *Mars*, and the other two that of *Vulcan*, impressed upon them.

(a) *Britan.* p. 356. edit. 1607.

Fig. 24.



- a a. The Road to Castor.
- b. A Spring Well.
- c. The Remains of a Wall.
- d. The Porta Praetoria.
- e. Castor Church, built partly of Roman bricks.
- f. The base of an old Tower, lately discovered.
- gg. The Ruins of a Wall.
- hh. The Bottom of a Wall.
- i. The Ruins of an old Tower, over grown with Ivy.
- l. The Porta Decumana.
- mmmm. The Rampart.
- nnnn. The Ditch.
- oo. A Passage cut thro the Rampart, but whether Original Gates or not is uncertain.
- pp. The River Wentsum or Wentsoe.

Venta Icenorum.

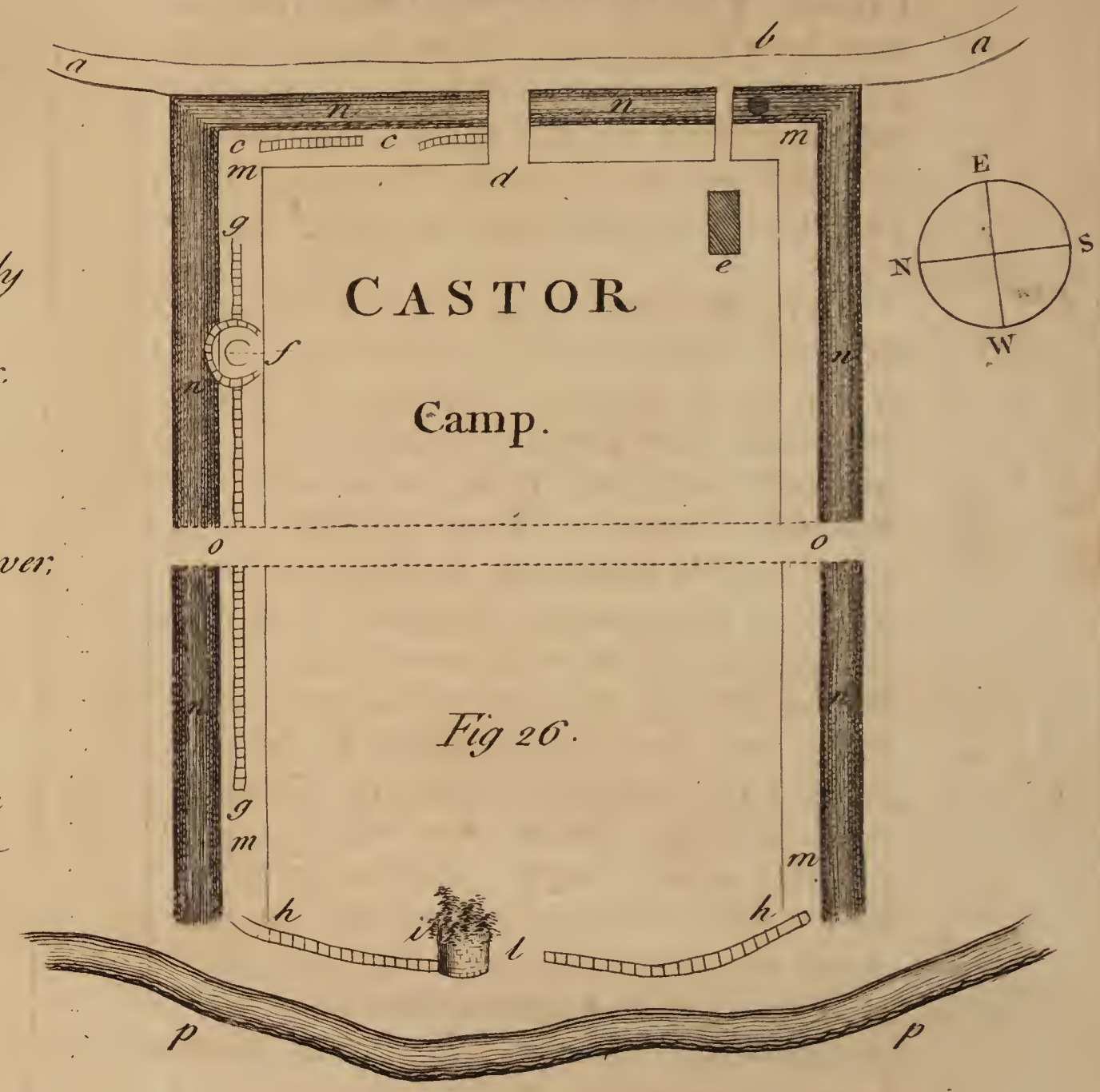
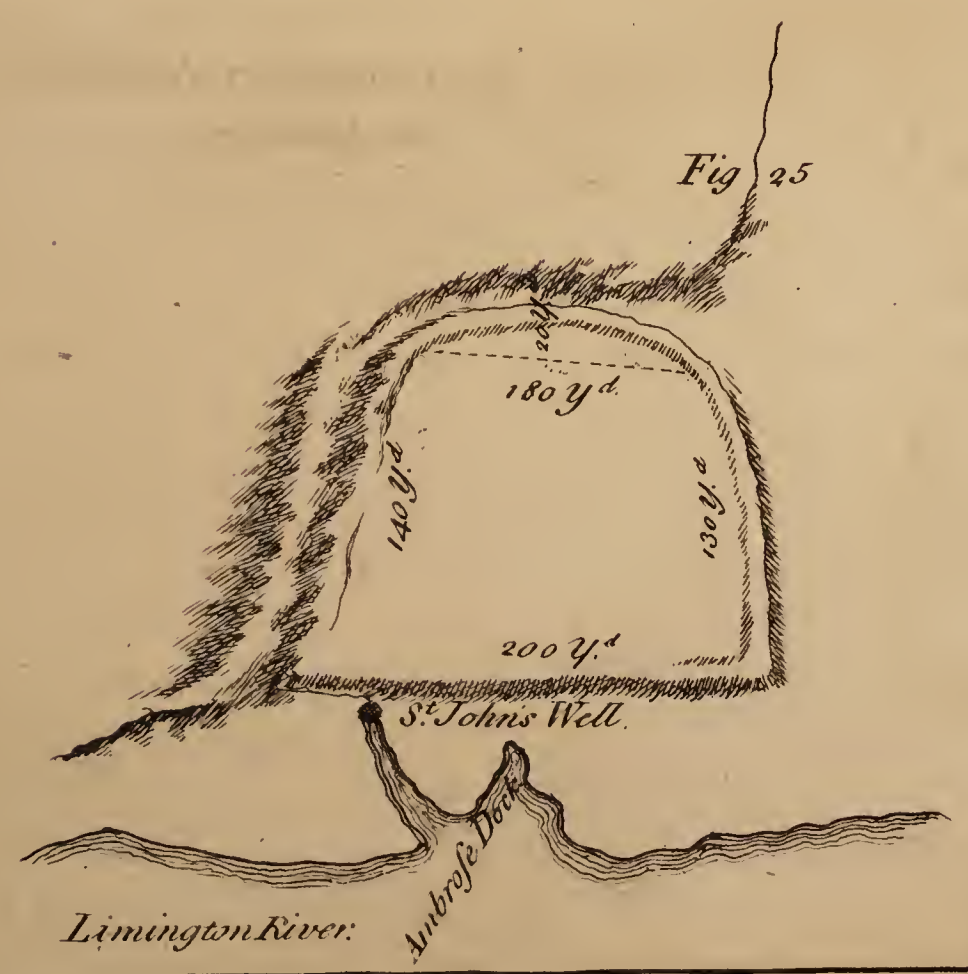
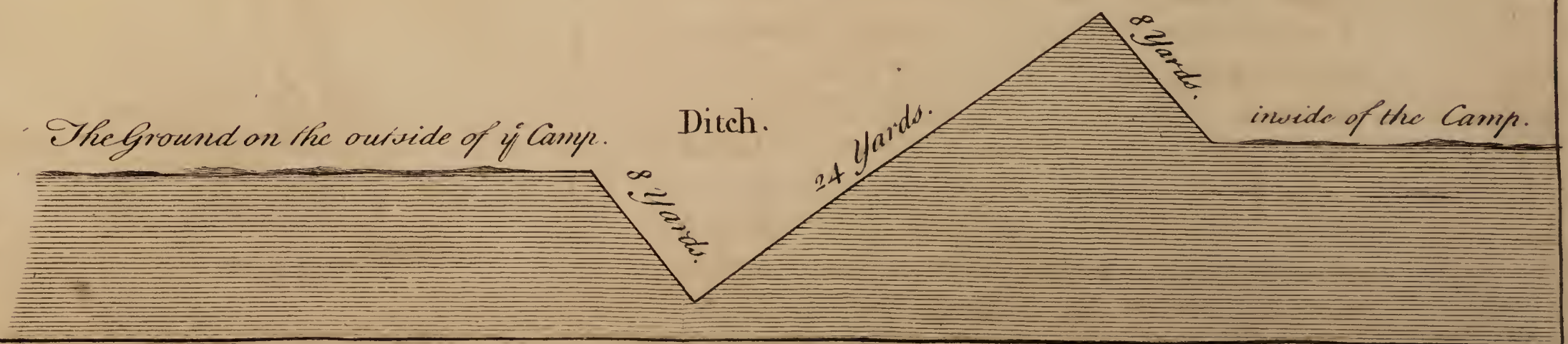


Fig 26.

A Section of the Rampart and Ditch, as they are to be seen at this Day.



The Ground on the outside of the Camp.



The following is a list of the names of the members of the American Medical Association who have been elected to the office of President of the Association for the year 1911.



The following is a list of the names of the members of the American Medical Association who have been elected to the office of President of the Association for the year 1911.

The image represents *Mars*, (see Fig. 27.) looking to his right side, Fig. 27. with an helmet on his head, and his body naked; his right arm extended, as if he had held a spear, and a thong round his left, like the remains of a shield now broken off with the hand; his right shoulder supporting a belt, which crossing his body descends on the left side; his right leg broken off at the knee, and his left foot lost. The height of what remains, from the end of the left leg to the top of the crest, is about 7 inches. It has been observed by *Montfaucon*, that tho *Mars* is frequently represented on coins, yet his statues are not very common*.

The other peice of brass (see Fig. 28.) is about $4\frac{1}{2}$ inches long, and Fig. 28. seems to have been the handle of a knife, or some such utensil.

The plates are for distinction sake numbered in the following order.

1. The first is broken into two parts, (see Fig. 29.) which put toge- Fig. 29. ther resemble the form of a leaf, and is near 21 inches high, and about 10 broad in the widest part. It contains an inscription inclosed in a compartment, addressed to *Mars Jovialis*, which will presently be explained.

2. The second (see Fig. 30.) is 8 inches in hight, and 4 in breadth, where Fig. 30. it is widest; and retains pretty much of the gilding, which none of the rest now do: tho all of them it seems, when first discovered, appeared to have been gilt, but in washing them the gilding came off. It has on it an image of *Mars* in a military habit, with an helmet on his head, a spear in his right hand, and his left resting on a shield; in the manner he is often seen upon coins. This image is placed in the front of a temple, between 2 pillars, with a *fastigium* or pediment over them. And beneath the temple in a compartment is an inscription to *Mars Alatorum*, which I shall endeavour to explain afterwards.

3. In the third plate, (see Fig. 31.) which is 3 inches high, and al- Fig. 31. most 2 wide, is an image of *Mars* placed between 2 pillars without a pediment, in much the same attitude as the former, with a *parazonium* over his shield.

4. The fourth plate (see Fig. 32.) which is $4\frac{3}{4}$ inches in hight, and Fig. 32. 1 inch $\frac{3}{4}$ in breadth, has the figure of *Mars* in a like attitude, inclosed only in a plain compartment.

5. The hight of the fifth (see Fig. 33.) is 8 inches, and the breadth Fig. 33. near $4\frac{1}{2}$; which has also a figure of *Mars*, much like those already mentioned, but turning to the left hand (whereas they all turn to the right) with a *chlamys* hanging down on his right side. It stands in the front of a temple, having two pillars on each side, called by *Vitruvius tetrastylus*†, and a double pediment over them.

6. The sixth plate (see Fig. 34.) is $6\frac{3}{4}$ inches in hight, and $3\frac{1}{4}$ in Fig. 34. breadth. It differs from all the former, as it represents the figure of *Vulcan*, having his usual attributes, a thick beard, high cap, short tunic, *femoralia*, and half boots; a *forceps* in his right hand, and a hammer

* *Supplem.* Vol. I. p 93.

† *Lib.* III. cap. 2.

in his left, with a *chlamys* thrown over his left arm. He looks to the right, and has before him a vessel like an altar, from which a flame ascends. He is placed in the front of a temple, between 2 pillars under a pediment, like *Mars*.

Fig. 35.

7. The last plate (*see Fig. 35.*) is $3 \frac{1}{2}$ inches high, and near 2 inches wide. It contains also an image of *Vulcan* in the front of a temple, with his several attributes, like the former, but differs from it in the other ornaments.

The design of both the inscriptions is to return thanks for some favour ascribed to the deity, to whom they are addressed. That on the first plate runs thus :

MARTI
IOVIALI
TI. CLAUDIVS. PRIMVS
ATTII. LIBER
V . S . L . M

That is

Marti Joviali Titus Claudius Primus, Attii libertus, votum solvit libens merito.

The word IOVIALI, in the second line, seems to be an epithet given to *Mars* in compliment to the Emperor *Diocletian*, who assumed the name of *Jovius* ; as his colleague *Maximian* did that of *Herculius*. Hence we meet with some military bodies in the *Notitia*, and elsewhere, called *Joviani* and *Herculiani* from those Emperors ; like the *Flaviani*, *Aeliani*, and the like, which were so denominated from the names of other preceding princes. There are also other epithets of the same form with that in the inscription, taken either from the names of Deities, or Emperors deified ; such were the *sacerdotes Augustales*, *Flaviales*, *Hadrianales*, and others, which often occur in *Gruter*. In like manner *Cicero* gives the title of *Ministri Martiales* to the priests of *Mars* (*a*) ; and calls the company of Merchants at *Rome* *Mercuriales* (*b*), as being under the protection of *Mercury*. And *Ganymedes* is stiled by *Macrobius*, *Jovialium poculorum minister* (*c*). Now as these several appellations took their rise from the peculiar relation and subserviency of the persons to those deities, from whom they were denominated ; so *Mars* himself, being here called *Jovialis*, is by an excess of flattery represented as subservient to this Emperor *Jovius* or *Jupiter*. For so he was also called, as we find in some like instances of fulsome compliments paid to him by the Panegyrist *Mamertinus* ; as when addressing to him, and his colleague *Maximian*, he says : *Sande Jupiter et Hercules bone* (*d*). And in

(*a*) *Pro Cluent* cap. 15.
lib. v. cap. 16.

(*b*) *Ad Q. Fr.* lib. II. Ep. 5.
(*d*) *Genethliac Maxim* cap. 16.

(*c*) *Saturnal.*

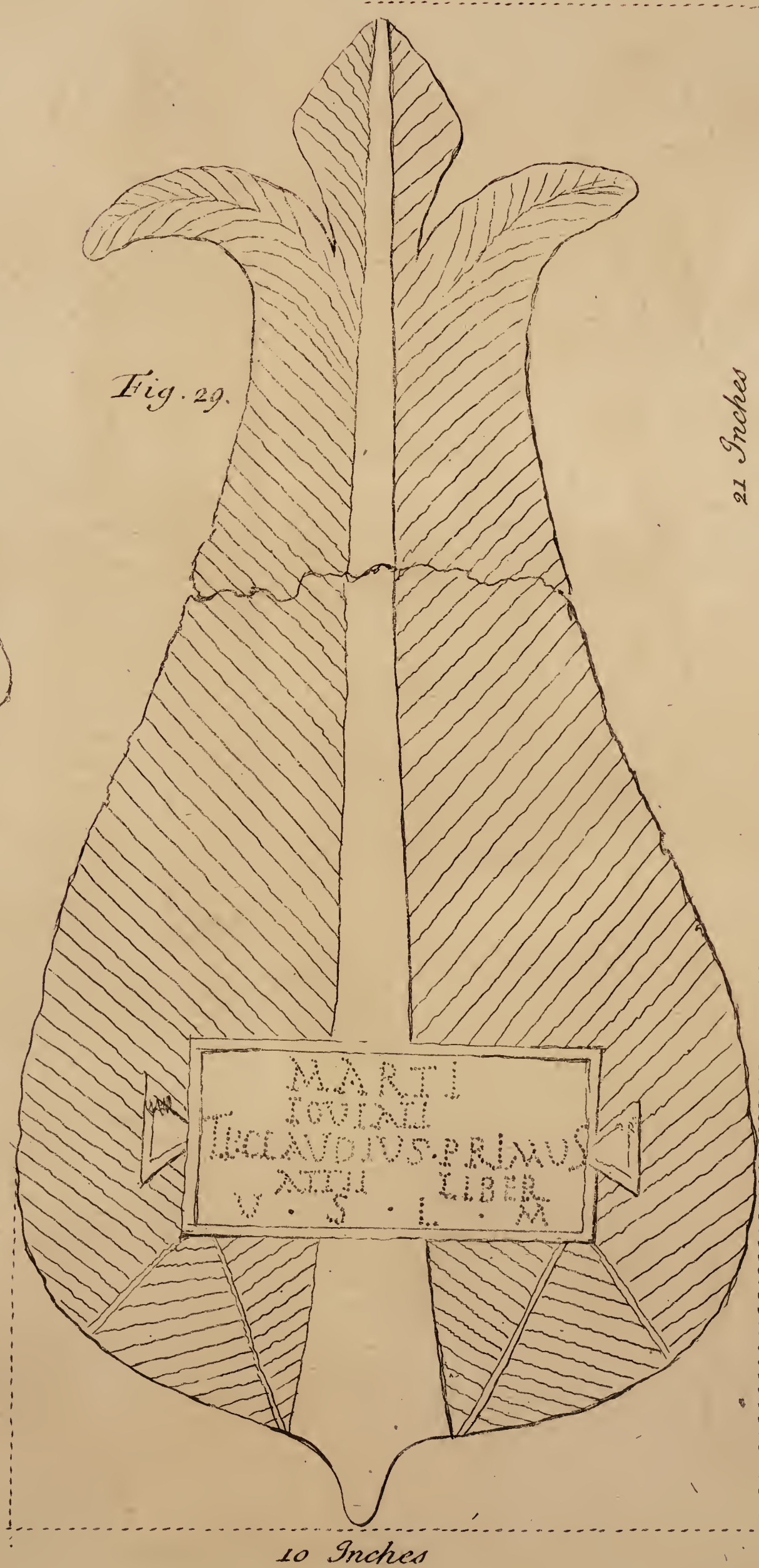
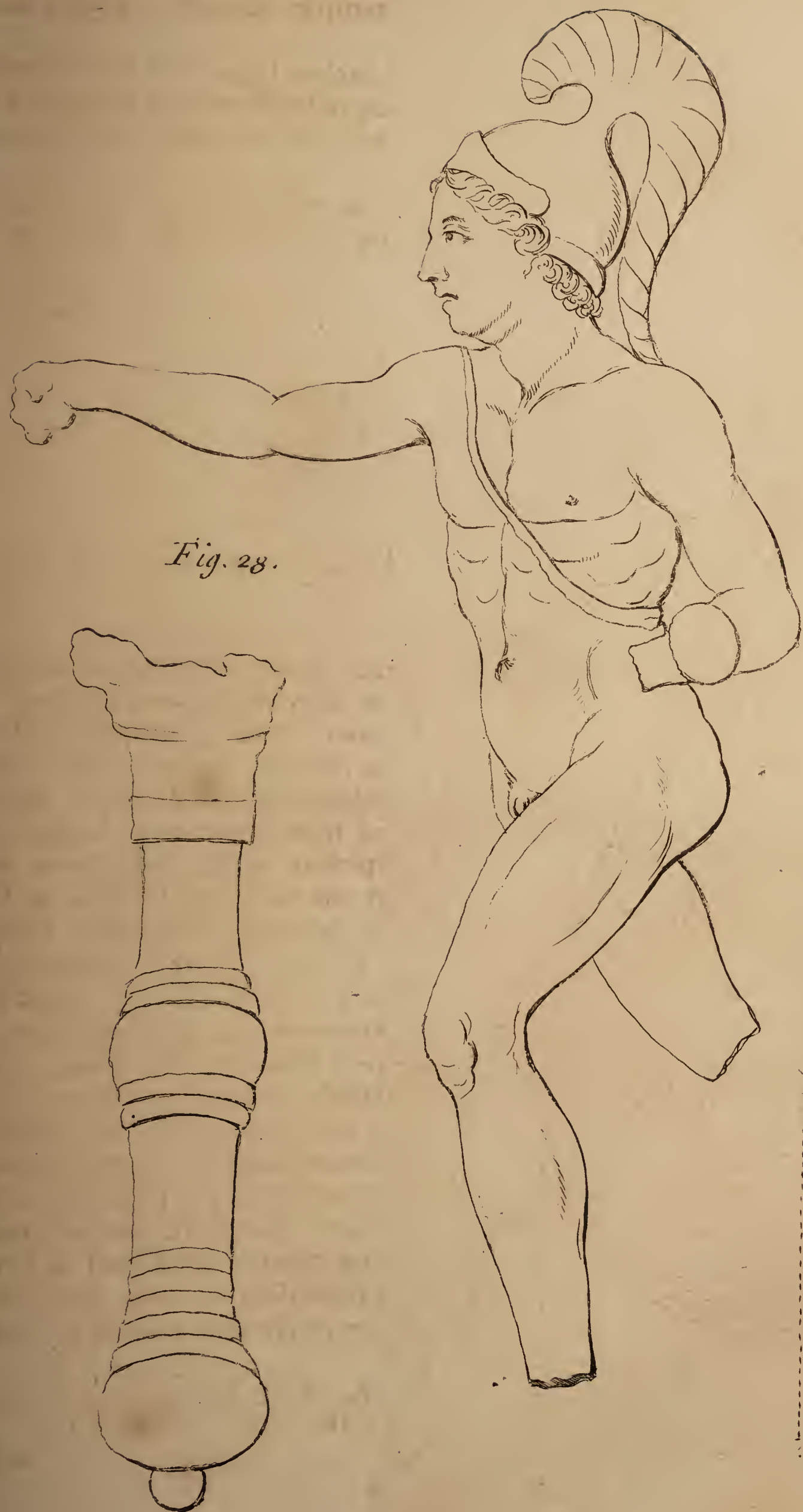


Fig. 31.



Fig. 33.



Fig. 35.



Fig. 34.

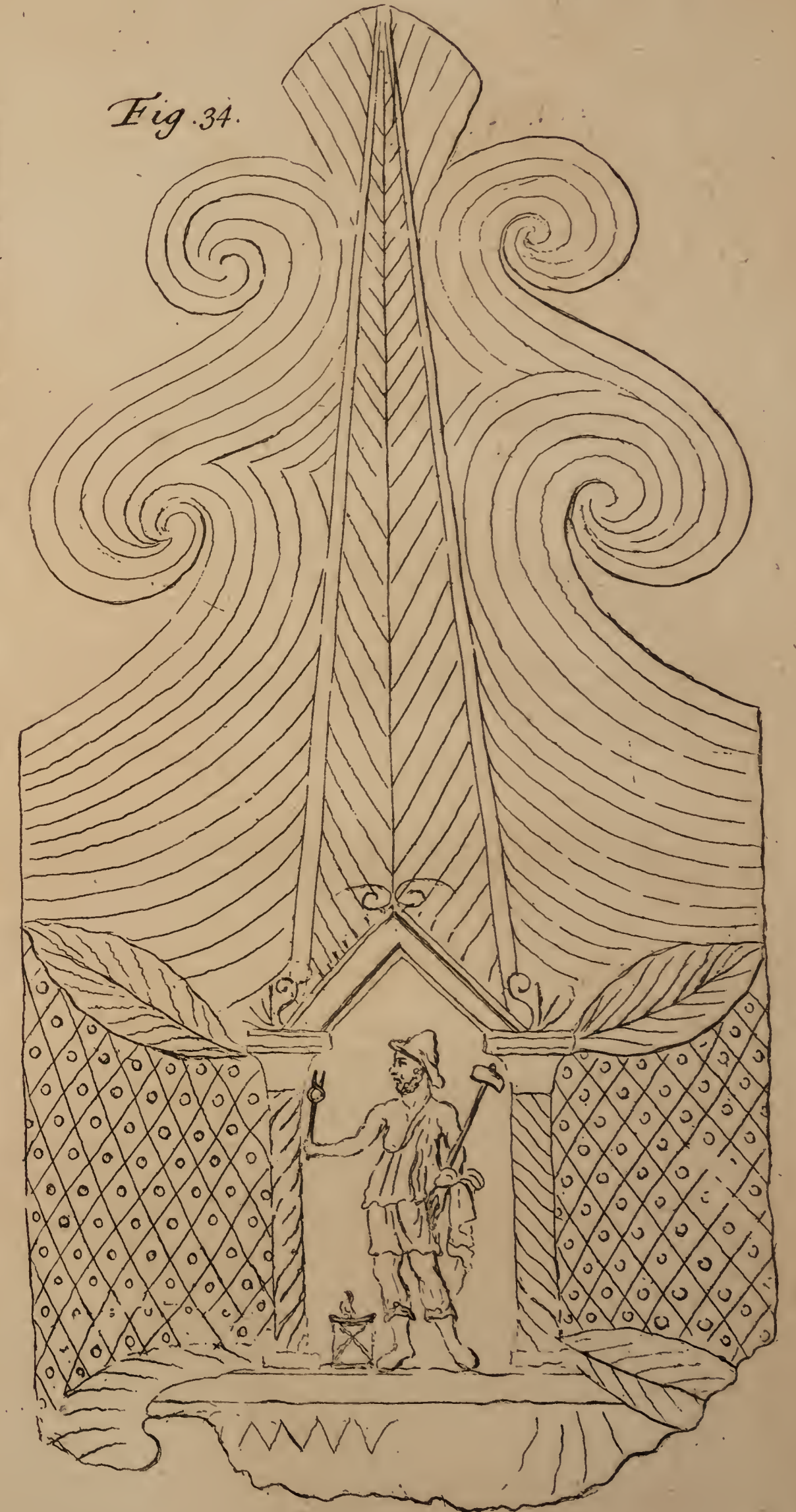


Fig. 32.



another passage: *Non opinione traditus, sed conspicuus et praesens, Jupiter cominus invocari; non advena, sed imperator, Hercules adorari* (e). And as if no degree of flattery could be too extravagant for this Emperor, there is an inscription in *Gruter*, which begins thus: AETER-NO IMPERATORI NOSTRO MAXIMO OPTIMOQUE PRINCIPI AVRELIO VALERIO DIOCLETIANO (f). The epithets OPTIMVS MAXIMVS, usually ascribed to *Jupiter*, had indeed been applied to some former Emperors; but AETERNVS, as a personal title, seems to have been first attributed to this Prince; tho', like other ill examples, it was soon imitated, and given to some following Emperors.

The third line contains the names of this votary, TITVS CLAVDIVS PRIMVS, each of which is separately found in *Horsley's Britannia Romana*; and in one of *Gruter's* inscriptions they all three meet in the same person, in the order they stand here (g). The next line tells us his character, that he was the Freedman of ATTIVS, that is, as I suppose, of TITVS CLAVDIVS ATTIVS; it being customary for Freedmen to assume the two first names of their patrons, as TIRO the Freedman of *Cicero* was called MARCVS TVLLIVS TIRO. Indeed ATTIVS generally stands as a family name, but we find it in the place of a *cognomen* in *Gruter*, MARCVS TVLLIVS M. L. ATTIVS (h). The last line contains the usual form of such addresses.

The inscription on the second plate is thus expressed:

D . MARTI . ALATORV
DVM . CENSORINVS
GEMELLI . FIL
V . S . L . M

That is, as I apprehend it may be read:

Deo Marti Alatorum Dum. Censorinus, Gemelli filius, votum solvit libens merito

The word ALATORV in the first line must, I think, stand for ALATORVM, the letter V being joined to the R in one character; as we find them in the *Britannia Romana*, where they make part of the word INSTITVERNT for INSTITVERVNT (i). How frequent and various such combinations were, especially under the lower empire, appears by the *table* of them published in that work (k). Some of which seem to have been the effect of fancy in the workmen, and others occasioned thro want of room, as in the present case. As to the meaning of the word ALATORVM, I suppose it to be an adjective, the substantive CASTRORVM being understood; and that the same place is

(e) *Ibid.* cap. 10.

(f) Pag. ccxxxix. 4.

(g) Pag. mcxxxi. 7.

(h) Pag. mxlii. 3.

(i) Northumb. xv.

(k) Pag. 189.

intended, which *Ptolemy* calls Πτερωτὸν στρατόπεδον (l), and modern Geographers generally take for *Edinburgh*. For as *Ptolemy* was himself a stranger to that country, his *Greek* name was probably an interpretation of the *Latin*, *Castra Alata*; which Mr *Horsley* thinks might be so called from the situation of the place somewhat resembling a wing (m). But as there is good reason to think, that this inscription was written long after the time of *Ptolemy* (as will be shewn afterwards) the word CASTRA might then have been dropt, and the common appellation of the place be only ALATA. There are other examples of the like kind, which may render this very probable. For we meet with a *Roman* station in the county of *Essex*, which both in *Antonine's Itinerary of Britain* (n) and *Peutinger's Tables* is called AD ANSAM, from the angular turn of the road there, as it is laid down in the *Table of Britain* (o). There was another in *Nottinghamshire*, which the *Itinerary* calls AD PONTEM (p), on the account of a bridge laid over the *Trent* at that place. And what appears more exactly parallel with the present case, in the same *Itinerary* we meet with MAGNA (q), which some have taken for *Old Radnor*; but Mr *Horsley* fixes it at *Kenchester* in *Herefordshire*, wherein he is followed by *Wesselingius* (r). Now in each of these instances CASTRA, or some equivalent word, must necessarily be understood; and probably at first the usual names were CASTRA AD ANSAM, CASTRA AD PONTEM, and CASTRA MAGNA, tho afterwards the word CASTRA was for brevity omitted; as in common speech we often find the names of places so shortened, that it is difficult to trace them back to their original. And this inscription might be addressed to DEO MARTI ALATORVM, as the topical deity of the place.

The second line, DVM. CENSORINVS, seems to contain two names of the person, who paid this vow to the deity here mentioned. Tho *Roman* citizens had usually 3 names, called *prænomen*, *nomen*, and *cognomen*; yet very often 2 only are expressed: and those either the first and second, as *Quintus Horatius* [Flaccus]; the first and third, as *Marcus* [Portius] *Cato*; or the second and third, as [Caius] *Cornelius Tacitus*. Besides, the order of these several names did not always continue the same; but what was at first a *prænomen*, became afterwards a *cognomen*: as PRIMVS in the former inscription. And the *cognomen* often became hereditary, and distinguished different branches of the same family; which I take to be the case here. One of *Horace's* odes in some editions is inscribed to C. *Marcus Censorinus* (s); but *Censorinus* often stands as a family name in *Gruter*, and others. So likewise in this inscription DVM. CENSORINVS is said to be FILIVS GEMELLI, that is GEMELLI CENSORINI. But as I do not find any

(l) *Ibid.* pag 359.(m) *Ibid* pag 364.(n) *Iter ix. Ibid.* pag. 381.(o) *Ibid.* pag. 505.(p) *Iter vi. Ibid.* pag. 381.(q) *Iter xii. Ibid.* pag.

457.

(r) *Vetera Rom. Itiner.* p. 485.(s) *Lib. iv. Carm.* 8.

other instance of a *Roman* name beginning with the syllable DVM. how that is to be read at length, I cannot say. Nor is there any thing particular in this; since there are several names in *Gruter*, which occur but once, and are no where else to be found. This inscription ends in the same manner, as the former.

I shall now proceed to inquire briefly into the design and use of these plates. The antient Pagans had not only their national but domestic deities, whom they addressed to in private, and sometimes carried their images about with them, as their guardians and protectors (*t*). And it appears to have been a custom among them to place their images in shrines, made in the form of temples, both for public and private devotion. The *Tabernacle of Moloch*, mentioned in the *Acts of the Apostles* (*u*), is generally taken to have been of the former sort. And *Herodotus* informs us, that the *Aegyptians* upon a solemn day carried in procession the image of a goddess, said to be the mother of *Mars*, in a wooden temple gilded over, which was drawn in a chariot (*x*). Such shrines are mentioned likewise by later writers. And others of a lesser size seem to have been made in imitation of them for private use. The *silver shrines of Diana*, mentioned also in the sacred History cited already (*y*), are by most interpreters said to be of this kind. And Mr *Kemp* had in his collection of antiquities one made of brass, but 5 inches high, with a goddess, supposed to be *Isis*, sitting in it; as it is described in the printed *catalogue* (*z*). Another of the same deity, but of a different form, and somewhat less, is now in the possession of *James West*, Esq; F. R. S. And sometimes they were placed in the monuments of deceased persons, an instance of which we find mentioned in an inscription published by *Reinesius*, which is there said to have been made of marble (*a*). The persons employed in making those sacred images were called by the *Greeks* ἀγαλματοποιοί, and by the *Romans* sigillarii, as we find in the antient *Glossaries* (*b*). And one of these artists is mentioned in two inscriptions of the *Britannia Romana*, where he is stiled SIGILLARIUS COLLEGII LIGNIFERORVM (*c*), who are more usually called by the *Greek* name DENDROPHORI; part of whose business might be to carry, or attend, the shrines in their processions at public festivals.

Whether or no these plates ever belonged to shrines, I cannot venture to assert; but I am rather inclined to think, they did not; except perhaps the first, which from the largeness of its size, and having no image stamped upon it, but only a written inscription in honour of *Mars*, might possibly have been laid over part of a wooden shrine, within which the brass image was placed, that was found with it. As for the rest, I imagine they were designed as partial representations of shrines

(*t*) *Ammian. Marcell. lib. xxii. c. 13.* (*u*) *Chap. vii. v. 43.* (*x*) *Lib. ii. cap. 63.* (*y*) *Acts xix. 24.* (*z*) *Monument. Kempian par. i. pag. 6.*
(*a*) *Class. xiii. num. 64.* (*b*) *In voce ἀγαλματοποιοί.* (*c*) *Pag. [354.]*

for the use of private persons ; having only the front of them with the image of the deity placed in it ; which being fixed upon wooden tablets might either be set up in their houses, or carried about with them, in devotion to those tutelar deities. And when any fortunate event happened, which they attributed to the success of their addresses made to them, they might sometimes express their acknowledgment of it by hanging them up in their temples, among other donations, making them a sort of votive tables. That they were intended for some such uses seems the more probable, from the number of them found together. They have a similitude with the reverse of many *Roman* coins, where the images of their deities are represented in the same manner ; from an imitation of which they might perhaps be introduced at first, as well for cheapness, as ready convenience, in some of the more remote provinces. And it is very remarkable, that no two impressions upon these plates are in all respects exactly alike ; as we do not often meet with two *Roman* coins struck from the same die.

As to the time, when the plates were made, the inscription upon the first fixes it to the reign of *Diocletian* ; and as not only the characters of the other inscription exactly correspond with that, but also the manner of the work upon each plate is the same ; it is highly probable, they were all made about the same time, which was near the end of the third century. And to this likewise the form of the letters, particularly A and M, very well agrees. Nor ought it to seem strange, if more of them have not been preserved ; since from the nature of them they appear so liable to be destroyed, either by the injuries of time, or for the sake of the silver.

Concerning
the ancient
Bridewell at
Norwich ;
by Mr Will.
Arderon of
Norwich ;
communicated
in a letter
from Mr H.
Baker. F.R.S.
to M. Folkes,
Esq; P. R. S.
N^o. 477. P.
520. Aug.
1745. Read
Nov. 7. 1745.

XV. This city of *Norwich* affords a remarkable instance of an art we have now lost ; I mean the wonderful art, which our ancestors knew, of cutting or rather breaking flint stones into uniform figures, of equal sizes, and with smooth and plain surfaces. Many remains of this sort are to be seen in our old buildings ; but none is more artificially and regularly finished than the N. wall of our *Bridewell*, which in length is 114 feet, and in height 30.

This ancient structure, as all ingenious strangers agree, is one of the greatest curiosities of it's kind, either in our city or county, and is not perhaps to be outdone in any part of the known world *. It was built by *William Appleyard*, the first Mayor of *Norwich*, who in 1403, held his mayoralty there : and, what is very strange, this flint-work appears now as perfect as if it had been finished but yesterday ; whereas the bricks, which were wrought-in near the bottom of the wall, as a ground-

* The gate of the *Austin Friars* at *Canterbury*, that of *St John's* abbey at *Colchester*, and the gate near *Whitehall*, *Westminster*, are in the same taste. But the platform on the top of the *Royal Observatory* at *Paris*, which, instead of being leaded, is paved with flint after this manner, is an instance that the *French* have, in some measure, recovered this art. C M.

work, are almost rotted away. The windows and mouldings, which were built at the same time, of freestone, are nearly in the same condition. But these flints have hitherto defied the devouring teeth of time, and will, probably, continue untouched for many ages; being, perhaps, the most durable way of building that ever was yet invented.

These beautiful flint-stones are squared to such a nicety, that the thin edge of a knife cannot be insinuated between the joints without a great deal of difficulty; and it is no easy task to make out that they were laid with lime. Most of them are about 3 inches square, and as smooth and level as if they had been ground. They are also laid with such great exactness, that no brick-work, or hewn-stone, appears more regular in it's courses.

XVI. 1. Some of the most remarkable curiosities, we saw, were,

1. A picture of about 5 feet long, and 4 feet wide, representing the education of *Achilles*, by his master *Chiron* the centaur. The figures are about half as big as the life. That of *Achilles* is standing in a noble action, and is seen in front, as the principal object of the picture. He seems to hearken with great attention to, and is looking steadily on the centaur, who is seen almost in a side view. The figures are both finely coloured, and well drawn; but that of the young man most exquisitely so.

Extract of a letter dated at Rome, Aug. 5. 1747, from Mr Hoare, a young statuary, now pursuing his studies there; to his brother Mr

2. Next to this is a picture of about 3 $\frac{1}{2}$ feet high, and narrow, in which is a woman sacrificing. The figure is about 2 feet high. This picture seems to have been taken out of some compartment of ornaments.

Hoare, an eminent Painter at Bath, giving a short account

3. Next to this is a broken piece, representing the judgment of *Paris*. The figures are about the same size as that last above-mentioned. They are not intire; the bottom part being broken off about the knees. This is also a very fine picture; but it is impossible to judge of all it's beauties, as it is extremely changed and decayed; which is quite contrary to all the others, but, in particular, to that of *Chiron* and *Achilles*; which is in a manner as fresh as if it had been painted but yesterday.

of some of the principal antique pictures found in the ruins of Herculaneum at Portici, near Naples; communicated by the Rev. Mr Birch. N^o.

4. The next is a fine picture of the story of *Virginia*. The figures are something bigger than those in the abovementioned. The characters and expressions of the heads are admirable. That of *Appius* gives a just idea of the furious transports in which the artist designed to describe him. *Virginia* is weeping; and, in a word, all the figures are finely disposed, and the characters well adapted to the subject.

484. p. 567. Oct. &c. 1747. Read Oct. 2^d 1747.

5. Two large pictures were in a nich in a *Basilica*, about 5 or 6 feet high. The first represents *Theseus* victorious over the *minotaur*. He is standing in a free and fine posture: one foot is on the head of the *minotaur*. But what seemed odd to me, was the figure of that monster itself, which I had always seen differently represented; for, in this picture, the head only represents that of a bull, which is joined to the body of a man. Several little *genii*, or *cupids* (as we call them), all seem im-

patient to to shew their respect to their deliverer : one kisses his hand, another clasps round his leg, and several others are in different attitudes of gratitude. The figures are almost as large as small life.

6. The other picture represents *Hercules* and the goddess of Nature. The figure of *Hercules* is standing (seen in a side view) reposing on his club ; something like the statue in the *Farneſe* palace at *Rome*. There is a victory crowning the hero, and the goddess is sitting before him, and seems to applaud and thank him for his labours. There are numbers of symbolical figures besides in this picture. Behind the goddess is a satyr, and at *Hercules*'s feet a boy sucking a doe. My friend, who conducted me, took particular notice, how delicately the doe seemed to dispose of her legs, not to hurt the child ; whilst at the same time she is licking his knees, as a mark of her tenderness for him. This picture is equal to the first-mentioned ; being exquisitely finely drawn and coloured, and well composed.

7. There is a little picture, which I thought extremely odd for it's composition. It is about $1 \frac{1}{2}$ foot long, and 8 or 9 inches high : it is a parrot drawing a chariot something like our modern chaises. In the chariot sits a sort of large horse-fly, whose two horns serve for the bridle and reins to guide the parrot.

8. Two pictures, of about $4 \frac{1}{2}$ feet long, represent the stage of a theatre, with comedians playing their parts upon it. The perspective in these pictures is very well observed.

9. A wedding, consisting of 3 figures only. They are much in the same taste of those of *Aldobrandini*'s marriage at *Rome*. There are besides numbers of little frizes representing sacrifices, and other ceremonies, of the ancient Pagans ; most of them on black or red grounds.

These pictures shew, that the Antients understood perspective and landschape, I mean, the keeping particularly, which I have heard strongly disputed ; but no one that has seen these pictures will, I believe, make any doubt of it.

It would be impossible for me to give you an exact description of all the pictures ; as there are so many entire, besides the bits, and fragments of others. Of some, the heads only remain ; and of others, pieces of figures ; numbers of small landschapes ; views of architecture ; flowers and fruit, painted extremely light and elegantly. There are even some grotesque pictures, something in the taste of *India* painting. Most of the small ones have been taken out of compartments : The guardian shewed me several places from whence they had been taken. They still preserve a beauty superior to any thing we see now-adays : the colouring, drawing, and liberty of pencil, may vie with the works of any master, even of *Raphael* himself.

There are two rooms full of them. — Perhaps you will say I have given you an account but of very few. — In the first place I must inform you, that no one is admitted without an order from the King's Superintendant ; and that, when one does see them, the guardian has

orders not to permit any person to take any sketch or account whatever of the pictures. My friend went thither but once, and that after dinner, when we came down from mount *Vesuvius*. The account I have herein given is all I could particularly remember in the evening when we came home. They are continually finding more pictures every day; and I do assure you, that had I a month to spare, I would willingly go on foot to *Naples*, to have the pleasure of studying those I have already seen, and seeing those which have been discovered since *.

N. B. Cardinal *Albani*, at *Rome*, has an antique group of *Theseus* and the *minotaur*; where the *minotaur* has the head only of a bull, as in the picture above-mentioned.

2. The paintings found under-ground in *Herculaneum* near *Portici*, are all done on *stucco* in water-colours in *fresco*. They have been taken from the walls of an amphitheatre, a temple, and houses, and are in great variety, some exceeding fine, and well preserved. I divide them into 2 classes; the first of which contains the 4 following pictures.

The first is a large piece of 7 feet by 5, representing *Theseus*, after having killed the *minotaur*. He is naked at full length, holding a club or knotted stick in his left hand by the small end: a young woman by his side, holding the said club a little higher with her right hand, and looking up wishfully at him: 3 children of different ages; one kissing his right arm, which is extended; the second his left leg, which is a little raised; and the third grasping and kissing his left arm; all as it were wishing him joy, and caressing him after the victory; the *minotaur* lying on his back dead at his feet, a human body with a bull's head and short horns. This piece has been a great deal larger. On the upper part is part of a naked arm with a trumpet.

The second is a noble piece of 10 feet by 7, intire, and seems to represent *Rome* triumphant; viz. a grand figure of a woman sitting, with a garland of flowers on her head, a majestic commanding countenance, a knotted club, exactly like that of *Theseus*, long and tapering, in her left hand, resting herself on her right elbow, with her hand to her temple: a young fawn laughing over her shoulder, with a musical instrument of twelve pipes in his hand. At her side is a basket of fruit: over-against her a naked figure of a man, robust and vigorous, with a beard; his back short, and, to sight, his face turned to the left shoulder; a garland of flowers or laurels on his head; a quiver, a bow and arrows by his side; under his left arm something like part of a lion's skin, and one paw, but faintly expressed: a fine natural attitude; most exquisite proportion and drawing. A little higher, close by him, a genius or goddess of Fame, with wings, a garland on her head, a sprig like ears of corn in the left hand, and pointing with the right; and both she and the man looking to a young infant below (a most beautiful figure, and natural attitude) sucking a doe, finely drawn and spotted, which is lick-

Remarks on
the principal
paintings
found in the
subterraneous
city of Herculaneum, and
at present in
the possession
of the King of
Naples; by
Blondeau, Esq;
communicated
by Tho. Stack,
M.D. F. R. S.
N^o. 491. p.
14. Jan. &c.
1749. Read
Jan. 26.
1748 9.

* See more of these curiosities in Vol. IX. Part iv. Chap. i. Art. xv. and xvi.

ing the child's knee. Under their feet an eagle with his claw upon a globe, and a lion, both as large as life. Some reckon the man *Hercules*, and the woman *Pomona*: but *Hercules*, I think, did not use the quiver; and *Pomona* has no such majesty, nor any business with a club, which is longer and smaller than that of *Hercules*.

The third is a piece of 4 feet square, representing the centaur *Chiron*, sitting, as it were, on his backside, and teaching his pupil *Achilles*, a young lad of about 12, to play upon the harp. Part of the horse is a very difficult forced attitude; the whole body being in view; left fore foot extended; great expression and attention both in *Achilles* and *Chiron*, who is putting his right hand round the boy, and playing, by the help of a small instrument, on the strings, which are ten in number. This is accounted a most masterly piece. *Chiron* has a mantle tied round his neck, made of the skin of some animal; and *Achilles* stands up-right naked.

The fourth is a piece of 5 feet by 4, representing some very solemn and melancholy story of the *Romans*, and contains seven figures, three men and four women. Perhaps the story of *Virginia*, when *Appius Claudius* wanted to accuse her falsely, in order to gratify his lust. One man sitting in a pensive mood, his left elbow on his knee, and his hand up to his forehead: another sitting over-against him, setting forth something in a paper, which he holds to the breast of the first: a young woman sitting on the right side of the first, a figure expressing great concern; her left hand affectionately about his shoulder: and another young woman standing with great attention and surprize by her: behind both, the figure of a woman larger than the rest, with a quiver appearing above her shoulder, as *Diana*: an elderly woman in a suppliant bending posture, with her finger at her chin, as if she were listening with great grief, and her face to the first figure. Also an old man, in much the same attitude, in great grief, as if weeping. Perhaps the family of *Virginia* listening to the accusation against her, and fearful lest she should be delivered over to the brutal lust of the consul: to avoid which, when no other remedy was left, *Virginius* desired to speak with his daughter in private, and killed her.

These are the four capital pieces; and they are so extremely well executed, that Don *Francesco de la Vega*, a Painter, whom the King of *Naples* sent for from *Rome*, as one of the best hands, to take draughts of these paintings, told me, that if *Raphael* were now alive, he would be glad to study the drawings, and perhaps take lessons from them. Nothing can be more just and correct: the muscles are most exactly and softly marked, every one in it's own place, without any of that preternatural swelling, which is so much over-done in some of the best *Italian* masters, that all their men are made to appear like *Hercules*. It is surprising how fresh all the colours of these pictures are, considering that they have been under-ground above 1650 years; besides the years they

they stood, before they were covered by the eruption, which cannot be exactly determined.

Theseus in the first, and the naked figures in the second piece, are a good deal upon the red colour; but the women and children are of as soft and mellow flesh colours as if painted in oil. The third and fourth are so highly finished, that you can scarcely discern whether they are done in water or oil-colours. The last pleased me most; the composition is good; the attitudes natural, and of fine kinds; the different characters justly expressed; the drawing and drapery exquisite; and, tho' done in water, with only 2 or 3 colours at most; yet the light and shade are so artfully managed, that the figures are quite out of the surface. The connoisseurs prefer the third, or the centaur.

We now come to those of the second class, which are as follow.

1. A piece of 4 feet by 3, supposed to be the judgment of *Paris*. Three goddesses, with rays like circles of glory about their heads, which are very fine: the first sitting inclined; two standing naked; good drawing, and natural attitudes. A figure of a shepherd at a distance above them, with a crooked staff in his hand, a garland on his head, his right hand grasping something, which is not distinctly seen, as not being so much finished as the rest.

2. A piece of 4 feet square, representing *Hercules*, when a child, tearing the serpent in pieces with great vigour and fierceness in his eyes: an old man drawing a dagger, being startled at the danger, in order to kill the snake: a woman designed holding up her hands to Heaven: an old woman holding a child in her arms. The whole natural and well-drawn.

3. A piece of 4 feet by 3: an old man naked, sitting: a naked boy standing by his side, with a piece of a rod or twig in each hand: the old man is pointing with his finger, and teaching the boy something. Fine drawing, somewhat defaced.

4. A piece of 6 feet by 3: a half length of *Jove* with thunder in his hand: a little *Cupid* looking over his shoulder: a rainbow: an eagle: a bold old head: a figure like *Venus* coming from bathing, naked down to the thighs. Beautiful contour, great softness, and fine flesh colours; seems to have the privy parts of a man, an hermaphrodite.

5. A small piece, about 14 inches square: two fine female heads, or half-lengths; one with a book in her hand; great expression! Two Muses.

6. A piece of about 18 inches square; two figures of women like *Graces*; one naked to the middle, sitting; something like a quiver at her feet; another in a robe, standing, and leaning on her elbow: good attitude; drawing and drapery very fine; colours faint.

7. and 8. Two pieces, of 3 feet square, of *Egyptian* sacrifices. First, The worshipping of an idol, which is placed above in the portico of a temple, and appears bloody: 7 figures bending and suppliant in the act of adoration: an altar in the middle: two birds, storks, standing one on

on each side: many other figures faint.—Second, a priest sacrificing upon a flaming altar: a row of different figures on each side: two in the middle in the act of preaching. Attitudes very just and natural, finely done, great solemnity or horror: when looked at near, seems more daubing and unfinished: by *virtuosi* esteemed a great piece of antiquity, and of great study.

9. Is a half-length of a man like a priest, with a small water-pot, pouring it into a basin, seen by the light of a lamp.

10. Is *Orpheus* and *Venus* lying together, kissing and caressing, chained by the legs: a servant holding a harp. Finely designed, but defaced.

11. An old man sitting, with a cup in one hand, a stick and garland in the other.

12. Is a half-length of a young woman.

13. Is a piece of $2\frac{1}{2}$ by 2 feet: old *Silenus* holding in his arms *Bacchus* a child: a satyr: a *Baccante*: *Mercury* sitting below: a tyger and ass lying. Finely drawn, and naturally expressed.

14. A sleeping nymph; a satyr lifting up her robe: three by-standers, who seem to be very curious. A small piece.

15 and 16. Two small pieces of satyrs ravishing nymphs: well drawn, and natural attitudes, but faint and defaced.

17. A piece of $4\frac{1}{2}$ feet by $1\frac{1}{2}$ foot: a figure of a *Roman* lady, almost full length, in attitude of great grief; her head a little inclined; her arms dropped down, and her fingers clasped; a sword, with the handle leaning in the hollow of her hand. Very just and natural expression, well finished.

18. The goddess *Flora* as descending from Heaven. Fine contours: about 2 feet square.

19. Is a piece 3 feet square: a naked figure with a lance like a General: a woman sitting: a young man holding his horse: an old woman. Finely done, but defaced.

20. *Orpheus* with his harp, sitting on a rock by the sea side: a child or sea-god riding on a dolphin, presenting him with a book.

21. Ten small pieces of *Roman* ceremonies with many figures; some eating, dancing, making love; others tied like prisoners.

22. Eight small *cupids* in different attitudes, and different paces. Very good.

23. A pheasant and other birds: two small baskets, one tumbled down: a rabbit eating. Exquisitely done.

24. Two naked figures, with *Cupid* betwixt.

25. A figure in the attitude of a warrior, with a sword in his right hand, a buckler in his left, and a cup with some jewels at his feet.

26. A large piece of architecture, which, looked at near, seems rough and daubing, at a distance very good perspective. You see quite thro' two portico's, one above another, into a palace or church. Very curious architecture, colours very lively and fresh.

27. A land-

27. A landfkape with houfes, ruins, a theatre. Good architecture : figures of pheafants, mules loaded, &c.

28. Another piece of architecture and perspective, very good.

A great many other figures of men and women, not eafy to be defcribed, becaufe pretty much defaced : alfo many fancies of birds, beafts, chariots drawn by different animals, children driving : all in fmall.

Little pieces of landfkapes, and other ornaments for the walls of their houfes, which were painted moftly of a yellowifh colour ; divided into fquares or panels ; with thofe peices of painting in the panel, and a border round it. There is a very good peice of ornament or cornice, that was upon the picture of *Theseus*, of a very good tafte, and finely finished *.

XVII. As *York* was undoubtedly the *Roman*, imperial city of *Britain* : fo is it ftill, cafually throwing up remains of it's antient grandeur and magnificence : even down to our time. About 2 years ago, in digging the foundation of a large houfe, fince built, in our *Trans Tyberim Street*, called *Micklegate*, *quafi Muckle*, or *Great Street*, the workmen went much below any former foundation that could be obferved on this fpot. And at the depth of 10 feet, came to a ftone, which upon taking up, appeared to have figures upon it, but miferably defaced. Upon my viewing of it foon after, I confefs, I was at a lofs what to make of it ; but judged it fome representation of an heathen facrifice, or game, and therefore fent as juft a drawing of it, as could be taken, to *Dr Stukely*, who according to his deep knowledge in the learning of the Antients, foon after returned me the following fhort, but curious explanation of this uncommon piece of fculpture.

*Account of a
bas-relief of
Mithras found
at York, ex-
plained by the
Rev. Dr Stuke-
ly, F. R. S.
communicated
to the Royal
Society, by Mr
Francis Drake
of York, An-
tiquary and
F. R. S. N^o.
493. p. 214.
O³. &c. 1749.
Read Nov. 23.
1749.*

“ The drawing (*Fig. 36.*) you fent me, of the bas-relief, dug up in
“ a cellar in *Micklegate*, anno 1747. is a great curiofity. 'Tis a fculp-
“ ture of *Mithras*, as ufual, facrificing a bull. He has on, the *Persian*
“ mantle, called *candys*, and the *Phrygian* bonnet, called *tyara*. He
“ represents the *archimagus*, performing the great annual facrifice, at
“ the fpring equinox : according to the patriarchal ufage. Thefe cere-
“ monies to *Mithras*, were generally celebrated in a cave of a rock ;
“ therefore, this fculpture was found fo deep in the earth.”

There is, commonly a figure on each fide of him, habited in the fame manner, ftanding crofs-legged : the one holds a torch up, the other down : here is only the latter, in your fculpture ; the other is imperfect.

Underneath, is the figure of an horfe, intimating thereby, the fun's courfe : for in time, when the old patriarchal cuftoms became profaned and defecrated into idolatry ; they made *Mithras* to be the *Apollo*, or the fun. Whence thefe fculptures had a number of fym-

* See accounts of this fubterraneous city in Vol. IX. *ut fupra*.

‘ bols, relating to the solar circuit of the year, through the 12 zodiacal
‘ constellations.

‘ The two figures attending on the *archimagus*, are inferior officers
‘ to him. There is a mystery in their standing cross-legged, like our
‘ effigies of croisaders in churches, and it means the same thing : for the
‘ cross was one part of the *Mithriac* ceremonies. These two, by the
‘ different attitude of their torches, represent day, and night, as *Mithras*
‘ represents the sun. The figure imperfectly drawn, at the tail of the
‘ horse, is I believe, a genius, twisted round with a snake ; which means
‘ the vitality, imparted to all things, by the solar power, and circle.

‘ The other figures are too imperfect, to trouble you with conjectures
‘ about them ; but they all regard the same design. They are officiating
‘ priests, and drest in such a symbolic manner, as intimates the sun’s in-
‘ fluence, and annual motion.

‘ The *Mithriac* ceremonies, as likewise the mysteries of the Antients,
‘ were but the expiring remains of the antient, patriarchal religion ;
‘ and worship of the true God : as it were, swallowed up by Paganism,
‘ and the devil’s inventions. For indeed the patriarchal religion was no
‘ other than Christianity antedated.

‘ For in the *Mithriac* ceremonies, and mysteries, they had plainly,
‘ the two Christian Sacraments : initiation by baptism : and the sublime
‘ communion by bread, and wine. *Mithras* is but another name of a
‘ *Messiah*, in his priestly character : it signifies mediator.

‘ Thus writes *Tertullian*, c. 40. speaking of the devil perverting the
‘ rites of true religion : “ (He), the devil persuades those that are ini-
‘ tiated into the *Mithriac* ceremonies, to believe an expiation of their
‘ sins by their being baptized : and they are crossed on the forehead, as
‘ his faithful foldiers. And likewise they there celebrate the breaking
‘ of bread.” But they did not take these ceremonies from the Chri-
‘ stians ; they are of a much antienter date ; perhaps from the begin-
‘ ning of the world.

‘ The *Romans* became extremely fond of the *Mithriac* sacreds, whence
‘ here you find this sculpture in the imperial city. I saw an image of
‘ *Mithras* at *Chester*, and no doubt there are many more in *Britain*,
‘ either destroyed, or undiscovered.

“ St *Jerom*, in his epistle to *Læta*, writes, A few years ago, your
“ cozen *Gracchus*, a name of *Patrician* quality, when he was præfect
“ of the city, destroyed, broke, and burnt the cave of *Mithras*.” This
was at *Rome*, and about the year 378. Not long after, we may well
imagine, your *Roman* præfect of *York* followed his example, and de-
molished the subterranean temple in *Micklegate* ; where this sculpture
of him was found.

*An account
of an antient
throne, for-
merly belong-*

XVIII. The shrine before us is a great curiosity. Few of this kind
of antiquities escaped the general ravage of the dissolution of abbeys :
for which reason I thought it would be an agreeable amusement to the
Society

Pla. XII. Vol. X. Part IV. pag. 1312.

Fig. 36.



Indica. 27 1/2.

W. Stukeley delin.

Inches 2.3 $\frac{1}{2}$.

J. Mynde sc.

Society to have a view of it; and to preserve a drawing of it as in Fig. 37, and 38. The shrine is made of oak, plated over with copper, upon which the figures are chased in gold: the ground is enameled with blue; in the ridge along the top are 3 oval crytals set transparently; it's dimensions are as expressed under the print.

Mr Eayre of St Neots sent it to me to have my opinion of it. It was found in the house of a Gentleman of that neighbourhood, who never shewed it during his life-time; and who possibly might have given us some account, as to the history of it; and at present we have no means left of finding it out, but by conjecture.

I conceive it came from Croyland-Abbey. There was an intercourse between this abbey and St Neot's priory; insomuch that St Neot's body was carried hence to Croyland-abbey, and inshrined there.

These shrines were made for receiving reliques of saints, in old abbies, churches, and cathedrals. (See a print in the history of Canterbury, and in Dugdale's Monasticon, of the high altar of the church of St Augustin there; no less than 13 of these shrines standing around.) These were carried about in processions on their anniversary days; sometimes embellished with jewels of inestimable value. Besides these portable ones, there were others, built of stone, marble, and other materials; like that of St Edward the Confessor in Westminster-Abbey; one now in Chester cathedral of St Werburga, whereon the episcopal throne is set, adorned with sculptures of Saxon Kings and Saints: one of St Thomas de Cantelupe Bishop of Hereford, in that cathedral. These now remain. There was one in the church of Burton-Coggles, Lincolnshire; and of Heckington in the same county; and innumerable others, destroyed at the dissolution of monasteries.

The shrine before us, from the manner of drawing, and workmanship, I conclude to be of Saxon antiquity, and that very high; now near 900 years old. I think it gives us the story of the murder of the Abbot there, and his monks, perpetrated by the barbarous Danes, in the year 870.

Sept. 25. that year, they rushed into the church of Croyland, whilst the religious were at divine service. Ingulphus, Abbot of the place, in his history, gives us this account. Lord Theodore was then Abbot of Croyland; who at that time pontifically officiated at the high altar, expecting the Barbarians. King Osketyl cut off his head upon the altar. Verus Martyr et Christi hostia immolatur, says our author, Ministri circumstantes omnes capitibus detruncati: "Thus fell the true Martyr and Lamb of Christ, as a sacrifice on the altar. All the assistant ministers were beheaded likewise," says he.

The two on our shrine are Frier Elfget the Deacon, and Frier Savin the Subdeacon.

Some days after, when the Monks that fled returned, they found the body of the venerable Abbot Theodore beheaded at the altar.

ing to the ab-
bey of Croy-
land; by Wm.
Stukely, M.D.
Coll. Med.
Lond. Soc. &
Eccles. D.
Georgii Mar-
tyr. Lond.
Reſtor. N^o.
190. p. 579.
Dec 1748.
Read Dec 8.
1748.
Fig. 37, 38.

Above is represented his successor Abbot Godric, with the ministers about him, putting the deceased Abbot into his shroud; whilst angels are carrying his soul up to Heaven.

I suppose some part of this martyr might be obtained, and kept in this shrine.

I observe the famous old sepulchral stone in *Peterborough* minster-yard, is exactly of the same shape as our shrine. It was set up over the grave of the Abbot and Monks murdered by the same *Danes*, the day after those of *Croyland-Abbey* suffered, *Sept. 26*. It is carved on the sides with the images of our Saviour and the Apostles. It is now removed into the library.

An abstract of a discourse intitled, Reflexions on the medals of Pescennius Niger, and upon some circumstances in the history of his life; written in French by Mr Claude Gros de Boze, keeper of the medals in the French King's cabinet, etc. and sent by him to Dr Mead, who communicated it to this Society. By John Ward, R.P.G. and F.R.S. N^o. 495. p. 452. May &c. 1750. Read May 31. 1750.

XIX. The learned author begins his discourse with observing, that no medals of the Roman Emperors, who reigned during the high empire, are more rare, than those of *Pescennius Niger*; that they are somewhat scarcer in silver, than in brass; and that it is the general opinion of Antiquaries, there is not one extant in gold.

And tho *Anthony le Pois*, who lived about the middle of the 16th century, affirms in his posthumous book of *Antient medals* (1), that he had a *Pescennius* in gold, and also some other persons: yet he thinks, that as it is not known what became of those medals, they were counterfeit; of which sort he has seen several, which being cast from silver ones of that Emperor, were afterwards repaired more or less artfully with a graver. He is likewise of the same opinion with regard to those, which have been collected by *Mediobarb* from other catalogues; since Mr *Vaillant*, the most knowing and experienced Antiquary of the last age, in treating of the medals of this Emperor says expressly, *ex auro non observantur* (2); and Mr *de la Bastie* has also remarked in his *Catalogue of the Roman Emperors*, that notwithstanding what is said by *Anthony le Pois*, no medal of *Pescennius Niger* in gold is to be found in any known cabinet (3).

Sigismond Liébe, who in the year 1730 published the cabinet of the Duke of *Saxe Gotha*, under the title of *Gotha Numaria*, in order to prove it superior to that of the Duke of *Parma* published by Father *Pedrussi* (4), and equal at least to that of the antient Dukes of *Arfschot* (5); has ranged the gold imperial medals of those 3 cabinets in 3 opposite columns; and when he comes to *Pescennius Niger*, he first remarks, that there is no gold medal of him in the cabinet of *Parma*; and then says, that the engraved one in the *Arfschot* collection is generally acknowledged to be counterfeit and cast. He likewise declares his agreement with all other Antiquaries, that there is no true one in gold;

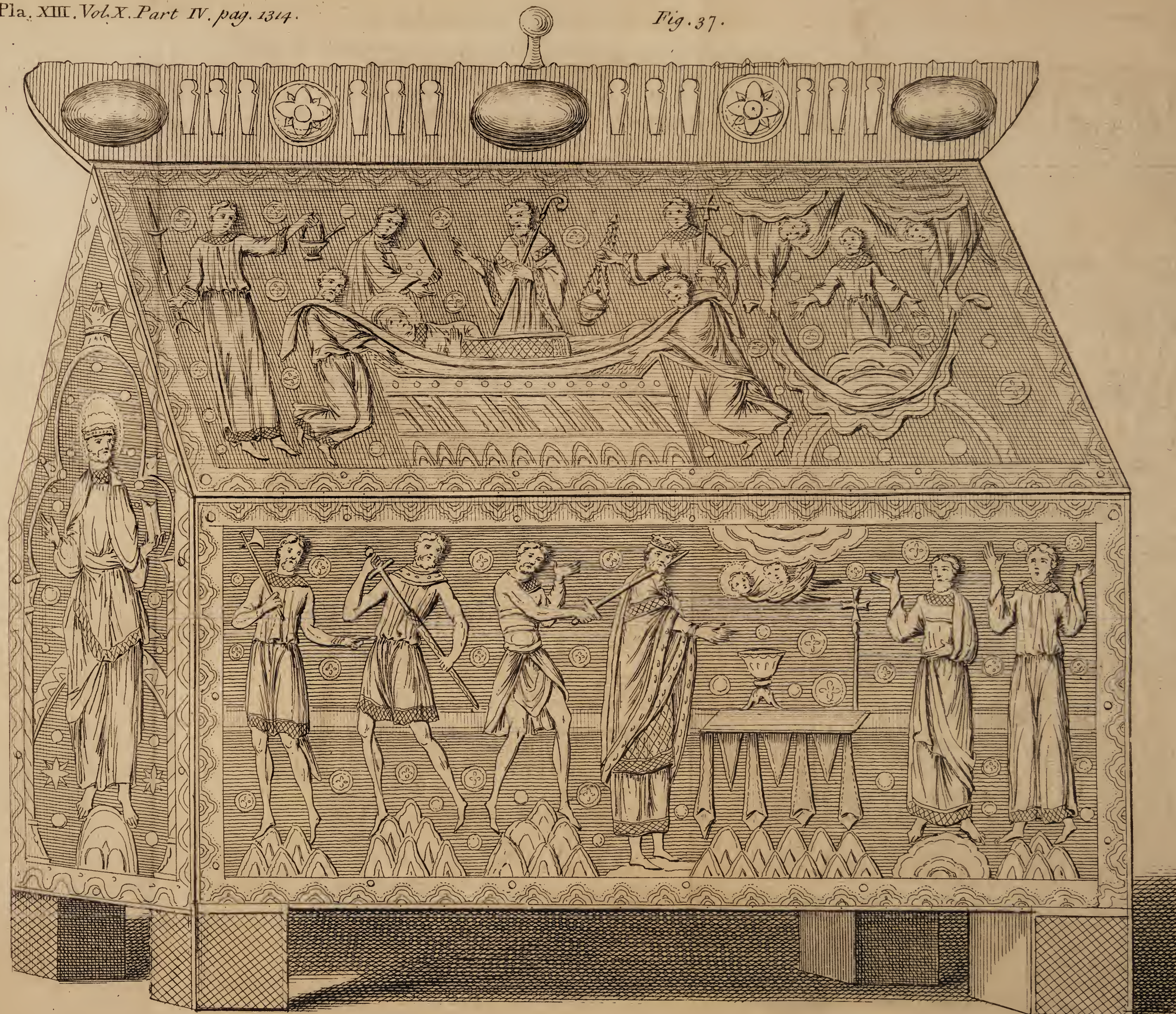
(1) *Discours sur les medailles antiques, etc. Paris. 1579. qu.*

(2) *Nmismat. Imp. Rom. praestantior.*

(3) *La science des medailles, Tom. II. p. 398. ed. 1739.*

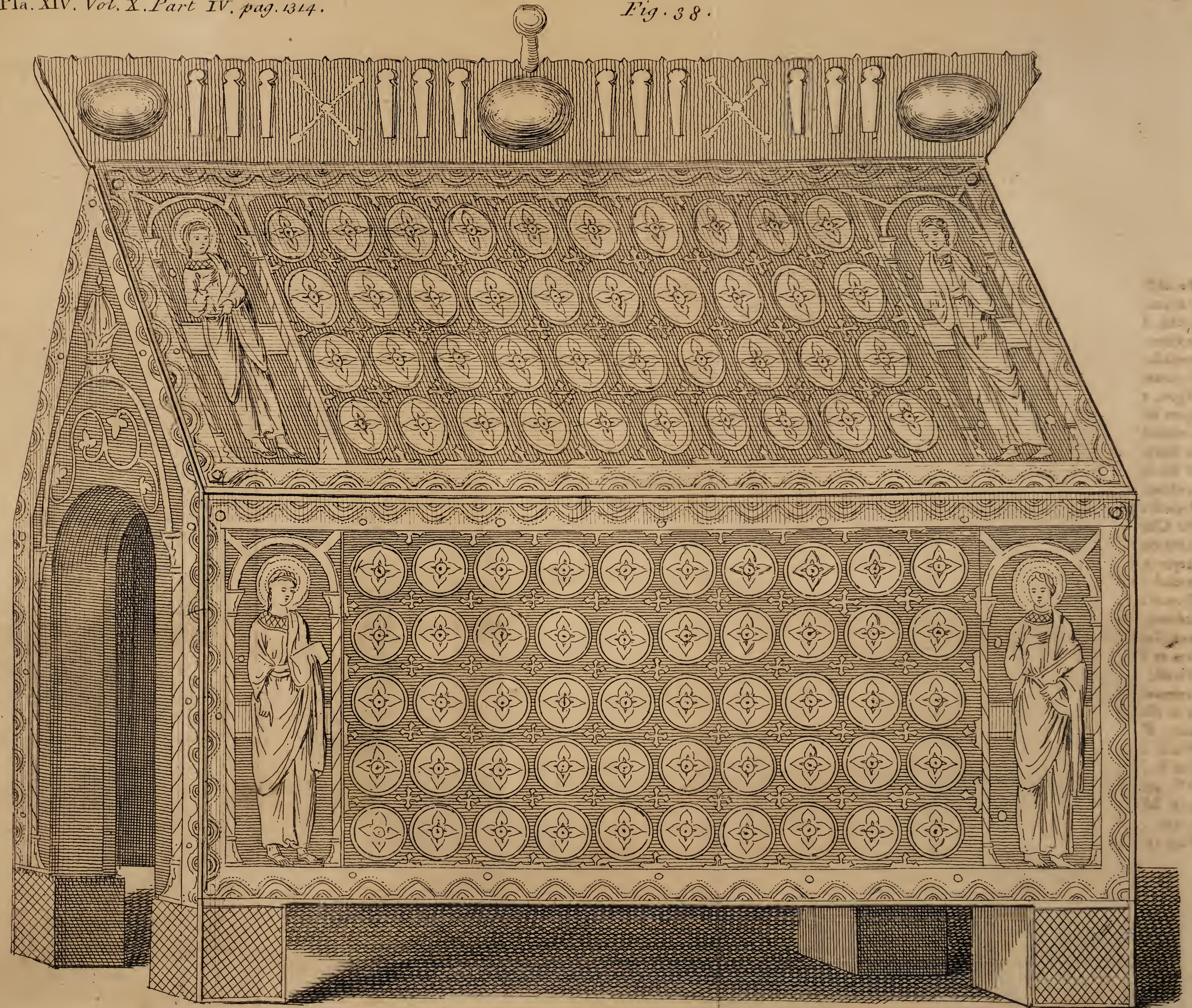
(4) See *Bandur. Bibl. Numar. p. CVII.*

(5) *Ibid p. XXXVII.*



J. Mynde sc.

*A most antient Shrine, of curious enamel'd work, on Copper, in Colours.
12 inches long 10½ high 4¾ broad.*



*The Backside of the Shrine. This elegant Antiquity is in Possession of
S.^r John Cotton Baronet. 1748.*

that, in order to make the Republic more complete, it was necessary to place the capital at the foot of the mountain, and not at the top of it.

For many years, the government of the Republic was in a state of confusion, and the people were in a state of poverty and distress. The government was unable to pay its debts, and the people were unable to pay their taxes. The government was also unable to maintain its army, and the people were unable to maintain their families.

The government was also unable to maintain its navy, and the people were unable to maintain their families. The government was also unable to maintain its army, and the people were unable to maintain their families. The government was also unable to maintain its navy, and the people were unable to maintain their families.

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but, in order to render the comparison more complete, he has ventured to place a false one of *Saxe Gotha* against that of *Arschot*, because it was one of the first in the collection, and not disapproved of by Mr *Morel* (6).

But notwithstanding this prevailing opinion Mr *de Boze* sais, that for upwards of 30 years, in which he has had the keeping of the King's medals, he has not ceased to inquire after a *Pescennius* in gold, as thinking it not impossible, but one might be found. Because, when a Prince or General was proclaimed Emperor, the first proof he gave of his authority, was to order gold or silver money to be struck as his coin in his own palace; the consent of the senate being only necessary for Latin brass coins, which were current at *Rome*, and required the usual signature S. C. for *senatus consulto*.

In the years 1726 and 1727 he received accounts, as he sais, of one and the same gold medal of *Pescennius*, as brought from 4 different quarters; first from *Spain*, then from *Sicily*, afterwards from *Malta*, and lastly from *England*. But he found it to be false, as all others had done, who had seen it. It had been cast from a silver one of that Prince, on the reverse of which is the figure of the goddess *Hope*; with the inscription of BONAE SPEI, which is the most common of any. Those in the cabinets of *Arschot* and *Saxe Gotha* have likewise the same reverse, and doubtless from the same origin.

About 10 years afterwards a learned Englishman, who came from *Montpellier*, informed Mr *de Boze*, that he had seen in a small collection of one Mr *Veissières*, Counsellor of the *Court of Aids*, 2 coins of *Pescennius* in gold; one of which was manifestly false, and the other deserved to be examined. Upon this information he applied himself to an eminent connoisseur (7), who viewed the 2 medals, as far as the delicacy of the owner would permit him; for he would not suffer them to be touched, as fearing lest some damage might insue from too near an inspection. However the Gentleman acquainted him, that they were both equally false; one being rough cast from a silver one, and the other carefully repaired, so as to give it some appearance of a genuine coin, when compared with the former, which it would not have, if viewed by itself: that the reverse was BONAE SPEI: and that such as they were, the owner would not part with them upon any consideration. But upon the death of Mr *Veissières* the examination of those medals became more easy, and served only to confirm the judgement given of them; and the intire collection was sold for little more than the weight.

At length, in the month of July 1748, Mr *de Boze* had fresh encouragement to pursue his inquiry; which he did with greater attention, and better success, than before. A barefooted *Carmelite* of the convent of *Paris* shewed him a letter, which he had received from one of his

(6) *Goth. num. cap. 111. § 2. p. 49.*

(7) *M. Bon, Premier President de la chambre des comptes, etc.*

own order at *Marseilles*, who lately arrived from the *Levant* (8), where he had been imployed as a missionary. His correspondent acquainted him, that he had a gold medal of *Pescennius*, which the curious at *Marseilles* were desirous to purchase, and had offered him a considerable sum for it; but as he hoped to get more at *Paris*, especially if it was not in the King's cabinet, he desired him to let him know that, as likewise what value *Mr de Boze* put upon it. His answer was, that he would certainly give a good price for it, if it was antient; but that he could offer nothing, till he had seen it. The owner therefore brought him the medal, which was fair, well preserved, and free from any thing, which might occasion the least suspicion; so that he valued it considerably higher, than what had before been offered, and immediately purchased it for the King.

Soon after he shewed it to the greatest connoisseurs and most curious persons at *Paris*, who were charmed with the sight of so valuable and unexpected a medal in the royal cabinet. And many both natives and foreigners being desirous of a draught of it, he ordered it to be engraved; together with a Greek medallion in silver, no less rare in it's kind, of the same Emperor, which is also in the same cabinet, having been purchased at *London* by *Mr Vaillant* of *Mr Falkner* (9), father of *Sir Everard*. A print of both these peices accompanies this paper. See *Fig. 39* and *40*.

Fig. 39.

The gold medal, *Fig. 39*. has on one side the head of *Pescennius Niger* crowned with laurel, with this legend, IMP CAES C PESC NIGER IVSTVS AVG. And upon the reverse, the goddess *Concord*, represented by a female figure standing, with a diadem on her head, one of her hands elevated, and a double horn of plenty in the other; and round the figure only the word CONCORDIA. For the letters PP, placed below in the feild, on the two sides of the figure, being the usual abbreviation of PATER PATRIAE, are to be considered as part of the inscription surrounding the head of *Pescennius*. And it is well known, as *Mr de Boze* observes, that in many medals of the Roman Emperors nothing is more common, than to find on the reverse a continuation of those titles, which could not be contained on the same side with the head. But he thinks it has not been yet sufficiently attended to, that in many medals of the cities in *Lesser Asia*, and especially of those in *Cilicia*, where *Pescennius* was first proclaimed, the title *Pater patriae*, which they expressed by the two Greek letters ΠΠ, for Πατὴρ πατρίδος, is scarce ever placed in the circular inscription on either side, but in the feild of the reverse. For which reason he is inclined to beleive, that this medal was struck at *Tarsus*, the metropolis of *Cilicia*, where that practice was more constant, than in any other place.

(8) *Le P. Elizée de St Benoist*.

(9) See *Numism. Imp. Rom. praeft.* p. 216. ed. 1696.

It would scarce be worth observing, he says, that in the word CONCORDIA the letter D is inverted after this manner α, since such mistakes are very common both in antient and modern coins; was it not to prevent any imaginary suspicion of it's being done by design, as emblematical of a pretended concord between the two Emperors *Septimius Severus* and *Pescennius Niger*. But this he shews to be highly improbable on the following accounts. First, that it is wholly inconsistent with the elegancy, grandeur, and simplicity of antient monuments; and especially of those appointed by authority, which never admitted of any kind of ridicule. Again, that it is a manifest abuse of the language of medals to imagine, that when they exhibit the name or figure of any Deity, as *Concord*, *Plenty*, *Peace*, and others, any thing more is signified, than addresses made to them on the account of what is there expressed, and hopes of it's being granted to the public. This he illustrates from several instances of imperial medals, which need not be here recited. And he further observes from the Historians of those times, that *Pescennius*, at the beginning of his reign, might think it no difficult matter to have adjusted matters with *Severus* upon terms advantageous to himself.

The silver medallion mentioned above, *Fig. 40.* has likewise the head *Fig. 40.* of *Pescennius*, crowned with laurel; and a Greek inscription round it thus abbreviated, ΑΥΤΟΚ ΚΑΙCΑΡ Γ ΠΕCΚΕΝΙΓΡΟ Δ. that is, *Imperator Caesaris Caio Pescennio Nigro Justo*. On the reverse is an eagle standing on a club, with the legend ΠΡΟΝΟΙΑ ΘΕΩΝ, *Providentia Deorum*; which is found also upon the medals of his predecessor *Pertinax*, with whom he is compared by Historians both for his civil and military virtues. And Mr *de Boze* apprehends, that as the eagle and club are the usual symbols of Greek medals struck at *Tyre*, this might probably have been coined there, either in the year 193, the first of his reign, or at the beginning of the following year; that is, before the two defeats, which he received, first near *Cyzicus* in the *Hellespont*, and after that between *Nicaea* and *Cius* cities of *Bitynia*. For then the cities, which *Severus* had artfully endeavour'd to draw over to his interest, declared for him; more from a jealousy of their neighbours, as *Herodian* observes, than from any dislike to *Pescennius* (10). This revolt was begun at *Nicomedia* in hatred of *Nicaea*, which was very zealous for *Pescennius*, and greatly favoured by him. *Tyre* and *Laodicea* followed the example of *Nicomedia*, from an aversion to *Antioch* and *Berytus*. And in like manner *Perinthus*, in opposition to *Byzantium*. In order to put a stop to this threatening mischief, *Pescennius* ordered some of those cities to be plundered and burnt, particularly *Tyre* and *Laodicea*. But among all the cities, which espoused his interest, *Byzantium* behaved with the greatest gallantry; and even after his death held out a siege of 3 years against all the power of *Severus*, till at length being reduced by famine

(10) *Lib. III. cap. 2.*

it was taken, and the walls demolished. Mr de Boze has given a succinct account of this tragical scene from *Dion Cassius* (11). But as it was not his design to write a narrative of all the occurrences relating to those wars, which may be found in the Historians, who have professedly treated of them (12); he employs the remainder of his discourse upon some particular circumstances, which respect *Pescennius*, and have not been yet so thoroughly considered, as they deserve.

And he remarks here, that the accounts of Historians relating to the descent of *Pescennius* are doubtful and uncertain; some telling us, that he sprang from a patrician and consular family, which had been employed in the highest offices of state, and dwelt long at Rome in great splendor: but others, that his birth was obscure, both his father and grandfather being no more than agents, or overseers, at the small town *Aquinum* in *Naples*; and that properly speaking he was a soldier of fortune, who raised himself gradually to command the forces in *Syria*, and gained some precarious honours, and a limited consulship, by the interest of the freedmen of *Commodus*, but principally of *Narcissus* the gladiator. But there are several antient inscriptions yet preserved, besides other monuments, which may afford some light in this affair. *Gruter* has 7 in his *Collection*, with the name of *Pescennius*; and there are some others in *Reinesius*, *John Baptist Doni*, and *Anthony Muratori*; and a Greek one in the *Miscellanea* of *Spon*, with Γ ΠΕΣΚΕΝΝΙΟΣ ΟΝΗΣΙΜΟΣ. But the great variety in the cognomen of these persons render it wholly improbable, that all of them should have been of the same family with the Emperor *Pescennius*; since there is no family, even in the most flourishing times of the republic, which appears to have contained so many branches distinguished by a different cognomen. And therefore he supposes most of them to have been freedmen or clients, who usually assumed the family name of their patrons, of which he produces many instances; besides others of foreign princes, who paid the like compliment to the Roman Emperors.

But *Philip à Turre* has published the fragments of two inscriptions (13), containing the rites and ceremonies of the *Fratres Arvales*, who were a college of priests of great esteem and dignity at Rome. Now among the members of this college mention is made of one *Pescennius Niger*. And as those inscriptions are dated in the fourth consulship of *Commodus* and second of *Ausidius Victorinus*, which answers to the year 183 of our common aera; *Muratori*, who has since republished them, says in a note, that this is the same *Pescennius*, who 10 years afterwards was advanced to the empire. But here, as Mr de Boze very accurately remarks, he has not observed the difference of the *praenomen*, which in all the remaining medals of this Emperor is *Caius*, but in both the in-

(11) *Lib. LXXIV. p. 844.*

(12) *Herodian, Dion, Spartian, Mr Tillemont, Tom. III.*

(13) *Fragment. inscript. Fratr. Arval.*

scriptions *Publius*; nor taken care, as he might have done, to support his opinion, notwithstanding that difference in the *praenomen*. For *Commodus*, who has usually the *praenomen* of *Marcus*, has sometimes that of *Lucius*; and *Geta* either *Lucius* or *Publius* indifferently; as *Aemilian* has *Caius* and *Marcus*. These are the only instances, which have occurred to him; but he thought it proper to mention them, that if any new medal of *Pescennius* should be found with *Publius* as the *praenomen*, it might not be rejected merely on that account.

For a further illustration of his family he refers to some passages in *Spartian*, who relates, that in the gardens of *Commodus* at *Rome* the figure of *Pescennius* in mosaic work was placed among those of the most intimate friends of that Emperor, as performing a sacrifice to *Isis* (14). The same writer likewise says, that 6 persons of the name of *Pescennius* were put to death by *Severus*, all of them men of eminence and dignity (15). And he further adds, that his house at *Rome* was remaining in his time, and went by the name of *Pescenniana*; on the pediment of which was placed his statue, which had been sent him as a present *a rege Thebaeorum*, as he expresses it (16). But what the real character of the person was, called here *rex Thebaeorum*, has not hitherto, as Mr *de Boze* remarks, been rightly settled; with this inquiry therefore he concludes his discourse.

Casaubon, and after him *Salmasius*, *Reineccius*, and Sir *John Marsham*, suppose there was then a governor of that part of *Aegypt*, subject to the *Romans*, to whom they gave the title of *rex*. But as this notion is no ways countenanced by history, he thinks it cannot well be admitted; and then proceeds to offer his own opinion with equal learning and address. *Augustus*, after the defeat of *Antony* and *Cleopatra*, having reduced *Aegypt* into the form of a province, divided the government of it among several persons of the equestrian order; not thinking it safe to intrust a nation so unsteady, daring, and always given to change, in the hands of senators, who were otherwise too powerful. This division of *Aegypt*, as *Arrian* observes, was made by the *Romans* in imitation of *Alexander* (17). And the governors, as *Strabo* says, appointed by the Emperor, tho persons of moderate rank, had τὴν τοῦ βασιλέως τάξιν, the authority of a King (18); or, as *Tacitus* expresses it, were *loco regis* (19). And agreeably to this *Spartian*, speaking of the Emperor *Severus*, tells us, that notwithstanding he granted to the *Alexandrians* a civil jurisdiction for their private concerns; yet in all other things he subjected them to the absolute government of a person sent by the Emperor, *ut sub regibus ante vivebant* (20). From these reflections, Mr *de Boze* thinks it may naturally enough be imagined, that this *King of Thebes* mentioned by *Spartian* was no other than a Roman Knight, who commanded at

(14) *Fragment. inscript. Fratr. Arval.* p. 76.

(15) *Fragment inscript. Fratr.*

Arval. p. 69.

(16) *Ibid* p. 78.

(17) *Arrian. De exped. Alex.* l. iii. c. 5.

(18) *Lib. xviii. pag. 797.*

(19) *Hist. lib. i. cap. i.*

(20) *In vit. Sever.* p. 170.

A description of some clay moulds of ancient Roman coins.

Thebes with the authority of a King; and might be so called there, in common with the governors of other parts of *Aegypt*; and even at *Rome* too in their ordinary discourse. Which may seem the more probable, if it be considered, how free the Romans made with that name, and to how many things they applied it; as *rex sacrorum*, *conviviorum*, and others. He further observes, that the custom of giving the title of *King* to one of their own magistrates obtained also at *Athens*, and some of the cities of *Greece*; and then concludes his discourse with a favourable character of *Pescennius Niger*, taken from antient accounts.

A description of some clay moulds or concaves of ancient Roman coins found in Shropshire; by Mr H. Baker, F. R. S. N^o. 483. p. 557. Mar. &c 1747. Read June 4. 1747.

XX. Having been lately favoured by a very ingenious Gentleman of *Shrewsbury* with the sight and use of some ancient moulds made of clay, bearing the same types and inscriptions that some of the *Roman* coins are known to have, and being enabled, by the assistance of our most worthy *President*, to make out the inscriptions and types impressed on the said moulds, I judged it might prove agreeable to this *Society* to see specimens of so great a curiosity as these moulds seem to be, and to have some account concerning them.

Four of the five in my possession were found in digging sand, at a place called *Ryton* near *Condover* (query whether *Rutunium*), 5 miles from *Wroxalter* (i. e. *Uriconium*) in *Shropshire*, about a mile from the *Watling-street* road: these are all of the size of a *Roman denarius*, and little more than the thickness of our halfpenny. They are made of a smooth pot, or rather brick-clay, that seems to have been well cleansed from sand or dirt, and well beat or kneaded, to render it fit for taking a fair impression. Great numbers of these were found, but, for want of care, most of them were broke in pieces. They are described N^o. 1, 2, 3, 4, in the account below. The fifth, which is twice as thick as any of the rest, was found at *Wroxalter*; the clay it is made of differs but little from the former; and the impression on it is also of the size of a *denarius*, and of the same time with the rest.

Fig. 41.

N^o. 1. Is probably the reverse of a *denarius* of *Severus*, mentioned in *Mezzobarba*, p. 268. from *Noris de Vot. X.*—*Figura velata coram ara sacrificans.*—VOTA. SVSCEPTA. X.

Fig. 42.

N^o. 2. On one side—*Caput Juliae Severi.*—IVLIA AVGVSTA. On the other side is the reverse of a *denarius* of *Severus*, *Mezzob.* p. 274.—*Victoria gradiens cum fune super scuto.*—P. M. TR. P. VIII. COS. II. P. P.

Fig. 43.

N^o. 3. A reverse of *Caracalla*. *Mezzob.* p. 286.—*Trophæum de Parthis cum duobus captivis assidentibus.*—PART. MAX. PON. TR. P. V. COS.

N^o. 4. *Caput Juliae Severi.*—IVLIA AVGVSTA.

N^o. 5. *Caput Juliae Severi.*—IVLIA AVGVSTA.

I remember no account of any such kind of moulds being found in other countries, excepting some said to be found at *Lyons*; but I believe more of them have been discovered at different times in *England*; though I cannot pretend to say when, or where. I have been informed, that

that some years ago, the Earl of *Winchelsea* had several impressions or moulds of this sort (all joined together side by side) on one flat piece of clay, as if for the making many casts at once: they were all of the Emperor *Severus*: and I have seen, in the Earl of *Pembroke's* most valuable collection a clay mould impressed on both sides, as N^o. 2. amongst these also is, one of the sides bearing the head of the same Emperor, and the other side a known reverse of his. Four of the five in my hands are also of *Severus* or his wife *Julia*, and the other is a reverse of *Caracalla*, his son and immediate successor; so that all, we know of, may be said to be of the same time very nearly.

They are seemingly intended for the coinage of money; though it is very difficult to conceive in what manner they could be employed to that purpose; especially N^o. 2. which has an impression on both sides; unless we should suppose they coined 2 pieces at the same time, by the help of 3 moulds, of which this was to be the middle one.

If, by disposing these into some sort of iron frame or case (as our *Letter-Founders* do the * brass moulds for casting their types) the melted metal could be poured into them, it would certainly be a very easy method of coining, as such moulds require little time or expence to make, and therefore might be supplied by new ones, as often as they happened to break.

These moulds seem to have been burnt or baked sufficiently to make them hard, but not so as to render them porous like bricks whereby they would have lost their smooth and even surface; which in these is plainly so close, that whatever metal should be formed in them would have no appearances like the sand-holes, by which counterfeit coins or medals are usually detected.

XXI. The brass plate, which accompanies this paper, and has been the occasion of it, was dug up some time since at Market-street in Bedfordshire; which lies in the Roman road called *Watling-street*, about 5 miles on this side Dunstable; and was brought to the Society by their worthy Member, *Samuel Clarke, Esq*;

The inscription ingraven on the two sides is,

TES. DEI. MAR
S E D I A R V M

See Fig. 44.

Which words may, as I apprehend, be read at length in the following manner:

Tessera Dei Martis Sedarum.

The first abbreviated word TES. I take to stand for *Tessera*, a dye or cube (a), so called from the Greek word τέσσαρα or τέσσερα, four; respect

(a) *Macrob. in somn. Scip. lib. 11. cap. 2.* they are made of fine polished iron.

* They use no brass in their moulds,

A brief account of a Roman tessera; by Mr John Ward, F.R.S. & Prof. Rhetor. Gresh. N^o. 486. p. 224. Feb. & Mar. 1748. Read Mar. 3. 1747. Fig. 44.

being had to it's number of sides, distinct from the 2 horizontal planes, above and below. And under this consideration it was distinguished from the *talus*, which being round at each end contained only four planes or faces, whereon it could stand, and therefore when thrown had no more than two side faces in view. Hence *ludere talis et tesseris* are spoken of by Roman writers as 2 different games (*b*).

But if this was the first and original notion of the word *tessera*, it was applied afterwards to many other things; and that not so much from a similitude in the figure, as from the relation they bore to some other thing, of which they were the sign or token; as the points, on the upper plane of the dye denoted the good or ill success of the cast. To recite the several uses of this word would both be tedious and unnecessary; and therefore I shall mention some few only, from which the design of this plate may the more easily appear.

And I shall begin with the *tessera hospitalis*, which was either public or private. As to the former, we find among the inscriptions published by *Gruter* instances of two municipal towns, who put themselves under the patronage of a Roman governor. And the reciprocal ingagement between them, which was ingraved on two copper plates, in the form of an oblong square, with a pediment at the top, is called in both *TESSERA HOSPITALIS* (*c*). The design of the latter was to cultivate and maintain a lasting friendship between private persons, and their families; and gave a mutual claim to the contracting parties, and their descendants, of a reception and kind treatment at each others houses, as occasion offered. For which end it was requisite, that those *tesserae* should be so contrived, as might best preserve the memory of that transaction to posterity (*d*). And one method of doing this was by dividing one of them lengthwise into 2 equal parts, upon each of which one of the parties wrote his name, and interchanged it with the other. A draught of one made of bone, and so divided, may be seen in *Thomasinus*, with the name of the person on each part. Upon one of them is,

POLYNICES

ASCANIO. F.

And upon the other,

CLAPHYR

ANDRAE. M. I. F.

The names are written on the inside; and when the two parts were put together, they made a cylinder (*e*). From this custom came the proverbial expression, *tesseram hospitalem confringere*; which was applied to those persons, who violated their engagements (*f*).

(*b*) *Cic. De senect. cap. 16.*
Pœn. v. 1. 22. v. 2. 87.
Cistell. II. 1. 29.

(*c*) *Pag. CCCLXII. CCCLXIII.*
 (*e*) *De tesser. hospital. cap. 15.*

(*d*) *Plaut.*
 (*f*) *Plaut.*

The *tesserae frumentariae* are often mentioned by Roman writers, which were small tallies given by the Emperors to the populace at Rome, intitling them to the reception of a certain quantity of corn from the public at stated seasons. And those, who were possessed of them, when they did not want the corn, sometimes sold them to others; as we learn from the satyrist, when he says:

*Summula ne pereat, qua vilis tessera venit
Frumenti (g).*

The person, who had the inspection of these *tesserae*, and distributed the corn to those, who produced them, seems to have been called *tesserarius*; as *Pignorius* observes from a funeral monument, inscribed SYMPHORO TESSERARIO SER. CAESARIS (*h*). These *tesserae* were first made of wood, as appears from the words of *Pliny*, where treating upon the nature and properties of trees he says, *Ligustra tesseris utilissima* (*i*). But *Fabretti* has published the draughts of 2 of them made of stone, in the form of cylinders, and of the same size with the originals. The length of them is somewhat short of 3 inches, the diameter $\frac{3}{4}$ of an inch, and the following inscriptions cut upon them contain the names of the persons, to whom they belonged:

| | |
|--------------|--------------|
| TORQVATVS | LVPVS |
| CREOP. D. I. | PELORI D. I. |

Where D. I. the two last letters in each inscription stand, as he supposes, for *die prima mensis*, the time appointed for receiving the corn (*k*).

There was also another sort of *tessera*, not much unlike these, which intitled persons to a sight of the public games and other diversions; but they are generally made in the form of an oblong square. *Pignorius* has given us the draught of one in his own possession, which consisted of ivory. Upon one of the sides is the name PHILOMVSVS PERELI, on the next SPECTAVIT, on the third a *trident*, and upon the fourth a *palm branch* (*l*); the two last of which plainly shew, that it was given for admission to the combats of the gladiators. Others of them had on different sides the name of the person, with the day, on which the show was exhibited, and the names of the consuls at that time. Instances of these may be seen in *Thomasinus*, one of which, as he says, was made of yellow glass (*m*).

But the *tessera militaris* most frequently occurs in the Roman Historians, which was the signal given by the general, or chief commander of an army, as a direction to the soldiers for putting in execution any duty or service required of them. This upon urgent occasions was only

(g) *Juvenal. Sat. VII. 174.*

(h) *De servis, pag. 318. ed. 1674. oct.*

(i) *H. N. lib. XVI. cap. 8. § 31.*

(k) *Inscript. antiq. pag. 530.*

(l) *Ubi supra,*

pag. 38.

(m) *De tess. hospital. cap. 15.*

vocal; as for a sudden march, or an attack upon the enemy. But in ordinary cases, as for setting the watch, taking their dinner, or the like, it was written on a tablet. Tho in either way it was first given by the general to the officers next in rank, and from them to the subalterns, till it came to the person, whose province it was to communicate it to the soldiers in each company. This tablet was commonly made of wood, as appears from *Polybius*, who calls it *ξύλινον*, a small piece of wood (n). The signal inscribed upon it was very short, and usually comprised in one or two words; as *viſtoria*, *palma*, *virtus*, *Deus nobiscum*, *triumphus imperatoris*, mentioned by *Vegetius* (o); with many others of the like nature, which may be seen in antient writers. The person, whose office it was to impart the signal immediately to the soldiers, is by *Vegetius* called *tesserarius* (p). Hence in *Gruter's* inscriptions we meet with AVRE. IANVARIVS. TESSERARIVS. LEG. XIII, and C. GALERIO. C. LIB. AGATHON. TESSERARIO. COH. XII. PRAET. MILITVM, as also L. POMPEIO. L. F. POMP. REBVRRO. TESSERARIO. IN. CENTVRIA... (q). By which different forms of expression compared together one would be led to conclude, that every century had it's *tesserarius*, from whom the soldiers immediately received the signal; and that when the legion or cohort only is mentioned, the meaning is not, that the person named in the inscription performed that office to the whole legion or cohort, but only to some particular century in each of them.

But besides these civil and military *tesserae* there were others, which more especially related to religious affairs, and may therefore be called sacred; to which the inscription on this brass plate seems to agree. For the two next words ingraven upon it, namely DEI. MAR. must, I think, stand for *Dei Martis*. And if the last word SEDIARVM be taken for the name of a town, called *Sediae*, this *tessera* may respect the *God Mars*, as the tutelar deity of that place. The religious worship among the Romans consisted chiefly in sacrifices and other public ceremonies, the expense of which in particular places was supported either by the contributions of the inhabitants, or by private gifts. We have an instance of the latter in an inscription first published by *Reinesius*, where it is said, that *L. Veratius Felicissimus*, (r) patron of *Tolentium*, (or *Tollentium* a municipal town in *Italy*) gave to the inhabitants their annual sacrifices, which were offered on the eleventh of May for a plentiful harvest. That inscription is cut on a brass plate in the form of an oblong square, with a female bust in a pediment at the top, designed very probably to represent the deity, to whom they addressed. As the inscription is peculiar in it's kind, I shall here give the whole of it, as it stands in *Reinesius*.

(n) *Lib. vi. pag 479. ed. Paris.*
cap. 7.

(o) *Lib. iii. cap. 5.*
(q) *Pag. DCVI. 10. DCVIII. 7. DCIX. 10.*

(p) *Lib. ii.*
(r) *Grut. Pag. CXCIV. 2.*

TESSERAM. PAGANICAM
L. VERATIVS. FELICISSI
MVS. PATRONVS. PAGANIS
PAGI ♡ TOLENTINES
HOSTIAS. LVSTR. ET. TESSER.
AER. EX. VOTO. L. DD
V. ID. MAIAS. FELICIT. (s).

This is called *tessera paganica*, as I imagine, from it's intitling the *pagani*, or inhabitants of that town, to the annual claim of the sacrifices therein mentioned. And so far it agreed with the nature of a public *tessera*, which being lodged in the hands of the proper officer, authorized him to collect the several contributions assigned for such religious purposes. And of this latter sort I take the plate to have been, which makes the subject of our present inquiry; both the form and size of it suiting very well with such a design, as it was portable, and ready to be produced, if occasion required. And agreeably to this notion of the word *tessera* the antient *Glossaries* interpret *tesserarius* by γραμματεὺς, a scribe or clerk.

As to the following word *SEDIARVM*, tho it no where else occurs, that I know of; yet this, I presume, can be no just objection against it's being taken here for the name of a town, called *Sediae*: when it is considered, how many instances of the like nature are to be found in the inscriptions collected by *Gruter* and others, which give us the names of many antient places in the Roman provinces not mentioned by any other writers. And besides, the form of this word appears analogous to the names of several other Roman towns here in Britain; as, *Durobrovae* Rochester, *Ratae* Leicester, *Rutupiae* Richborough, *Spinae* Spene, and some others. It is not improbable, that this plate was found not far from the place, whose name it bears; and which might be situated among the *Cateuchlani*, as their territories are described by Camden (t). But as I have never before seen, nor heard of any thing similar to it, I would submit what is here offered to the judgement of the curious in these inquiries.

XXII. The beginning of June last, a labouring man, of *Amcotts* in the isle of *Axholm*, in the county of *Lincoln*, was digging turf or peat in the moors of *Amcotts*; and, at about 6 foot from the surface, his spade cut the toe of a sandal, which dropped into the pit he was grave- ing peat in; also part of the foot dropped in, which terrified the man, and he left it. Hearing of this discovery, I went and took some ser- vants with me, to make further discovery; when we soon found the

A letter from Mr G. Stovin to his son, concerning the body of a wo- man, and an antique shoe, found in a morass in the Isle of Ax-

(s) *Append. num. 8.*

(t) *Britann. pag. 275. ed. 1607.*

holm in Lin-
colnshire. N^o.
484. p. 571.
Oct. &c. 1747.
Read Oct. 22.
1747.

other fandal (which I now send you whole and firm). It was very soft and pliable, and of a tawny colour, with all the bones of that foot in it, and all the grisly part of the heel: and proceeding further, we found the skin and thigh-bones, which I measured to be 18 inches long. We then found all the skin of the lower parts of the body, which was of the same colour of the sandals, and very soft, with fresh hair upon it, &c. which distinguished it to be a woman. The skin drew or stretched like a piece of doe-leather, and was as strong. We then found the skin of the arms, which was like the top of a muff or glove, when the bones were shaken out. We then found this hand I have sent, with the nails as fresh as any person's living; which are now, both hand and nails, shrunk very much, since it was exposed to the air: this hand is the lady's natural skin so tanned, with the nails. We left the bones in the fingers, where the nails are, for fear the nails should drop off, if that joint was taken out.

I want to be informed what age they wore those sandals in. These must, I think, be very antient, and have most certainly been made of a raw hide, by reason they, and the skin of the lady, were both of one colour, and both had one tanner; which I presume, is the moor-water; which is exactly of the colour of coffee; and made so by reason of such great quantities of oak and fir-wood, that we frequently dig out of these moors; several oak-trees affording 1000 pales for fencing, $5\frac{1}{2}$ feet long, and 6 to 8 inches broad; which oak-wood is rated as black as jet. The fir-wood retains it's turpentine-smell, and in hot weather (when it is exposed to the sun) the turpentine will drop from it. This wood is frequently riven into laths for the roofs of houses or floors; and what is remarkable, no worm will touch them. The pales mentioned above are sold from 10 to 15 shillings *per* hundred. We frequently find hazle-nuts and fir-apples in abundance; which I think is a plain proof, that the trees fell in autumn, when the fruits were at maturity. I think Dr *Shuckford* makes it plainly appear that the general flood happened in autumn. This lady in all probability was overwhelmed by some strong eddy of water; for she lay upon one side bended, with her head and feet almost together.

It appears by the maps of the country, that this has been the rendezvous of all the waters from the S. W. and N. parts of the kingdom; as for instance, the river *Dun*, from *Doncaster*, *Rotherham*, and *Sheffield*, which took in many more streams; as the *Idle*, *Trent*, *Torn*, *Dare*, *Rother*, &c. &c. Then the river *Trent*, which runs S. to *Gainsbrough*; then to *Torksey*, *Newark*, *Nottingham*, *Derby*, *Burton upon Trent*, *Strafford*, *Trentham* in *Staffordshire*: and takes in a vast number of rivulets: then the *Ouse*, which comes from near *Richmond*, and takes in the *Ure*, *Wharf*, *Bishop's Dike*, *Aire*, *Calder*, and a great number of rivulets; which are all lost in that famous estuary the *Humber*.

It is also to be observed, that here is one morass 20 miles round, part in *Hatfield-Chace*; another 10 miles round in the same chace, where
the

the famous *William* of *Lindham* had his cell. In the middle of it, where his body was found, for 8 miles round, is all a morafs.

The connoisseurs will give you their opinion in the chief thing I want to know, which is, in what age those fandals were worn, and by what nation: for it is not like the *Scots* or *Irish* broges; though the *Scots*, I think, formerly inhabited but a little way off, to wit, N. of *Humber*. Perhaps the *Danes* may wear such, or the antient *Saxons*; for both these people must be well acquainted with these parts, as the *Danes* under *Edgar-Atbeling* incamped a whole winter in this neighbourhood, and had a station at *Gigansburgh*, now *Gainsbrough*, upon this river *Trent*.

At *Boxby* was a famous *Roman* pavement, 15 yards square, the *Roman* road, &c.; also a square platform at *Aldbrough*, which I take to be *Roman*, though no discoveries have as yet been made there; but at *Roxby* large quantities of *Roman* coins have been found.

P. S. As to this water upon these moors preserving human bodies *, it is most certain; viz. Part of a body taken up at *Geel* by your grandfather *Mr Empson* 50 or 60 years ago, and one in the great moor near *Thorn*, about 7 years ago, with the skin like tanned leather, the hair, teeth, and nails quite fresh.—You will see the sandal † is of one piece of leather, and a seam at the heel, with a thong of the same leather. Fig. 46. and 47.

It is the skin of the hand that is stuffed, which has suffered by the spade ‖.

C H A P.

* See these *Transf.* N°. 434. p. 413.

† *Mr Catesby*, F. R. S. being present, said, this shoe or sandal was exactly like what the *Indians* in *Virginia* wear at this day, and call *Mokasin*.

‖ *Mr Geo. Virtue* communicated to me (C. M.) his sentiments concerning this sandal in the following words:

‘ When the above letter was read at the *Society of Antiquaries*, there was produced a hand of the woman therein mentioned, and a sandal or shoe taken from one of her feet; it being made of leather, tanned ox-hyde; but remarkable for being cut out of one flat piece, (see Fig. 47.) so as to fold about the foot and heel; the form and make being so contrived without under heel-piece, as to be flat to tread on; the shape, that of a woman’s foot, and the toe round-pointed. This being of an antient form, the Society ordered an exact draught to be taken of both that and the hand; which drawings are preserved amongst others belonging to that Society. It may be observed concerning the antiquity and use of leather shoes in *England*, that this shoe or sandal appears by it’s form to be ancient. I conceive it was before *Edward* the IV.’s time, when, by custom, piked shoes had increased in length, that all such who wore them in excessive length were to be mulcted, or have them cut shorter, in passing in or out of the city-gates of *London*. This very likely had passed amongst the better sort of people about the kingdom; for *Chaucer* in his time mentions the use of long piked shoes, so long as to be tied up by strings or small chains to their knees.

‘ Thus it might have been with mens shoes, but not in so long a degree for womens use; tho’ observing antient pictures of men and women in books of illuminations, piked shoes appear in several reigns from *Ed. III.* to *Rich. III.* in *England*.

‘ Also on our antient monuments of stone or alabaster cumbent statues have mostly piked shoes. But some of earlier date than *Ed. III.* have broad turn-up shoes at the

'toes, of the same like form and make as this woman's. The mens broad toes, and the womens narrow.

'Therefore I conclude this very sandal could not well be earlier than *Ed. I.* or *Hen. III.*; also, that the cutting the form, and sewing to form the heel cleverly, by a stitching behind the heel with a small leather thong, may have been in use before that of waxed thread used by shoe-makers, formerly called cord-wainers.'

Fig. 45, 46,
47.

Fig. 45. shews the shoe side-ways, laced, as when upon the foot. *Fig. 46.* the same seen from above. *Fig. 47.* the same unlaced, and laid flat, to shew the manner of it's being cut out of the raw hide.

CHAP. II.

VOYAGES and TRAVELS.

Concerning the
island of Zet-
land, in 2
letters from
Mr Thomas
Preston to
Mr Joseph
Ames, F.R.S.
N^o. 473. p.
57. May &c.
1744. dated
Zetland,
Jan. 31. 1744.
Read May 31.
1744.

I. **T**HIS island has very rarely any correspondence with the rest of the world for the 6 winter months; I should say, for 6 of the winter months; for the year may be said to contain 10 months of winter, and two of cold raw weather. I thought it very absurd, to hear the inhabitants complain of heat, at the same instant that I complained of cold, and wished for a great coat. They are so accustomed to stormy bad weather, that they will venture to sea in small boats, when you would not venture to cross the *Thames*—It is the northermost belonging to *Scotland*, situated between the lat. of 60° and 61°: it's length is, N. and S. 60 miles; it's breadth 30; and so divided into head-lands and smaller islands, creeks, bays, inlets, and coves, &c. that you cannot place a compass on any the most inland parts of it's chart that shall be 2 miles from the sea; which makes it extreme difficult to make a good draught of the island; of which there is no chart extant worth naming.

The land is wild, barren, and mountainous, nor is there so much as a tree or bush to be seen. The shores are difficult, and, in many places, inaccessible, rude, steep, and iron-like; the sight of which strikes the mind with dread and horror; and such monstrous precipices, and hideous rocks, as being all *Brobdignag* before your thoughts.

In winter the sun sets soon after it rises; and in summer it rises again soon after it's setting; so that the nights at that season are near as light as the day; as, on the contrary, the day in *December* is near as dark as the night. About the solstice, we see, almost every night, the *aurora borealis*, which spreads a broad glaring appearance over the whole northern hemisphere.

I shall only just mention, that a comet has appeared to us for some time from the west, large and plain to the naked eye.

Letter II.

dated Leith,
May 12. 1744.

The island is called by the *Dutch*, *Hitland*; by us, commonly, *Sbetland*; but the proper name of it is *Zetland*; wherein there are thirty parish-churches, and about 80 gentlemens houses, besides the towns of *Lerwick* and *Scalloway*: it was first inhabited by the *Pights* or *Piets*, who

Fig. 39.



E CIMELIO REGIO

Fig. 43.



Fig. 42.



Fig. 45.



Fig. 40.

Clay Moulds



Fig. 42.



Fig. 46.

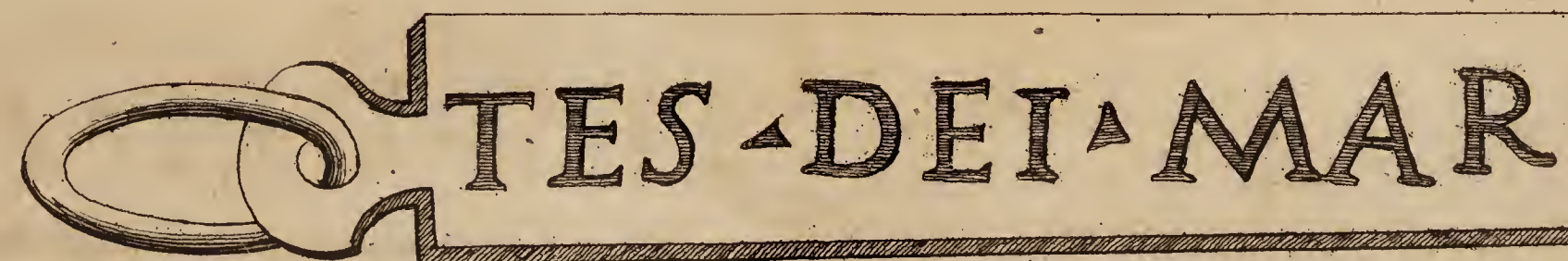


Fig. 44.

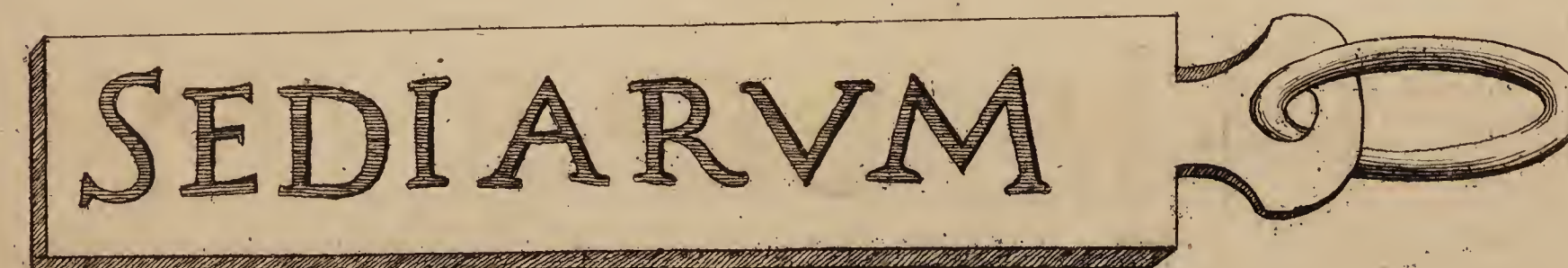
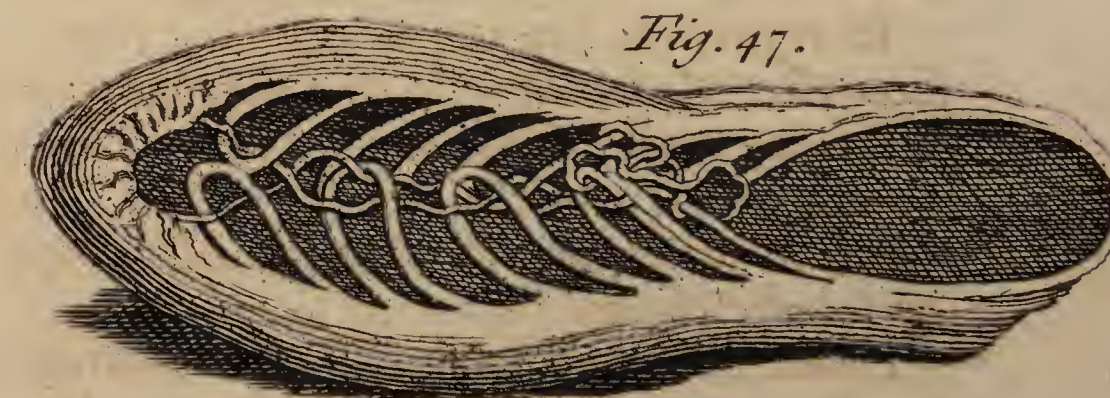


Fig. 47.



The first settlement of the island of Jersey was made by the Normans in the year 1034. The island was then a part of the Duchy of Normandy, and was governed by a Count. The Count of Jersey was a member of the royal family, and his seat was at St. Helier. The island was then a part of the Duchy of Normandy, and was governed by a Count. The Count of Jersey was a member of the royal family, and his seat was at St. Helier.

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who were driven out by the *Danes*. *Christian*, King of *Denmark* and *Norway*, whose daughter *Margaret* was given in marriage to *James III.* of *Scotland*, in 1468, agreed, That the islands of *Orkney* and *Zetland* should remain in the possession of the said King *James*, until he had paid to him 50,000 *Rhenish* florins for his daughter's dower; and *Christian* afterwards, upon the birth of a young Prince his grandson, called *James*, renounced his title to the said island, in favour of King *James*; which has ever since belonged to *Scotland*.

The long. of *Zetland* differs but little from the eastern part of *Scotland*; and it's lat. is from 60 to 61° N.: the longest day is near 20 hours. I have read a very small print in my chamber at midnight with the windows shut. The air is temperate, considering the country lies so far N.; and agrees tolerably well with those that can endure cold and a thick fog. I must own, I have not found the winters so cold as in some other parts of *Great Britain*; nor are the summers near so warm; for which, indeed, the length of the days makes some amends. I have heard, that some fishermen have affirmed, that at sea they have seen the sun's body at midnight: but that is impossible; since the sun in the shortest day in *Dec.* is 4 hours above the horizon; and it must certainly be depressed as long under it in *June*. The winters are more subject to rain than snow; nor do the frost and snow continue so long on the ground, as upon the main land of *Britain*; yet I saw it wholly covered with snow the 20th of *May* the last year. The winds, during the long winter season, continue to blow most boisterously generally between the S. and the W. which occasions many shipwrecks. There have been 3 or 4 on the island in the time I was there. The land is mountainous and moorish, abounding with moss and heather; under which they dig peat (or turf) for firing; under that is hard rock.

Their horses are very little, but strong, and well mettled, which they call *shelties*. Their oxen, swine, and sheep, of which last they have plenty, and their cattle of all kinds, are small. The price of a horse is a guinea; an ox somewhat less; a sheep half a crown; a calf six-pence. Rabbits they have in some parts of the country. Frogs and toads there are none; nor, perhaps, any poisonous animals. There are many otters, which they call *tikes*; and seals, which they call *selkies*: sometimes there are many young whales, which they call *pellacks*, or spout-whales: they run into the creeks, and so intangle themselves among the rocks, that they are cast on the shore, or easily taken.

There are plenty of sea-weeds, called *tangle*, growing on the rocks, of which might be made *kelp* for the making of soap. There is plenty of shell-fish of most kinds: their oysters are the best I ever eat. In the sea they catch cod, ling, haddock, whittings, scate, turbut, herrings, cole-fish, flukes, trouts, &c.

There are many sorts of wild-fowl; namely, the *Dunter* goose, *Clark* goose, *Solan* goose, swans, ducks, teal, whaps, toists, lyres, kittiwaiks, maws, plovers, scarfs, &c. there is likewise the ember-goose, which is said to hatch her egg under her wing: this is certain, that none

ever saw her on the land, or out of the water; and that they have a cavity or hollow place under one of their wings only, capable of containing a large egg.

There are very large eagles, which they call *earns*, which prey upon the young lambs, &c. There is a law in force, That whoever kills one of these eagles, is to have a hen out of every house in the parish wherein it is killed (tho' it is never now demanded).

I don't doubt but that there are mines of silver, tin, and lead, in the country; for several sorts of minerals are to be found: and I have been credibly informed, that pieces of pure massy silver, of considerable bigness, have been turned up by the plough: but such treasures are neglected, or not improved, through the poverty or carelessness of the proprietors. In many places, marl, quarries of excellent freestone, lime-stone, and slate, are found, and some veins of marble

Sometimes there are cast up by the sea, timber, pieces of wreck, hogsheds of wine, brandy, &c. which are frequently grown over with the shells of a fish called *cleck geese*, which I take to be the *conchæ anatiferae*; and sometimes *sperma ceti*, ambergrise, water-sponges, and camshells (*os sepiæ*) are found on the shore.

There is no forest or wood, nor so much as a tree, or hardly a bush of any sort in the whole country, except in some gentlemen's gardens; and those never dare to peep over the garden-wall, for the blast of the N. wind.

The country is so divided by the sea, that it cannot be expected there should be in it any rivers; yet there are many small brooks, and little runs of water called *bourns*, and *lockes* or lakes; many of which afford trouts; and I have eaten excellent cod-fish, that have been taken in one of these *lockes* or lakes of perfect fresh water, which has been very near the sea.

The people are generally civil, sagacious, of a ready wit, and of a quick apprehension, piously inclined, much given to hospitality, civil and liberal in their entertainments, and exceeding kind to strangers; which I may say indeed from experience; for I never met with more civility in any part of the world.

They are generally of a dark complexion, personable, and comely enough. The women are lovely; and the gentry of them go well dressed, are genteel in carriage, well-behaved in company, and smart and pleasant in conversation, even to a miracle, considering they live in such a remote island, which has so little correspondence with the rest of the world: they delight more in the conversation of men, than in the common tittle-tattle of their own sex; they are strangers to plays, opera's, masquerades, balls, assemblies, set visiting-days, extravagant dress, gallantry, &c. and are free from those fashionable vices which so much disgrace their betters: in a word, they are modest virgins, and virtuous wives: adultery is not so much as known among them: with the common sort fornication sometimes happens; but their constancy is such,
that

that they are sure to marry each other after; nevertheless, if a child happens to come in less than nine months after the marriage, they are both obliged, by the law of *Scotland*, to do penance in the *kirk*. This heinous sin the pious priest calls *antenuptial fornication*.

The country is most commodious for navigation; which makes me wonder it has been so long neglected, and that we have not even so much as a map of it. There are more than 20 safe harbours, of easy access, capable of receiving large ships; the most remarkable of which are *Lerwick* or *Bressay* sound; *Dura Voc* and *Balta* sound on the E. side, and *Scalloway*, *Olisvoe*, and *Valley* sound on the W. side of the country. The coast is all high and bold, and may be seen many leagues from sea. There are no sands round the whole island, and but few sunken rocks, and those very near the shore; except one dangerous shoal on the W. side, called *Have de Grind*, and some rocks on the N. W. above water; both which, as well as the whole island, I have exactly surveyed.

N. B. The author has lately published a very accurate map of these islands.

II. When I got to *Barcelona*, I did not find an opportunity of going immediately to *Naples*, as you proposed; yet am I not sorry that I was forced through the S. of *France*, where are many places, I thought, well worth seeing.

When I got into *Italy*, it was most convenient for me to hasten to *Rome*; where I spent the winter; and went early to meet the spring at *Naples*; from which I began your route.

You cannot more regret your own not having seen the natural curiosities of that place, than I do the loss thereby to the public. The voyage-writers do not seem to me sufficiently to have considered the force and effects of steam, which may be formed by springs of water falling upon a vast surface of the fluid *lava*, and talk too much of sulphur, deceived by the complexion of a salt that covers the ground in some places there. In the *solfatara* I held a cold iron in the vent, and there ran down it a stream of water. When I went down into the crater on the top of *Vesuvius*, it was full of smok. Yet I did not perceive it suffocating, and thought it steam. The guides indeed tell the *English*, that a Milorde of their country was suffocated there: being asked his name, they think it was my Lord *Plinio*. That which they call sulphur, when I got it home, ran *per deliquium*.

I owe to you the seeing of *Beneventum*; a place full of antiquities. At *Arienzo*, a village half-way to it, I saw coppice-woods, from which they make *manna*. They are of the tree which our Gardeners call the flowering ash. The manna is procured by wounding the bark at the season, and catching the sap in cups: it begins to run (they used the Scripture-term *piovare*, i. e. *to rain*) the beginning of *Aug.*; and, if the season proves dry, they gather it 5 or 6 weeks. The King has a great revenue from it; yet the tree grows as well in *England*.

A letter from Robert More, Esq; to the Pres. containing several curious remarks in his travels thro' Italy. N^o. 495. p. 464. May & c. 1750. Read June 14. 1750.

At *Terni* I was obliged to your directions for seeing the cascade below, as well as above. I went down by the side of the precipice; which I believe few have done; or they would not imagine the fall so little as *Misson* makes it, very short of what the people of the place call it. Mr *Addison*, on the contrary, makes the aqueduct at *Spoletto* as many yards, as I take it to be palms. One finds indeed strange incorrectness in all the travel-writers (though you very justly recommended the best) when one reads them upon the spot. One of them conjectures the fine bridge in ruins at *Narni* might have been an aqueduct, which manifestly rose all the way towards the town, to ease the steep ascent to it. But I was most surprized to see Mr *Addison* misquote a *Latin* verse of *Bembo's*, under a statue of *Bacchus*, which I think he calls *Apollo's*.

I believe the *Museum* of the *specula* at *Bologna* is improved since you were there; the joint collections of count *Marsigli*, *Marchese Cospi*, *Aldrovandus*, and others, form the finest sett of natural curiosities I ever saw; and are now improving by the munificence of the present Pope.

I had certainly missed seeing the continual fires upon the *Apennines*, by the badness of the weather, if it had not been for your caution. I indeed saw that at *Fiorenzuola* only at a distance; but I spent good part of a night over a more considerable one, as they told me, at *Pietra Mala*, a village among the snows. The fire I imagine to be of the same sort with that about a little well at *Brosely* * in *Shropshire*; of which I think the *Society* has had an account; the same as of the foul air sent them from Sir *James Lowther's* † coal-pits; and the like made by a Gentleman with filings of iron and oil of vitriol. The flame here, when I saw it, was extremely bright, covered a surface of about 3 yards by 2, and rose about 4 feet high. After great rains and snows, they said, the whole bare patch, of about 9 yards diameter, flames. The gravel, out of which it rises, at a very little depth, is quite cold. There are 3 of these fires in that neighbourhood; and there was one they call extinct. I went to the place to light it up again, and left it flaming. The middle of the last place is a little hollowed, and had in it a puddle of water: there were strong ebullitions of air through the water. But that air would not take fire; yet what rose through the wet and cold gravel flamed brightly. Near either of these flames, removing the surface of the gravel, that below would take fire from lighted matches.

Observations
and Experi-
ments made in
Siberia, ex-
tracted from
the Preface to
the Flora Sibi-
rica, five

III. By direction of the late Empress of *Russia*, several Members of the *R. Acad. of Sciences* at *Petersburgh* undertook a journey into *Siberia*; in order to inquire into the natural history of that country, and to make such experiments and observations, as might tend to give a just idea of that almost unknown region, and to the improvement of physics in general.

* See Part ii. Chap. ii. Art. vii.

† Vol. VIII. Part ii. Chap. iii. Art. i.

Dr John George Gmelin, Professor of Chemy and Natural History at *Petersburgh*, was sent at the head of this deputation, who, besides several of his colleagues, and some students, had a Painter or two, a Miner, Huntsman, and proper attendants in his retinue. He set out upon this expedition in *Aug.* 1733. and returned to *Petersburgh* in *Feb.* 1742. after having spent 9 whole years in visiting almost every part of *Siberia*.

Hist. Plant.
Sibiriae cum
tabulis æri-
incisis. *Auct.*
D. Gmelin.
Chem & Hist.
Nat. Prof.
Petropli
1747. 4to.
Vol. I. by
John Fother-
gill, M D.
Lic Coll. Med.
Lond. No.
486. p. 248.
Feb & Mar.
1748. Read
Feb. 11.
1747-8.

The fruits of this undertaking are designed to be communicated to the public; and one volume of the History of Plants has already appeared, under the title of *Flora Sibirica, &c.* This is intended to be followed by several others, containing not only a description of the plants, their *locus natalis*, &c. but their uses amongst the inhabitants, so far as the Professor could get information concerning them.

In a large preface to this first volume, the ingenious and indefatigable author has given us a concise account of *Siberia* in general, it's rivers, lakes, mountains, mines, the nature of the soil, fertility, &c. with several judicious experiments and remarks on the altitude of the earth above the level of the sea; but especially on the qualities of the air in that climate; an abstract whereof, at first drawn up for private entertainment, was thought not unworthy of more public notice, and is therefore addressed to the *Royal Society*.

The country, whose Natural History Dr Gmelin has collected, is of vast extent: it is bounded by a chain of mountains called the *Werchoturian* and *Vralian* on the W.; by the sea of *Kamtschatka* on the east; and comprehends all those countries that lie betwixt the *Mare glaciale*, and the borders of the *Kalmucks* and *Mongales*, to the very confines of *China*.

The rivers which water this tract are numerous; some of them large, and even receiving streams in their course, which in other countries would be looked upon as capitals themselves. The space they measure is no less considerable. The *Jaik* is the first river of note on the western side. It rises under the lat. of 54, of long. 78. and runs into the *Caspian* in 47 of lat. and 74 of long. The *Irtisch* rises in the country of the *Kalmucks*, lat. 46 $\frac{1}{2}$, long. 103; and empties itself into the *Oby*, lat. 61, long. 86. The *Oby* rises under 32 lat. 103 $\frac{1}{2}$ long.; and loses itself in the *Mare glaciale*, lat. 67, long. 86. after running a course of near 800 leagues, and receiving a great number of rivers of considerable note. The *Jenisea* is not much less than the *Oby*. The *Selenga* takes it's rise under lat. 48, long. 114; runs into the lake *Baical*, in 51° 20'' latitude, with many others equally considerable, which it would be tedious to mention. The water of these rivers is for the most part fresh, clear, and salubrious: in some it is a little brackish, by the mixture of currents from salt lakes and springs, which abound in many places: they contain fish of various kinds in great plenty, and mostly of an excellent flavour.

The lake *Baical* may deserve some mention to be made of it, being one of the greatest fresh-water lakes yet discovered: it extends, according

ing to our author, from 101° of long. to 127° , being upwards of 500 leagues in length, and is from 25 to 80 leagues in breadth. It is everywhere deep and navigable; the water is extremely clear; it abounds with great plenty of fine fish: it receives a great number of rivers, but the *Angara* alone runs out of it; which joining the *Tungusca*, loses its name; as this likewise does, when it runs into the *Jenisea*.

Salt lakes are common in many parts of *Siberia*; some contain a pure white salt, well-tasted, and fit for use; which, in summer, is crystallised by the heat of the sun alone, and forms a crust on the top of the lake. In some, this grows so heavy as to break, and fall to the bottom. Besides this kind of pure common salt, which is fit for use, there is another sort of a bitter taste, much resembling the *sal mirabile*, found in several lakes in this country. Springs of salt water are sometimes observed to rise in the midst of fresh water: our author assures us, that he has seen several such; one especially he observed rising through a stone, in the bed of the river *Angara*. On the banks of the river *Kaptendei*, where it runs into the *Wilvius*, are a great number of salt-springs, which afford excellent salt; and about 30 leagues above this place, along the same *Kaptendei*, on the right hand, is a hill about 30 fathom high, and 210 long, consisting intirely of *sal gem*. There are some lakes, which, our author informs us, in the memory of man, contained only fresh water, but are now very salt. One of this kind, about 40 years ago, abounded with fresh water fish, but is now become salt, smelling strong of sulphur, with a bitter taste, and all the fish are killed. The inhabitants assured our author, that some fresh-water lakes have been by degrees dried up, and that others have appeared, where formerly it was dry ground; and that even some of these new-formed lakes, which at first had no fish in them, are now very plentifully stocked. They have not recourse to subterranean caverns or passages, for a solution of this *phænomenon*; but assert, that ducks, sea-mews, &c. that live upon fish, carry the eggs from one lake to another.

In the description which our author gives us of the course of rivers, situation of lakes, &c. he takes notice of the soil, its barrenness, fertility, &c. These are different, as it may be supposed, in the different parts of such an extensive climate under such latitudes. About the lake *Baical* is the most fruitful tract, and thence is called the granary of that part of *Siberia*. They grow some little corn about the lat. of 61 . They have made of late trials still further; but the success was not known.

In his passage through *Siberia*, he tells us, that he could scarce think himself in *Asia*, till he got over the river *Jenisea*: till then, he saw no animals, but such as are common in *Europe*, at least may be seen in the plains washed by the lower part of the *Volga*: the plants and stones were of the same kind, and the face of the country in general, like other parts of northern *Europe*. But from the *Jenisea*, both to the E. N. and W. the climate seemed to be wholly different, and as if it were enlivened with new vigour. It is mountainous; but these mountains are inter-

mixed

mixed with rich delightful vallies, and fruitful plains. The animal that affords the musk, and the *musimon* of the Ancients, were now to be met with. Many of the most common *European* plants by degrees disappeared, and others became frequent, which are strangers in *Europe*. The purity, clearness, and salubrity of the waters, the exquisite taste of the fish and fowl, but more especially the different genius and way of life of the inhabitants, plainly proved they were got into another climate. This remark our author submits to the consideration of Geographers.

Amongst the curiosities of *Sibiria* the Professor mentions a place remarkable for it's excessive coldness in the midst of summer. It is in the province of *Jacutski*, about the middle way to *Ochotz* along the river *Junacan*; it is called by the *Russians* *springing ice*, by the natives the *icy lake*. Three other such places occur within the circuit of 80 leagues.

The provinces beyond the lake *Baical* are mountainous, with high and wide-extended plains lying betwixt them, which in many places are only covered with barren sand; so that in some places one may travel through such deserts one, two, or three days together, without finding wood enough to make a fire, or any other water than of salt springs, which are very frequent; and being dried up by the summer-heats, leave a saline crust, very much resembling *natron*, being of an alkaline nature, with a sulphureous smell.

The country that borders on the rivers *Urunca* and *Gasimur* is extremely rich and fruitful. The face of the country is delightful, and it's produce to the husbandman almost exceeding his hopes: but what renders it still more surprising, is, that a country, whose soil yields to few in fertility, and the beauty of it's bloom, should yet cover immense riches in it's bosom. Here are mines of gold and silver, which have long been worked to advantage: the veins are rich, and lie shallow; yet communicate no poisonous *effluvia* to the vegetables that cover them: nor do those distinguishing marks of sterility appear here, which in most other mining countries are so observable.

The highest part of *Sibiria* is towards the springs of the rivers *Argun*, *Schilca*, &c. about the 49th deg. of lat. 130th longit. This part is destitute of marble and lime-stone, which are almost every-where to be met with in the lower tracts both of *Sibiria* and *Russia*: no petrifications are to be found here, either of the testaceous or crustaceous animals: and the veins of ore are always found near the surface, never entering deep into the earth. Besides the mines of gold and silver above-mentioned, copper and iron are found in several places; likewise the *Glacies Mariæ* or *Muscovy Glass* is dug near the river *Mama*. Loadstones are also got in *Sibiria*; and in several of the rivers beautiful transparent pebbles and crystals occur.

I shall only add, that there are some natural warm baths in several parts of *Sibiria*, and some of them of a most agreeable temperature; and proceed to the account of our author's observations and experiments on the height of the earth, &c.

Pauda is allowed to be the highest of all that ridge of mountains called *Werkoturian*. Our author endeavoured to take the height of it by means of the barometer. Dec. 11. 1742. at our author's lodgings at the foot of *Pauda*, the mercury in the barometer, in a cold place, but within-doors, stood at $26 \frac{83}{100}$ Paris measure. He then carried it up the mountain as high as he could go, which was about $\frac{1}{3}$ of the whole height, where he hung up the barometer on a tree, from 9 to 11 in the forenoon, making a good fire pretty near it, lest the intense cold, which sunk the quicksilver in *De Lisle's* thermometer to 201, should affect the barometer, and lead him to ascribe that to gravity, which was only owing to the contraction of cold. Under these circumstances the quicksilver sunk to $25 \frac{32}{100}$. Hence, according to M. *Cassini's* calculation, our author's first station will be 941 feet higher than the level of the sea: the second on *Pauda* 1505 f. and the whole height of this mountain 4515, or 752 Paris toises; which, added to 941 feet, the height of his lodgings at the foot of *Pauda*, makes 5456 feet, or 909 toises, the height of *Pauda's* top above the sea; supposing the level of the sea to be 28 inches, as the Paris Academicians have fixed it: though this differs from observations made on the barometer at the sea-coast of *Kamtschatka* at *Bolcheretz*; where, from experiments made for above two years, the mean height of the mercury was 27 inches, $6 \frac{1}{2}$ lines. And at *Ochotz*, during a year's observations, the mean height was found to be 27 inches and about $8 \frac{1}{2}$ lines. Hence it would appear, that the sea of *Kamtschatka* is higher, with respect to the earth's centre, than the ocean and Mediterranean; and at *Bolcheretz* higher than at *Ochotski*.

The following list of barometrical observations, made in various parts of *Siberia*, will shew the different heights of the different tracts in it.

| | | Feet | Toises | Inches |
|--|---|---------------------------|--------|---------------------|
| The mean height of the barometer, from a year and 10 months observations at <i>Ircuts</i> , was | } | — | — | $26 \frac{83}{100}$ |
| | | 1355 or 226 | — | — |
| It's height above the sea will then be | | — | — | — |
| At <i>Selengia</i> , 1 month's observations, | | — | — | $25 \frac{95}{100}$ |
| It's height above the sea | | 1779 or 296 | — | — |
| At <i>Kiachta</i> , a town on the confines of <i>China</i> 12 days observations in <i>April</i> and <i>May</i> , mean height | } | — | — | $25 \frac{35}{100}$ |
| It's height | | 2400 or 400* | — | — |
| At <i>Nertschia</i> , from 20 days observations in <i>June</i> , | | — | — | $25 \frac{99}{100}$ |
| The height above the sea | | 1738 or 298 | — | — |
| At the silver-mines at <i>Argun</i> 9 days in <i>July</i> , | | — | — | $25 \frac{62}{100}$ |
| The height above the sea | | 2121 or 353 $\frac{1}{2}$ | — | — |

* In the copy before me appears to be a great mistake, either of the Printer, or in the manuscript; it being put down in words at length, *bis mille quadringentorum Orgyarum cum dimidia*; which is impossible; and the number of feet is not exact, according to other calculations.

Our author adds several judicious reflections upon the time and manner of making these observations, in order to determine any thing with certainty ; which he has endeavoured to keep strictly to in these experiments ; and concludes, that the plains in some parts beyond the lake *Baical*, are almost as high as the tops of high mountains in some other countries ; mount *Massane*, according to the *French* Geometricians, being but about 408 toises high ; which differs but little from the plain country at *Kiachta* ; which yet has considerable mountains rising in it's neighbourhood.

From whence our author concludes, that the elevation of the earth, in this tract, above the level of the sea, is very great, compared with the W. part of *Sibiria* and *Europe* *.

The air of *Sibiria*, with respect to it's gravity, is, as in other countries, the nearer the sea the heavier ; and the more remote, the lighter : so that at *Kiachta* scarce one person in our author's retinue escaped without some indisposition : they were seized after their arrival, some with acute fevers, others complained of extreme lassitude and dejection. It was in the spring season, the weather moderate, their manner of living regular, nor had they been much fatigued with their journey ; in short, they could attribute it to no other cause than the lightness of the air.

In these provinces, viz. beyond the lake *Baical*, our author tells us, that intermittents are seldom heard of, and ophthalmies are endemic : but that, in the fenny tracts which lie near the *Oby* and *Jenisea*, intermitting fevers are very frequent.

The coldness of the air of *Sibiria* is of all others the most remarkable quality. In some places it snows frequently in *September*, and not seldom in *May* : In *Jacutsk*, if the corn is not ready to cut in *Aug.* which often is the case, the snow sometimes prevents it, and buries the harvest all together. At *Jacutsk* the Professor ordered a hole to be dug in the earth, in a high open place, on the 18th of *June* ; the mold was 11 inches deep ; below that was sand about $2\frac{1}{2}$ feet ; it then began to feel hard, and in half a foot more it was froze as hard as possible. In a lower place, at no great distance from this, he ordered another hole to be dug : the soil was 10 inches ; soft sand 2 feet 4 inches ; below this,

* M. De la Condamine, in his voyage through the inland part of *S. America*, makes *Quito* to be between 14 and 1500 toises above the level of the sea. Suppose 1450
He tells us, that *Pichincha* is 750 higher

| | | | |
|-----------------------------|------|------|------------|
| ———— | ———— | ———— | 750 |
| This makes in the whole | | | 2200 Tois. |
| above the level of the sea. | | | |

P. Martel, Engineer, in his account of the *Glacieres* in *Savoy*, printed at *London* 1742, tells us, that the barometer at *Geneva*, by the side of the *Rhone*, stood at $27\frac{2}{12}$ I. which is 656 feet above the level of the sea according to *Schenzer* ; and that the highest point of *Mont Blanc*, measured partly by the barometer, and where inaccessible from the snow that covers it, by trigonometrical operations, is 12459 feet, or somewhat more than 2076 toises above the level of the *Rhone* ; which, added to the height of this above the sea, makes 13115 *French* feet, or about two *English* miles and two thirds.

all was congealed ; so that the earth is scarcely thawed even in summer above 4 feet deep.

Our author inclines to the received opinion, that the eastern climates under the same latitude are colder than the western ; and thinks this is confirmed by experiments made in different parts of *Sibiria*.

The mercury in *De Lisle's* thermometer often sunk in winter in very southern parts of this country, as near *Selinga*, to near 226, which is equal to $55\frac{1}{2}$ below 0 in *Fahrenheit's* thermometer. But the cold is often much more intense than this, as appears by the following experiments, made at *Kirenginski*.

Feb. 10. 1738. at 8 in the morning the mercury stood at 240° in *De Lisle* ; which is 72 below 0. in *Fahrenheit's*. On the 20th it sunk one degree.

At the same place in 1736.

Decemb. 11. at 3 in the afternoon 254 in *De Lisle*.

Almost 90 below 0. in *Fahrenheit*.

Decemb. 20. 4 o' clock p. m. 263 in *De Lisle*.

99 $\frac{44}{100}$ below 0. in *Fahrenheit*.

D. F.

Novemb. 27. 12 at noon 270 = 107 $\frac{73}{100}$ below 0.

Jan 9. 275 = 113 $\frac{65}{100}$

1735 *Jan.* 5. 5 in the morn. 260

6 — 280 = 120

8 — 250 and rose by degrees till 11 at night,

when it stood at 252.

Such an excess of cold could scarcely have been supposed to exist, had not experiments, made with the greatest exactness, demonstrated the reality of it.

During this extreme frost at *Jenisea*, the magpies and sparrows dropped down as they flew, and to all appearance dead ; though they most recovered when brought into a warm room. This was quite new to the inhabitants of that country ; tho' it frequently happens in *Germany* in much less intense cold, when the weather sets in at once very severe. The air, says our author, was at that time extremely unpleasant ; it seemed as if itself was frozen, being dark and hazy ; and it was scarce possible even to bear the cold in the door-way for 3 or 4 minutes.

These experiments, our author assures us, were made with all possible exactness, and agree with many others, made in different parts of *Sibiria* by his direction ; and from these we may conclude that the cold in *Sibiria* is more intense than it has yet been found to be in any other part of the world.

It was not apprehended that a greater degree of cold existed anywhere, than that artificial one produced by *Boerhaave*, by means of concentrated spirit of nitre, which sunk the mercury 40° below 0. in *Fahrenheit's* ; which was supposed to be the point beyond which no animal could bear it.

But

But the utmost limits of cold are yet unknown; or to what degree an animal can subsist in it, when inured to it by little and little. The history of heat is alike imperfect. The celebrated Professor abovementioned was induced to think, that a man could not bear, without the utmost danger, a greater heat than that which would raise the mercury to 90 in *Fahrenheit's*; but an ingenious and accurate correspondent of our author's at *Astrachan* informs him, that it not only rises there to this degree frequently, but even to 100, and he has seen it $103\frac{1}{2}$. Even in the bagnio's in *Russia*, the heat is often equal to 100: it sometimes makes the quicksilver ascend to 108, 110, and to 116, as may be tried every day; and yet people not only bear them with impunity a few minutes, but often stay half an hour or an hour.

One necessary observation our author makes, which is, that the ball or tube containing the mercury ought to be as dry as possible on the outside, during these or any other trials with the thermometer: for the adhering moisture, by forming a cooler atmosphere around it, has sometimes occasioned a difference of 10° .

These are some principal facts given us by our author in his Preface, relative to the Natural History of *Siberia* in general: what follows chiefly regards the work it is prefixed to. As a just idea of this part cannot be exhibited in a narrow compass, the curious in this branch of science must be referred to the book itself.

I have only to acknowledge with gratitude the instruction and entertainment I have received from this elaborate work: it is a tribute justly due to the learned and ingenious author, in return for the pains he has taken, and the fatigue he has endured in this inhospitable region; and to intreat your indulgence, if I have flattered myself too much, in apprehending this excerpt might afford you some amusement.

CHAP. III.

MISCELLANEOUS PAPERS.

I. **T**HE first step is, to take a perfect and sharp impression, in black sealing-wax, of the coin or medal you desire the picture or figure of: when this is done, the chief trouble is over, and the rest of the operation may be executed at leisure.

Cut the wax away round the edges of the impression, with the point of a penknife, or a pair of sharp scissars; and, having ready a preparation in gum-water of the colour you would have the picture, spread your paint upon the wax impression with a small hair-pencil, observing to work it into all the sinking and hollow places, those being the rising or projecting parts of the medal, and what only are necessary to be laid

An easy method of procuring the true impression or figure of medals, coins, &c. by Henry Baker, F.R.S. N^o. 472 p. 77. Jan. &c. 1744. Read April 19. over 1744.

over with the colouring ; for it must be intirely taken away from every other part before we can proceed.

The way of getting off the paint from the places where it should not be, is, to moisten your fore-finger a little, but not too much, with spittle or water, and pass it gently, but nimbly, over the surface of the wax impression ; wiping it each time upon a cloth, till you perceive all the rising parts of it perfectly fair and clean, and the letters and sinking parts of it only coloured.

This done, take a piece of very thin post-paper, a little larger than the medal ; wet it till it be moistened quite through, but let not any water hang upon it : place it on the wax impression, laying on the back of the paper 3 or 4 pieces of thick woolen cloth, or flannel, about the size thereof.

I should premise, that you must have a couple of flat smooth iron plates, about 2 inches square, and $\frac{1}{10}$ of an inch in thickness. The wax impression must be placed, with it's face upwards, on the middle of one of these plates, before you spread the paper and flannels on it, and the other plate must immediately be laid over them : then, holding all tight together, put them carefully and evenly into a little press, made of 2 iron planks about $5\frac{1}{2}$ inches long, one inch and $\frac{1}{2}$ wide, and $\frac{1}{2}$ an inch in thickness (shaped like what book-binders use of a large size in wood), having a couple of long male screws that run through them, with a turning female screw on each to force the planks together : and these female screws must have strong shoulders, whereby to work them. But all this will be comprehended better by the figure hereto annexed. See Fig. 48.

Fig. 48.

Things being thus adjusted, hold the press in your left-hand, and, with a little hammer, strike first on the shoulders of one screw, and then on the shoulders of the other, to bring the planks together parallel, and render the pressure every-where alike ; unless you find it requisite to give more force to one side than the other, which these two screws will put in your own power.

The press opens again, by a stroke or two of the hammer, the contrary way, on the shoulders of the screws : and then you will find a true and fair picture neatly printed off ; which (if any deficiencies appear therein) you may easily repair, when dry, with a hair-pencil, or a pen, and a little of the same colour.

If your paper does not soak in the moisture well, by being over-sized, it is necessary to wet the flannels, or the paper will not come off strongly enough coloured : and, if the *relievo* of your medal be very high, it is best to put a little cotton immediately upon the back of the paper, between that and the flannels, that the paper may be duly pressed into the deep hollows of the wax mould.

This method is very easy and ready for taking the picture of a medal in any colour : but, if you desire a *relievo* only, without any colour, the way is abundantly shorter ; for nothing then is necessary, but to place a
piece

piece of card, or white pasteboard, well soaked in water, on the wax mould, without any colouring, and letting it remain in your press a few minutes, the business is done at once.

As it is plain, from what has been said, that the whole success depends on the goodness of the wax impression or mould, I shall lay down some rules for taking it, which much experience has taught me.

1. The wax must be very fine; or it wants a proper hardness, and the impression will not prove sharp.
2. It must be spread wider than the medal, and of a thickness in proportion to the *relievo* of it.
3. The medal must be clapped on when the wax has a right degree of heat: for, if it be too hot, the medal is apt to stick; and, if too cold, no good impression can be taken. I observe the best time to be, just after the wax ceases to work up, and have little bubbles in it.
4. Take not the impression on a table, or any hard body, without a sheet or two of paper, or, what is better, a woollen cloth, underneath; for that gives way to the pressure and form of the medal, which hard bodies will not do.
5. The medal should be squeezed down equally hard on every side, and the pressure continued till the wax is near cold: for, if you lift up the medal while the wax remains hot, the rising parts, being still soft, sink down, and become much less sharp.
6. The best paper for your wax impressions is white pasteboard, card-paper, or some other thick paper.

The pictures may be coloured as every one fancies. I have done them in most colours, but think a red the best; which was the reason I advised the impression to be taken in black wax; since the wax and paint must be of different colours, or it will be impossible to distinguish when the colour is laid on properly, or rightly cleared away. Therefore, if the pictures are chosen in black and white, to resemble copper-plates, the wax must not be black, but red.

The red colouring I use is a mixture of lake and vermilion, which works off more kindly than either of them alone.

Gamboge makes a good yellow print, and appears very lively in the day-time, but can scarce be seen by candle-light: mixed with carmine, it affords a much better colour.

Burnt umber affords a brown; but is more agreeable, if tintured with a little lake.

Blue may be composed of verditer and indico; but is troublesome to clear off; and, after all, has but an indifferent effect.

Green is likewise, in my opinion, not very agreeable; but, if desired, sap-green serves the purpose best.

Indian ink makes the best black; and affords pictures very like those from copper-plates.

A method of taking the true impression of medals, &c.

All these colours, except gamboge, sap-green, and *Indian* ink, must be ground extremely fine, with gum-water of an exact strength: for, if there be too much gum, the colouring will not easily be cleared away from the wax impression, nor readily come off upon the paper; and, if too little, every touch will spoil your picture, after you have worked it off. Some few trials will teach the proper temper it ought to have.

If you would have the hair, face, robes, or any parts of the print to be of different colours, you must spread them so on your wax impression. I have worked off many in that manner, which I have here to shew; but, as it is much more troublesome to lay on the colours thus, and requires a good deal of finishing with the pencil afterwards, I think it also less proper for the picture of a medal, than one single colour.

I am not unacquainted with many ingenious inventions for taking off medals, in sulphur, plaster of *Paris*, paper, &c.; but, since a mould must be formed for each of these, either of clay, horn, plaster of *Paris*, or some other materials, which requires a great deal of trouble and time to form, I believe this way will be judged abundantly more convenient: and taking impressions on paper from the medals themselves, by passing them through the rolling-press, and colouring them afterwards, is not only much more tedious, ineffectual, and less practicable (as a Gentleman cannot manage a rolling-press), but does really a great deal of injury to the medals, by impairing the sharpness of their most delicate and expressive strokes; as I myself have found by repeated experience.

But wax is always ready, and hurts not the finest medal in the least degree; and, however brittle it may be supposed, the moulds made thereof resist the force of downright pressure, almost as effectually as if they were made of steel; and might serve to take off a thousand impressions, were they not apt to crack, and the marks of those cracks to render what are taken from them afterwards, not quite so elegant: but each mould will usually afford 3 or 4 good impressions, either coloured or plain. And, when they are done with, if the paint is washed off clean, the same wax may be melted and employed again several times over.

It is evident, that impressions taken thus, must be exactly what the medals are from whence we take them, and that any-body who can borrow medals from his friends, may, in this manner, at the expence of a little pains, procure a noble collection of genuine prints of medals, which may be placed in books, in orderly series, and moved from one leaf to another at pleasure, if they are only pasted down by little slips of paper left round the edges for that purpose.

N. B. Instead of the water-colours herein mentioned, I have lately made use of the mixture (both red and black) which is usually employed by the rolling-press Printers in working off copper-plates, with very good effect: but when this is used, after it has been wiped off with a linen cloth, it is necessary to clear it perfectly from the ground of the wax impression, by rubbing one of your fingers on

on a piece of whiting, and passing it two or three times over your impression, in the same manner as copper-plates are cleared.

II. 1. The curious in this country talk much of a phænomenon, which is called of the *Bologna* bottle, because it was first discovered at *Bologna*. If you let these bottles fall perpendicularly from some height upon a brick floor, they will not be broken; but if you drop into them some little hard bodies, they will burst in pieces.

I took one of these glass bottles, whose form resembles a *Florence* flask, and whose capacity is about $\frac{3}{4}$ of a pint, and let it fall down from the height of $5\frac{1}{2}$ feet upon a floor of brick, and it was not broken: I then let fall down into it, from the mouth to the bottom internally, a piece of flint-stone, weighing 11 grains; and immediately the bottle burst into many pieces.

I took one of those pieces, weighing 3j, and let it fall in the same manner into another bottle, which I moved circularly for a minute; and then putting it upon a table, in about $\frac{1}{4}$ of an hour it broke in pieces.

Into a third bottle I dropped a piece of whetstone, weighing 40 grains; and in some few minutes the bottle was broken.

I filled another bottle half-full of water, and let fall into it a small piece of flint stone; and after four hours it burst.

I let fall into 3 other bottles a piece of wood weighing 50 grains, a piece of brass weighing 300 grains, and a ball of lead weighing 140 grains; and neither of them was broken.

These bottles are thicker at the bottom than the neck. The glass-maker blows them, and lets them cool, without putting them again into the oven. And, from the experiments, I take notice, that what is capable of breaking them ought to have some roughness: and I am told that a grain of river-sand will break them.

2. The following paper contains the account of several experiments of an odd nature, that have lately been tried both in *Italy* and in *Holland*, upon some unannealed glass phials; that is to say, such as have been exposed to the air as soon as blown, without passing through the operation that is commonly called annealing.

The excessive fragility of these sorts of glasses must have been observed, as long as the art of making glass has been in use: it having been found, that almost all the vessels that were made of such glass were entirely useless upon that account; as being subject to break and fly, almost constantly, of themselves, and that even frequently before they were well cold.

It was therefore to remedy this inconveniency that the practice of nealing or annealing them was devised; whereby, passing very gradually, in the space of some hours, through what is called the leer, from a very intense degree of heat to the temperature of the common air, they were found to acquire such a toughness or tenacity, as fitted them for the several uses for which they were respectively designed.

But

Concerning
the Bologna
bottles; in a
letter from
Dr Josephus
Laurentius
Bruni of Tu-
rin, F. R. S.
to Mr Henry
Baker, F. R. S.
N^o. 475. p.
272. Jan &c.
1745. Read
Jan. 31.
1744 5.

An account of
some experi-
ments, lately
made in Hol-
land, upon the
fragility of
unannealed
glass vessels;
communicated
to the Pres.
N^o. 477. p.
505. Aug. &c.
1745. Read
Oct. 31. 1745.

But some of the *phænomena* depending upon their first brittleness, or at least very nearly connected with it, have been often judged to deserve the attention of the curious. One of the first very worthy founders of the R. S. the R. Hon. Sir *Rob. Moray*, very early gave in his experiments, which appear in the register, upon those drops or *lachrymæ* of glass, which, instead of being nealed, had been immediately quenched in water, or some other fluid. And the same learned person further observed, that hollow balls, made of unnealed glass with a small hole in them, would flie in pieces with the heat of the hand only, if the small hole, by which the internal and external air communicated, but was stopped with the finger.

The glasses which the following paper concerns, have been already mentioned to the *Society* by Mr *Baker*; * and Mr *Ellicot*, having very soon after caused some unnealed glasses to be made here, repeated with them some of the same experiments, which he found to answer agreeably to what Dr *Bruni* had mentioned.

But it will further appear to be remarkable in the present paper, that, according to the experiments made abroad upon those glasses, it is not the weight alone of the bodies severally dropped into them, which occasions their rupture; for some certain bodies break them with abundantly more ease than others of the same or even much greater weights: insomuch that such phials as are shivered to pieces by the fall of very small particles of flint and some other substances, are nevertheless capable of resisting the much greater shock they receive, in like manner, from a leaden bullet, though some hundreds of times heavier than the flint.

The author of the paper is M. *Allamand*, a Gentleman of distinction, merit, and learning, in *Holland*, a person of great curiosity, and particularly well versed in all the parts of natural and experimental knowledge. This gentleman communicated his observations to the Hon. *William Bentinck*, Esq; of the *Hague*, F. R. S. who was pleased immediately not only to transmit them over to the *Pres.* but also to oblige him at the same time, with a number of glass phials, of the very same sort as those upon which M. *Allamand*'s experiments had been made; that he might thereby be enabled both to report to the *Society* the facts he should take notice of, and to repeat, some of the experiments themselves in their presence.

M. *Allamand*'s paper is in *French*; but the substance of it in *English* is as follows.

These glasses have been known some time, and an account has already been given of them in a Dissertation printed at *Padua* in 1743: the extract of which dissertation, published in the *Leipsic Acts* for Feb. last, gave me also the curiosity to repeat in *Holland* the same experiments that had been already made both in *Italy* and some other places.

Experiments
made upon
glass phials,
which break
with the
stroke of cer-

* See the preceding paper.

These glasses only differ from ordinary phials in this, that they have ^{tain bodies ;} not been set to cool gradually in what is called the nealing furnace, but ^{but resist the} have been immediately exposed to the open air as soon as formed. They ^{shocks of} may be made of any shape : I have had some cylindrical with a flat bottom, others of the figure of a common drinking glass, others that were ^{others, though} conical, and others again elliptic. The experiments have equally well succeeded upon all these several glasses ; and all that needs to be observed in the making of them is, to take care that their bottoms may be thicker than their sides : and, indeed, the thicker the bottom is, the easier do the glasses break. I had one particularly, whose bottom was above 3 fingers breadth in thickness, and that flew with as much ease at least as the thinnest glass. I have had some others equally thick all over ; these have flown also, but with more difficulty than the others. ^{much more ponderous.}

These glasses are capable of resisting very hard blows coming from without : I have given to some, with a mallet, strokes sufficient to drive a nail into wood tolerably hard, and they have held good without breaking. They also resist the shock of several heavy bodies, that are let fall into their cavities. Thus I have dropped, from the height of 2 or 3 feet, musket-balls, pieces of iron, brass, tin, silver, gold, antimony, bismuth, pyrites, jasper, and several sorts of woods, ivory, and bone : all which is indeed no-ways extraordinary ; for other glasses equally thick would also bear the strokes of the same bodies ; but herein consists what is more surprizing.

I took a shiver of flint, of the size of a small pea ; I let it fall into the glass from the height of 3 inches ; and in about 2'' the glass flew. And having repeated the same experiment upon several other glasses with the same piece of flint, the greatest part broke in the moment of the shock, and the others 1 or 2'' after it.

I have let fall into different glasses a shiver of flint, of but half the size of that used in the former experiment, and the glasses flew in the same manner.

Another bit of flint, of the size of a small lentil, has also produced the same effect.

Being encouraged with this success, I let fall into one of my glasses a piece of flint no larger than a grain of sand : this was too light to produce any sensible shock, and accordingly the glass did not break. In order to try further, I shook the glass with the small piece of stone in it ; and nothing following, I repeated the same experiment upon 4 other glasses, none of which broke. I then judged my experiment to have failed, and set by those 5 glasses ; but, about $\frac{1}{2}$ an hour after, one of those glasses flew, and the other 4 soon after ; insomuch that the glass which remained the longest entire broke also, about $\frac{3}{4}$ of an hour after it's being shaken.

Tho' flint is, of all the bodies that I have employed, that which has hitherto broken these glasses with the greatest ease, it is not however the only body that produces this *phenomenon*.

I let fall into one a sapphire set in a ring; and tho' the bottom of the glass was near an inch in thickness, the sapphire passed thro' it as thro' a spider's web. The glass was dispersed on all sides, and the ring remained upon the table just where the glass rested.

A bit of porcelane, of the thickness of $\frac{1}{2}$ a line, and the breadth of 2 lines, broke also several glasses; but that only some seconds after the shock.

A bit of glass, of the same size, produced the same effect; and so did a very small pebble.

Diamonds of several sizes have constantly done the same.

A very small piece of hard-tempered steel has broken all the glasses into which I have dropped it.

One of those pellets also that boys call marbles, broke a glass into which it was dropped; but not till 4' after it's fall.

Being desirous to know if the bodies upon which I rested my glasses contributed any thing to the ease of their breaking, I repeated the same experiments, holding the glasses in my hand, setting them upright in clay, placing them on a down-pillow, and putting them in water; in all which cases they broke in the very same manner. I then half filled one of them with water, and a piece of flint, about the size of a pea, broke it.

All the bodies with which I had yet broken glasses having been elastic without being ductile, I was willing to inquire, if those qualities were essentially necessary, tho' I was already satisfied, that all the bodies that had those qualities, such as ivory, for example, would not produce the effect. After many trials, none of which succeeded, I thought of slightly rubbing the bottoms of some of the glasses with my finger, and all those upon which I made that experiment broke; though some of them did not fly till above $\frac{1}{2}$ an hour after they had been so rubbed. Thinking, that perhaps the heat I communicated to them with my hand might occasion their breaking, to examine whether it was so or not, I poured into several some almost boiling water, which certainly gave them a much greater heat than I could have given them with my hand; but none of those glasses broke.

I have found in the animal kingdom but one sort of bodies capable of breaking these glasses, which are pearls: I dropped one of near a line diameter into a glass, and that glass broke in about $\frac{1}{2}$ an hour.

Though the experiment of rubbing with my finger had convinced me, that the stroke or shock of a falling body is not always necessary to break these phials, I thought of scratching with a flint the bottom of the glass, and the glass immediately broke. To assure myself whether the scratch I had made was the occasion of it's breaking, I took a rod of iron whose end was rounded; I pushed it strongly against the bottom of the glass, and the glass flew. I then did the same, and even pushed much harder, against the bottoms of several ordinary glasses, but with-

out

out any effect: for tho' these glasses were much thinner than the others, yet none of them stirred.

If the glasses in question are every where extremely thin, they do not break in the circumstances above-mentioned; I have frequently dropped into such glasses the same sorts of bodies as had broken the thicker ones, but without any success. I have only met with one that split: and I am not even sure but that the weight of the body dropped into it, which was a stone of some size, might occasion it's breaking.

All the phials upon which I have yet made these experiments were of white glass: I have not had an opportunity of trying those made of the green.

The author of the dissertation, published at *Padua* upon this subject, pretends to account for all these singular *phænomena* by saying: that the bodies dropped into these phials cause a concussion that is stronger than the cohesion of the parts of the glass; and that consequently, a rupture of the same must ensue. But why does not a ball of gold, silver, iron, copper, or any of the other bodies which I have tried unsuccessfully, tho' 1000 times heavier, equally cause this concussion, and break the glasses? Shall it be said, It is because they are not elastic? Copper, iron, silver, and ivory, are elastic; and as much so as flint and porcelain; and surely much more so than the end of one's finger.

It appears to me, that, before we undertake to give the solution of these *phænomena*, we should apply ourselves to the making a much greater variety of experiments about them; that we should both try a greater number of glasses, and those with a greater variety of differing bodies, that we may be able thence to collect at last, in what classes the several bodies are to be ranged, that are either fit or unfit for these purposes: and then it may, perhaps, be time to inquire, Whether it is from the principles of Chemistry, or from those of Mechanics, or any other branch of Natural Philosophy, that we are to seek for the reasons of the several facts.

After the reading of this paper, the *President* produced before the *Society* several of the phials themselves, which he had received from *Holland*, together with some others he had caused to be made at Mr *Ceile's* glass-house in *White-Fryars*. He acquainted the company, that he had yet made himself but few trials of these glasses, as he was desirous to preserve them till he could have the satisfaction of shewing them to the *Society*, and repeating some of M. *Allamand's* chief experiments in their presence: that he had designedly broken only 4 of his foreign phials; in all which matters had succeeded agreeably to the foregoing relation; and that a fifth, into which he had dropped a leaden bullet, had flown of itself, about $\frac{1}{2}$ of an hour after; but whether from any operation of the bullet, or from the heat of the room only, he could not take upon himself to determine.

After this, he proceeded to make a good number of experiments upon the phials; by all which it appeared,

That none of them, either foreign or *English*, were broken by the shock of a leaden bullet weighing 12 pennyweight: tho' the same was let fall from the height of above 2 feet.

That all the foreign phials, and several of the *English*, into which a shiver of flint, of the weight of 3 grains, was let fall, and that from a height of only 2 inches, were broken; most of them instantaneously, and the others within 2 or 3".

That several of them were broken with a small shiver of their own broken glass; but that, generally, this operation was not so quick, the phials sometimes not flying till 2 or 3' after the shock.

That several of them were shattered immediately, by the shock of a small piece of hardened steel broken off from a steel rod of about $\frac{1}{8}$ of an inch in thickness.

That of two phials, into which a boy's marble was dropped, the one broke presently, and the other not till 3' or 4' afterwards.

That of the two phials rubbed with the finger on the inside of the hollow, the one did not break, but the other did, about 5' after.

That one of them did not break with the fall of a rough *Cornish* diamond into it, though the experiment was twice repeated; but that the same afterwards was immediately broken by the flint.

That the trials made upon *English* glasses, though, seemingly, of the same sort, did not succeed quite so well, and so certainly, as those made upon the foreign ones; which were, probably, more suddenly cooled, as the workmen already acquainted with their properties, may be more expert in their management of them.

Upon the whole, all the company were perfectly satisfied of the truth and exactness of M. *Allamand's* experiments; and ordered their thanks to be recorded both to that gentleman and Mr *Bentinck*, for these very curious communications.

M. *Allamand* observes, that he had yet only tried these experiments upon phials made of white or crystal glass. But the *Pres.* since received from the Rev. Dr *Littleton*, F. R. S. some large hollow cups, made at *Worcester*, of the common green bottle glass; all of which, tho' of a much greater size than the others, and some of them above 3 inches thick at bottom, were instantly broken with a shiver of flint weighing but about 2 grains; tho' they had before resisted the shock of a musket ball from the height of near 3 feet.

N. B. That all the foreign glasses mentioned in this paper were nearly of the shape represented in Fig. 49. and about 4 inches in height.

An account
of glasses of a
new contri-
vance, for
preserving

III 1. Those who have collections of Anatomy, or Natural History, and preserve animals, vegetables, or any of their parts, in spirituous liquors, are sensible how expensive it is to supply the waste of these spirits, which evaporate considerably.

Having in 1739, begun to make a collection of this sort, it was not long before I felt the necessity I was under of contriving some kind of vessel, or some way of closing the common glasses, which might prevent, or at least diminish the quantity of, this evaporation. After trying several methods, which did not fully satisfy me, I fixed upon the glass, which is represented in *Fig. 50.* and *51.*

It's difference from the common ones consist in having, quite round the edge of it's orifice, a circular groove or chanel, *Fig. 51. AA*, an inch deep for the smaller sizes, and 2 inches for the larger. This circular groove is intended to receive a border, *BB*, of the same figure on the cover *C*: in the middle of the concavity of this cover is a double hook, on which the threads are to be fastened, which suspend the piece or preparation that is to be put into the glass.

In putting the piece into the glass, which contains the preserving liquor, care must be taken to place it on the hooks in the same position, which you intend it should keep in the glass: that done, you are to pour oil, or quicksilver, which is better, into the circular groove *AA*, so as to make it about half-full. Then you are to let down the piece into the preserving liquor; and when it is intirely sunk down, the cover *C* ought to light upon the glass, and it's circular border *BB*, enter into the circular groove *AA*, where it falls into the oil or quicksilver, which rises up and fills the whole groove; by which means the glass is in some measure hermetically sealed. The oil indeed permits some small evaporation; the quicksilver more completely answers the end of this invention, but it is more expensive.

As I had contrived this glass for my own private use, I neglected making it public; till, happening to be present at the public meeting of the *Acad. of Sc. of Paris*, at *Easter 1746*, I heard *Mr de Reaumur* read a memoir on this subject. My glass seemed to me vastly preferable to the vessels proposed by that great Academician. This incident roused my emulation, and gave me the better opinion of my glass, a pattern of which I sent to *M. Morand*. The same motive engages me to take the advantage of the freedom of correspondence re-established between *France* and *England*, in sending this description to the *R. S.*

2. In using the bottle, of which I have given a description, I found that it was attended with one inconvenience.

The circular groove, the edge of which was turned up on the inside of the bottle, retained a small quantity of the water, when I emptied and rinsed it; so that it was impossible to do it thoroughly.

Wherefore I caused this vessel to be made, as represented in the annexed figure, *Fig. 50.* which is a section of it; and where it appears that the groove *AA* is placed on the outside of the edge of the bottle. By this means every drop of the contained fluid can be poured out.

It is easy to see that the circular border *BB* of the cover is to enter into these grooves *AA*, which are filled with oil or quicksilver; and that

pieces of Anatomy or Nat. Hist. in spirituous liquors; by Claud. Nic. le Cat. M. D. F. R. S. R. Demonstrator in Anat. and Surgery at Roan Translated from the French by T. S. M. D. F. R. S. N^o. 491. p. 6. Jan. &c 1749. Read Jan. 19. 1748. Fig. 50, 51.

Addition to the preceding paper; by the same. Ibid. p. 88.

that the hook C, of the same cover, is destined to suspend in the liquor such pieces as are to be preserved in it.

*A letter from
Dr John Lining, at
Charles Town
in South Carolina, to
James Jurin,
M. D. Coll.
Med. & Reg.
Soc. Sodal.
serving to accompany some
additions to
his Statical
Experiments
printed in N^o.
470, of these
Transactions.
N^o. 475. P.
318. Jan &c
1745 Dated
South Carolina, Charles-
Town, Jan.
29. 1743.
Read Mar 21.
1744-5.*

IV. The first, second, and third tables * are the same with what I sent you at first †: and in the third I have supposed the *ingesta* each month to be $\bar{3}3000$, to coincide with two columns in my *Sanctorian* tables, where I have daily supposed the *ingesta* to be $\bar{3}100$. This answers to TAB. V. but I shall here give it anew.

The mean quantities contained in the fifth table, were obtained by a tedious operose calculation; namely, by calculating each day's mean diurnal and nocturnal urine and perspiration of one hour in every month; and by dividing the sum of each month's mean diurnal perspiration, &c. by the number of days in each month. The mean diurnal and nocturnal heat by *Fahrenheit's* thermometer were taken by the same method.

The deductions from the table I have digested all that I could of them tabularly, that they might be as clear and short as possible, and appear to me to point out the physical principles, from whence we may account for the production of these epidemic diseases of the different seasons, which are not infectious. For are not these the effects of different constitutions of the air on human bodies? And are not the increments and decrements of the sensible and insensible excretions, regard at the same time being had to the quantity and quality of the *ingesta*, and to the exercise, &c. the only *index* of the changes produced in the human constitution, by the vicissitudes of the weather?

That indeed was the only view I had in going through these troublesome experiments with so great assiduity for one whole year. The design was benevolent, and I am afraid far superior to my capacity, especially as I am situated in a place where I can have no immediate assistance.

That I may be furnished with as many *data* as possible, I propose to take the specific gravity of the *cruor*, of the *serum* and *crassamentum* of the blood, in different diseases, and in their several *stadia*, by a very nice hydrostatic balance, made by Mr *Jackson*. But this indeed is attended with greater difficulties than I was at first apprised of; for the experiment requires a greater quantity of blood than can at all times be safely taken away; and rain-water, with which the specific gravity of the blood is compared, I have found, by repeated experiments, to lose about $\frac{3}{512}$ parts of a grain for each degree of heat by *Fahrenheit's* thermometer; and oil of turpentine, in which the *crassamentum* is weighed, loses much more of it's specific gravity.

If I can possibly obtain the same kind of instruments which Dr *Langrish* used, I would likewise take the cohesion of the blood, and analyse it and the urine in different diseases, &c. but the difficulty of getting exact instruments is very great.

* Therefore omitted here.

† See Vol. IX. Part iv. Chap. iii. Art. i.

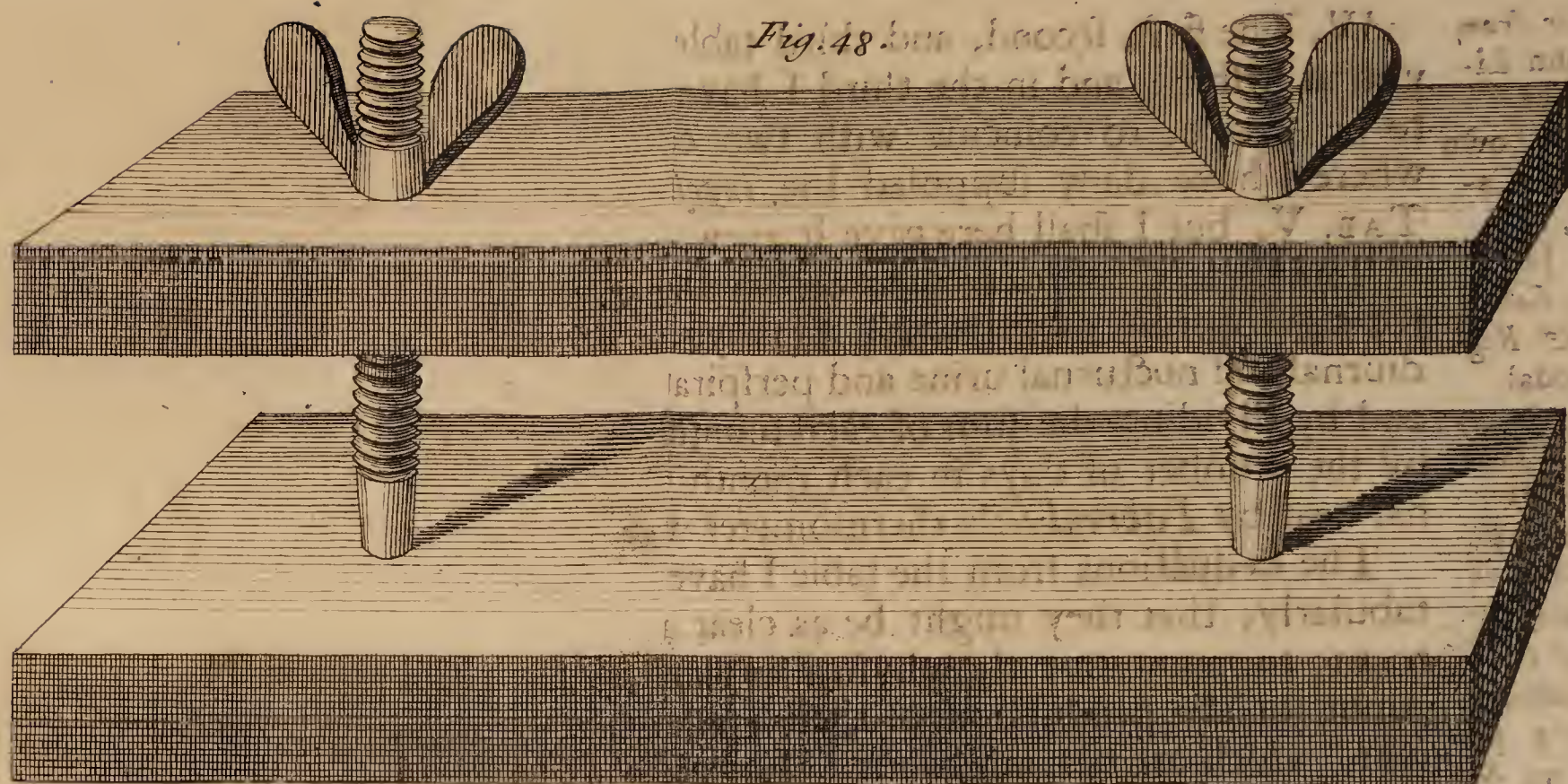


Fig. 48.

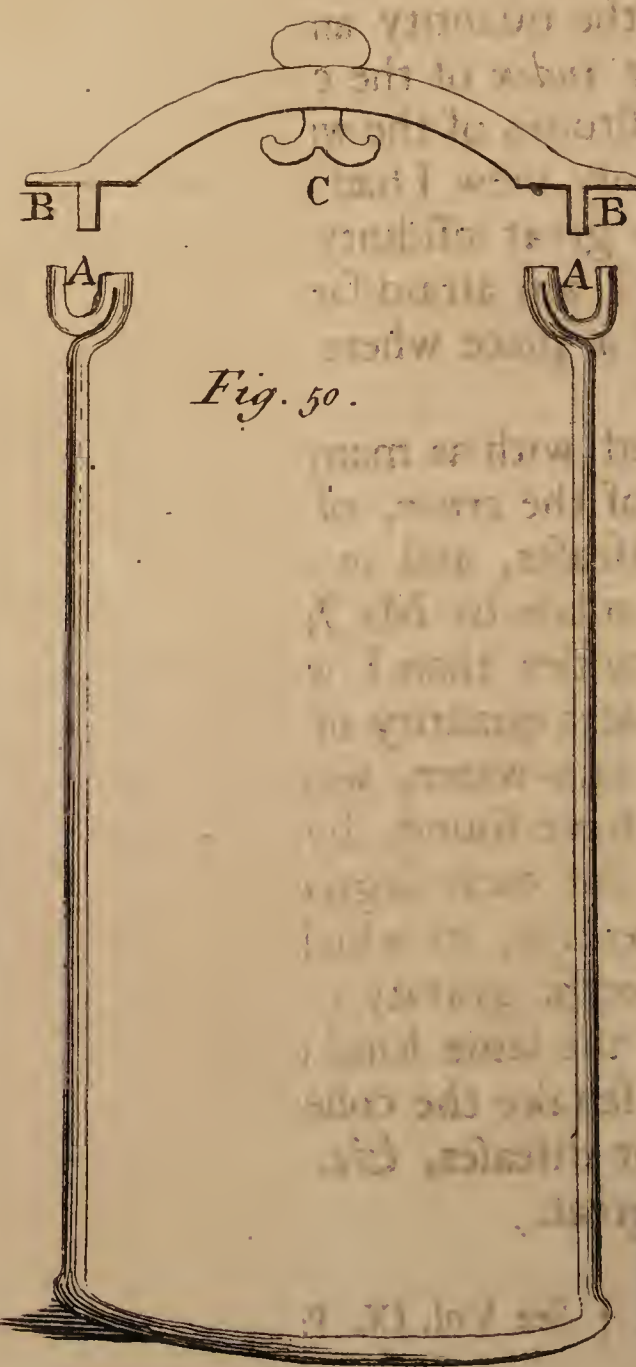


Fig. 50.

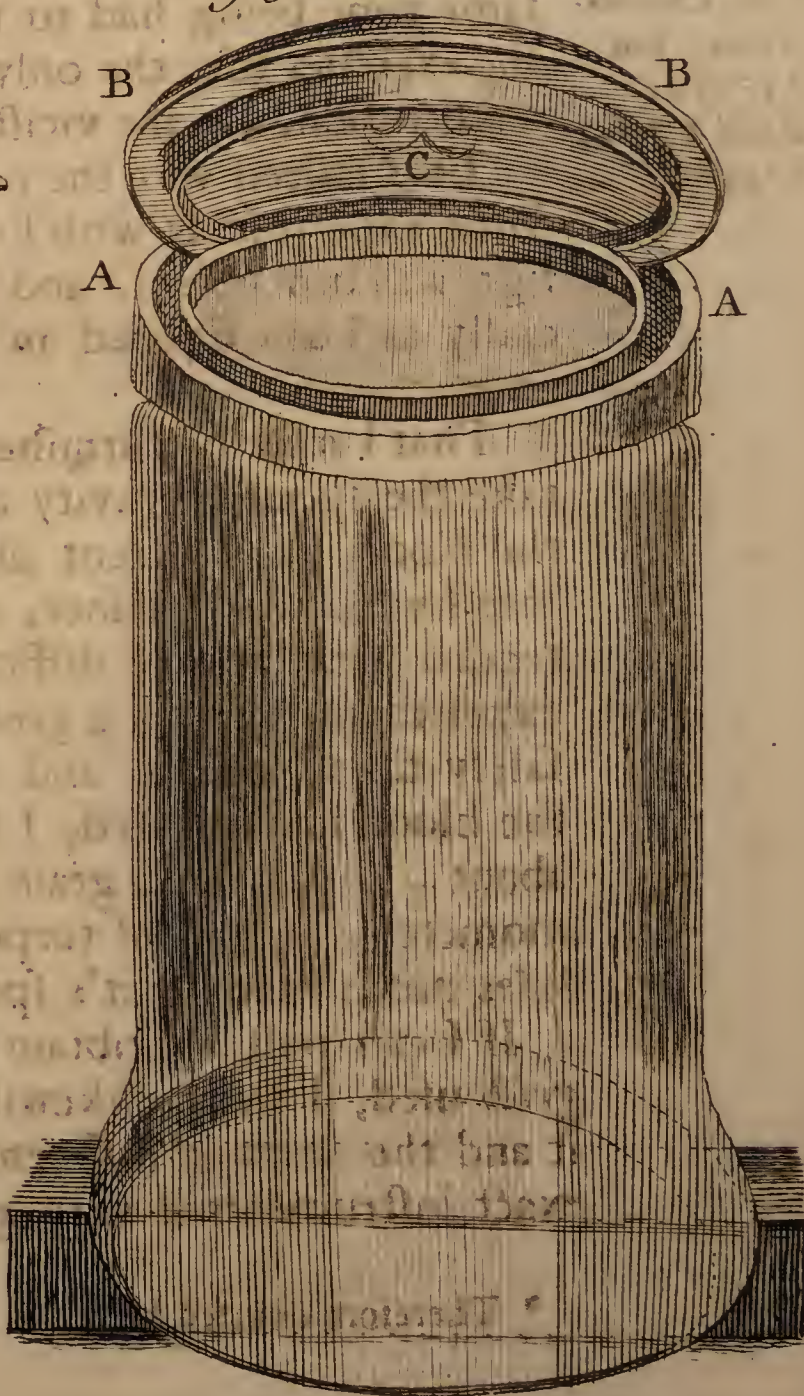


Fig. 51.

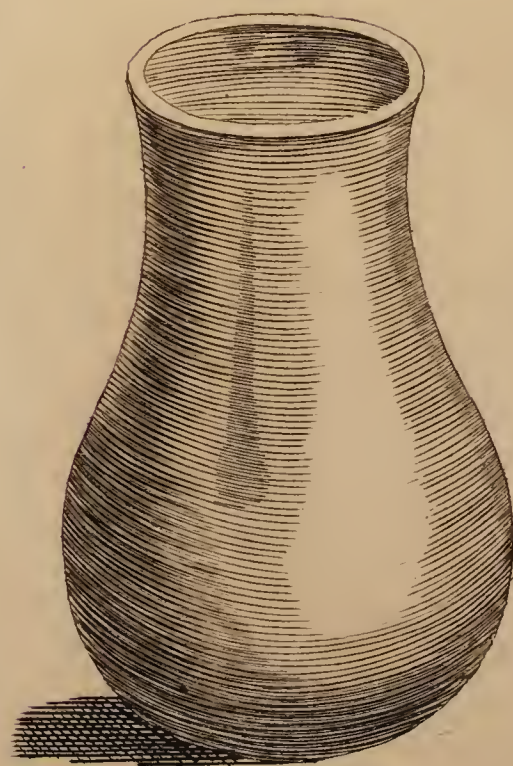


Fig. 49.

The following table contains the sum of the extremes in the different months of the year, and at bed-time, in each month of the year. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given.

| Month | Mean | Extremes |
|-----------|------|-------------|
| January | 32.5 | 25.0 - 40.0 |
| February | 34.0 | 26.0 - 42.0 |
| March | 36.0 | 28.0 - 44.0 |
| April | 38.0 | 30.0 - 46.0 |
| May | 40.0 | 32.0 - 48.0 |
| June | 42.0 | 34.0 - 50.0 |
| July | 44.0 | 36.0 - 52.0 |
| August | 46.0 | 38.0 - 54.0 |
| September | 48.0 | 40.0 - 56.0 |
| October | 50.0 | 42.0 - 58.0 |
| November | 52.0 | 44.0 - 60.0 |
| December | 54.0 | 46.0 - 62.0 |

The following table contains the sum of the extremes in the different months of the year, and at bed-time, in each month of the year.

Table VI

| Month | Mean | Extremes |
|-----------|------|-------------|
| January | 32.5 | 25.0 - 40.0 |
| February | 34.0 | 26.0 - 42.0 |
| March | 36.0 | 28.0 - 44.0 |
| April | 38.0 | 30.0 - 46.0 |
| May | 40.0 | 32.0 - 48.0 |
| June | 42.0 | 34.0 - 50.0 |
| July | 44.0 | 36.0 - 52.0 |
| August | 46.0 | 38.0 - 54.0 |
| September | 48.0 | 40.0 - 56.0 |
| October | 50.0 | 42.0 - 58.0 |
| November | 52.0 | 44.0 - 60.0 |
| December | 54.0 | 46.0 - 62.0 |

The following table contains the sum of the extremes in the different months of the year, and at bed-time, in each month of the year. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given. The mean of the extremes in each month, and at bed-time, in each month of the year, is also given.

As the quantity of *ingesta* varied considerably in almost every month, the increase and decrease of the several *excreta*, as they were influenced by that constitution of the air, which is exhibited in TAB. IV. cannot plainly appear. I have therefore calculated the following table from that, supposing the mean *ingesta* of each month to be $\bar{3}3000$, which is equal to $\bar{3}100$ each day; and that the several *excreta* were evacuated in the same *ratio* from these $\bar{3}3000$, as from the real quantity of each month's *ingesta*: whence the increment or decrement of the sensible and insensible evacuations, in each month, as they were influenced by the weather, will be more conspicuous.

| | Urine | Perfsp | Stools | Urine | | Perspirat. | | Stools | |
|--------|-------|--------|--------|------------|-------------|------------|-------------|------------|-------------|
| | | | | Increased. | Diminished. | Increased. | Diminished. | Increased. | Diminished. |
| March | 1799 | 1106 | 91 | | | | | | |
| April | 1618 | 1323 | 91 | | 181 | 217 | | | |
| May | 1731 | 1492 | 87 | | 187 | 169 | | | 4 |
| June | 1254 | 1706 | 91 | | 177 | 214 | | 4 | |
| July | 977 | 1941 | 88 | | 277 | 235 | | | 3 |
| Aug | 1274 | 1628 | 98 | 297 | | | 313 | 10 | |
| Sept. | 1016 | 1943 | 113 | | 258 | 315 | | 15 | |
| Octob. | 1477 | 1263 | 191 | 461 | | | 680 | 78 | |
| Nov. | 1717 | 1107 | 91 | 240 | | | 156 | | 100 |
| Dec. | 1790 | 1078 | 95 | 73 | | | 29 | 4 | |
| Jan. | 1846 | 1006 | 104 | 56 | | | 72 | 9 | |
| Feb. | 1976 | 948 | 81 | 130 | | | 58 | | 23 |

The following TABLE contains the sum of the *excreta* in the different seasons.

[To be added to TAB. VI.]

| | Urine. | Perspirat. | Stools. |
|---------|--------|------------|---------|
| Spring. | 5393 | 3377 | 263 |
| Summer. | 3662 | 5139 | 266 |
| Autumn. | 3767 | 4834 | 402 |
| Winter. | 5353 | 3191 | 290 |

The succeeding TABLE contains in ounces and centesimals the mean diurnal and nocturnal urine and perspiration of one hour in each month, with their *ratio's* to each other: and that the causes of the great disproportion which they bear to each other, in the different seasons, may be more conspicuous, I have added the mean thermometrical altitude in the heat of the day, and at bed-time, in each month of the year.

Mean

| | | | | | | | | | | | | | | |
|--|---|--------|--------|--------|--------|--------|--------|--------|-------|--------|--------|--------|--------|--------|
| Mean thermo- metrical alti- tude. | The mean daily ingesta. | 117.38 | 109.03 | 117.92 | 125.51 | 138.78 | 130.42 | 118.22 | 96.89 | 109.62 | 118.43 | 118.76 | 118.46 | 117.88 |
| | The difference. | 9 | 9 | 8 | 9 | 10 | 7 | 6 | 7 | 6 | 7 | 7 | 6 | 8 |
| | At bed-time. | 54 | 65 | 70 | 74 | 76 | 74 | 72 | 56 | 50 | 41 | 43 | 46 | 60 |
| | At 3 in the afternoon. | 63 | 74 | 78 | 83 | 86 | 81 | 78 | 63 | 56 | 48 | 50 | 52 | 68 |
| The ratio's which the diurnal and nocturnal perspiration and urine bear to each other. | The nocturn. perspirat. to the noct. urine as 1 to | 1.32 | 1.19 | 1.46 | 1.21 | 1.14 | 1.73 | 1.20 | 1.40 | 1.48 | 1.70 | 1.24 | 1.63 | 1.40 |
| | The diurnal perspiration is to the diurnal urine as 1 to | 1.71 | 1.10 | 0.81 | 0.58 | 0.35 | 0.57 | 0.46 | 0.98 | 1.57 | 1.61 | 1.94 | 2.24 | 0.98 |
| | The diurnal urine is to the nocturnal as 1 to | 0.326 | 0.766 | 1.164 | 1.117 | 1.510 | 1.400 | 1.260 | 1.374 | 0.832 | 0.764 | 0.467 | 0.571 | 0.922 |
| | The diurnal perspiration is to the nocturnal as 1 to | 0.924 | 0.702 | 0.644 | 0.611 | 0.471 | 0.461 | 0.488 | 0.966 | 0.882 | 0.717 | 0.728 | 0.782 | 0.644 |
| Mean urine of one hour | Nocturnal. | 2.61 | 2.06 | 2.58 | 2.55 | 2.37 | 2.87 | 2.13 | 2.35 | 2.32 | 2.43 | 1.7 | 2.22 | 2.35 |
| | Diurnal. | 3.16 | 2.69 | 2.22 | 2. | 1.57 | 2.05 | 1.69 | 1.71 | 2.79 | 3.18 | 3.64 | 3.89 | 2.55 |
| Mean per- spiration of one hour. | Diurnal. | 1.71 | 1.72 | 1.77 | 2.1 | 2.08 | 1.66 | 1.77 | 1.68 | 1.57 | 1.42 | 1.37 | 1.36 | 1.68 |
| | Nocturnal. | 1.85 | 2.45 | 2.75 | 3.42 | 4.42 | 3.6 | 3.63 | 1.74 | 1.78 | 1.98 | 1.88 | 1.74 | 2.61 |
| | March | | | | | | | | | | | | | |
| | April | | | | | | | | | | | | | |
| | May | | | | | | | | | | | | | |
| | June | | | | | | | | | | | | | |
| | July | | | | | | | | | | | | | |
| | August | | | | | | | | | | | | | |
| | September | | | | | | | | | | | | | |
| | October | | | | | | | | | | | | | |
| | November | | | | | | | | | | | | | |
| | December | | | | | | | | | | | | | |
| | January | | | | | | | | | | | | | |
| | February | | | | | | | | | | | | | |
| | The Means | | | | | | | | | | | | | |

From the preceding TABLE it appears, that,

1. The mean diurnal perspiration in the

| | | | | | | |
|--------|---------|-----|---|--------|--|---|
| Spring | } was { | .20 | } Parts greater than the mean nocturnal perspiration in the | Spring | } And the days warmer than the nights by | 8 |
| Summer | | .44 | | Summer | | 9 |
| Autumn | | .43 | | Autumn | | 7 |
| Winter | | .23 | | Winter | | 6 |
| Year | | .36 | | Year | | 9 |
2. The mean diurnal urine in the

| | | | | |
|--------|---------|-----|--|---------|
| Spring | } was { | .29 | } Parts greater than the mean nocturnal urine in the | Spring. |
| Summer | | .23 | | Summer. |
| Autumn | | .32 | | Autumn. |
| Winter | | .33 | | Winter. |
| Year | | .08 | | Year. |

3. The

3. The excess of the mean diurnal perspirat. above the nocturnal in $\left\{ \begin{array}{l} \text{Spring} \\ \text{Summer} \\ \text{Autumn} \end{array} \right\}$ was $\left\{ \begin{array}{l} \text{less by} \\ \text{greater by} \\ \text{greater by} \end{array} \right\} \left\{ \begin{array}{l} .04 \\ .72 \\ .67 \end{array} \right\}$ Parts, than was the excess of the mean diurnal perspirat. above the noct. in winter.

4. The excess of the mean diurnal perspiration above the nocturnal, in *July*, *Aug.* and *Sept.* taken together, which are our sickly months, exceeded the excess of the mean diurnal perspiration above the nocturnal in winter, by .79 parts of what was the mean diurnal perspiration above the nocturnal in these 3 hot months taken together.

5. Though the mean nocturnal heat in *July*, *Aug.* *Sept.* taken together, exceeded the mean diurnal heat in *Nov.* *Dec.* *Jan.* and *Feb.* taken together, by 22 degrees; yet the perspiration, under such very different degrees of heat, was nearly equal. For the mean nocturnal perspiration of one hour, in these 3 warm months taken together, was 884 grains; and the mean diurnal perspiration of one hour, in these 4 cold months taken together, was 889 grains.

6. In *July*, though the excess of the mean diurnal heat above the nocturnal exceeded the excess of the mean diurnal heat above the nocturnal in winter by 4 degrees only; yet the excess of the mean diurnal perspiration above the nocturnal in *July* exceeded the excess of the mean diurnal perspiration above the nocturnal in winter, by $\frac{8^2}{100}$ parts of what was the excess of the mean diurnal perspiration above the nocturnal in *July*.

7. The day and night's perspiration, at a *medium*, in equal spaces of time, was more nearly equal in the cold than in the hot months; and in *Oct.* and *Nov.* or at the accession of the cold weather, they were more equal than in any month of the year, *March* excepted.

8. The evenings and nights are not absolutely colder in proportion to the heat of the day in autumn, than in any other season of the year, as *Celsus* has affirmed; but are only so relatively, as, at that season, we are weakest, and most sensibly affected by any increment or decrement of the air's heat.

9. In *June*, *July*, *Aug.* and *Sept.* taken together, the excess of the mean nocturnal urine above the diurnal, in equal times, was .65 parts deficient of what was the decrement of the nocturnal perspiration, or the excess of the diurnal perspiration above the nocturnal in these 4 warm months taken together.

10. In summer, the excess of the mean nocturnal urine above the diurnal was .63 parts less than was the decrement of the nocturnal perspiration in that season.

11. In autumn, the excess of the mean nocturnal urine above the diurnal was .34 parts less than what was the decrement of the nocturnal perspiration in that season.

12. The excess of the mean nocturnal urine above the diurnal was not equal to the decrement of the nocturnal perspiration in *May*, *June*, *July*, *Aug.* *Sept.* by .61, .58, .66, .58, .63 parts of what was respectively the decrement of the nocturnal perspiration in these months; but, in *Oct.*

the excess of the nocturnal urine above the diurnal was .91 parts greater than was the decrement of the nocturnal perspiration in that month.

13. If the mean diurnal *ingesta* had been $\frac{5}{3}$ 100 thro' the year, and the excretions proportionate from that quantity to what they were from the real mean diurnal *ingesta* of each month, then, by computation, it appears that

| | | | | | | | | |
|---------------------------------------|---|---|---|--|---|--|--|---|
| A, the mean diurnal perspiration in | $\left\{ \begin{array}{l} \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \\ \text{Feb.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{was in-creased} \\ \text{was dim.} \\ \text{was inc.} \\ \text{was dim.} \\ \text{was inc.} \\ \text{was inc.} \\ \text{was dim.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .30 \\ .04 \\ .14 \\ .18 \\ .16 \\ .10 \\ .42 \\ .10 \\ .10 \\ .07 \\ .07 \end{array} \right\}$ | Parts of what was the mean diurnal perspiration in | $\left\{ \begin{array}{l} \text{March} \\ \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{and was inc.} \\ \text{and was dim.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .33 \\ .42 \\ .52 \\ .43 \\ .49 \\ .23 \\ .47 \\ .52 \\ .48 \\ .52 \end{array} \right\}$ | Parts of what was the mean diurnal perspiration in March. |
| | | | | | | | | |
| B, The mean nocturnal perspiration in | $\left\{ \begin{array}{l} \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \\ \text{Febr.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{was inc.} \\ \text{was dim.} \\ \text{was inc.} \\ \text{was diminished} \\ \text{was inc.} \\ \text{was inc.} \\ \text{was diminished} \\ \text{was diminished} \\ \text{was inc.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .08 \\ .05 \\ .10 \\ .07 \\ .18 \\ .15 \\ .13 \\ .17 \\ .22 \\ .03 \end{array} \right\}$ | Parts of what was the mean nocturnal perspiration in | $\left\{ \begin{array}{l} \text{March} \\ \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{and was inc.} \\ \text{and was dim.} \\ \text{and was inc.} \\ \text{and was dim.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .03 \\ .13 \\ .06 \\ .12 \\ .03 \\ .16 \\ .01 \\ .23 \\ .21 \\ .21 \end{array} \right\}$ | Parts of what was the mean nocturnal perspiration in March. |
| | | | | | | | | |
| C, The mean diurnal urine in | $\left\{ \begin{array}{l} \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \\ \text{Febr.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{was diminished} \\ \text{was inc.} \\ \text{was dim.} \\ \text{was inc.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .08 \\ .32 \\ .05 \\ .26 \\ .25 \\ .09 \\ .19 \\ .31 \\ .06 \\ .12 \\ .07 \end{array} \right\}$ | Parts of what was the mean diurnal urine in | $\left\{ \begin{array}{l} \text{March} \\ \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{and was dim.} \\ \text{and was inc.} \\ \text{and was inc.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .38 \\ .41 \\ .57 \\ .41 \\ .47 \\ .35 \\ .06 \\ .12 \\ .18 \end{array} \right\}$ | Parts of what was the mean diurnal urine in March. |
| | | | | | | | | |
| D, The mean nocturnal urine in | $\left\{ \begin{array}{l} \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \\ \text{Feb.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{was dim.} \\ \text{was inc.} \\ \text{was diminished} \\ \text{was inc.} \\ \text{was dim.} \\ \text{was inc.} \\ \text{was dim.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .15 \\ .14 \\ .07 \\ .13 \\ .20 \\ .19 \\ .26 \\ .13 \\ .03 \\ .12 \\ .37 \end{array} \right\}$ | Parts of what was the mean nocturnal urine in | $\left\{ \begin{array}{l} \text{March} \\ \text{April} \\ \text{May} \\ \text{June} \\ \text{July} \\ \text{Aug.} \\ \text{Sept.} \\ \text{Octob.} \\ \text{Nov.} \\ \text{Dec.} \\ \text{Jan.} \end{array} \right\}$ | $\left\{ \begin{array}{l} \text{and was dim.} \\ \text{and was inc.} \\ \text{and was dim.} \end{array} \right\}$ | $\left\{ \begin{array}{l} .01 \\ .09 \\ .20 \\ .01 \\ .19 \\ .09 \\ .05 \\ .08 \\ .47 \\ .16 \end{array} \right\}$ | Parts of what was the mean nocturnal urine in Mar. |
| | | | | | | | | |

A letter from Dr Lining at Charles-Town in S. Carolina.

1355

The following TABLE exhibits, in ounces and centesimals, the greatest and least mean diurnal urine and perspiration of one hour.

[This to be added to T A B. II.]

| | Mean diurnal urine of one hour | | Mean diurnal perspi- ration of one hour | |
|-----------|-----------------------------------|-------|--|-------|
| | greatest | least | greatest | least |
| March | 5.01 | 1.81 | 3.64 | 1.07 |
| April | 4.41 | 1.47 | 3.37 | 1.42 |
| May | 3.9 | 0.83 | 4.42 | 1.32 |
| June | 3.89 | 1.15 | 5.25 | 1.7 |
| July | 3.88 | 0.71 | 5.64 | 2.36 |
| August | 3.52 | 0.95 | 5.56 | 1.85 |
| September | 3.95 | 0.62 | 5.12 | 2.22 |
| October | 3.46 | 0.67 | 2.66 | 0.93 |
| November | 4.83 | 1.14 | 2.11 | 1.36 |
| December | 5.13 | 1.60 | 3.04 | 1.34 |
| January | 6.39 | 1.40 | 2.61 | 1.45 |
| February | 6.29 | 2.05 | 2.26 | 0.79 |

A TABLE of the depth of rain in inches and millesimal parts in Charles-Town.

| | 1738 | 1739 | 1740 | 1741 | 1742 | 1743 | The means |
|-------------|--------|--------|--------|--------|--------|--------|--------------|
| January | 1.097 | 2.310 | 4.873 | 4.492 | 2.189 | 3.172 | 3.022 |
| February | 4.416 | 2.875 | 3.084 | 3.535 | 1.650 | 2.435 | 2.999 |
| March | 4.532 | 5.609 | 1.141 | 5.713 | 5.203 | 0.621 | 3.803 |
| April | 1.082 | 0.195 | 1.092 | 1.308 | 0.918 | 5.292 | 1.648 |
| May | 3.127 | 5.120 | 5.612 | 4.841 | 5.898 | 2.535 | 4.522 |
| June | 1.567 | 15.839 | 4.648 | 5.538 | 3.250 | 1.903 | 5.458 |
| July | 10.660 | 5.452 | 3.013 | 3.399 | 1.252 | 7.738 | 5.252 |
| August | 4.104 | 12.211 | 7.301 | 7.144 | 7.647 | 3.767 | 7.020 |
| September | 10.792 | 4.834 | 3.200 | 6.734 | 2.895 | 4.686 | 5.524 |
| October | 1.358 | 6.593 | 1.257 | 3.399 | 0.759 | 1.672 | 2.506 |
| November | 2.656 | 1.235 | 1.848 | 2.964 | 3.388 | 3.220 | 2.552 |
| December | 3.877 | 3.689 | 2.736 | 1.919 | 0.957 | 2.706 | 2.647 |
| Total depth | 49.268 | 65.962 | 39.805 | 50.986 | 35.826 | 39.747 | 46.932 |

A state of the English weights and measures of capacity, as they appear from the laws as well ancient as modern; with some considerations thereon; being an attempt to prove, that the present Avoirdupois weight is the legal and ancient standard for the weights and measures of this kingdom; by Samuel Reynardson, Esq; F.R.S. N^o.

491. p. 54.

Jan. &c. 1749.

Read Mar. 9.

1748.

V. It is declared by (1) *Magna Charta* that there should be, throughout the realm, one measure of wine (2), one of ale, and one of corn; viz. the quarter (3) of London; and that it should be of weights as of measures.

This declaration has been repeated in many subsequent laws (4), and by several of them the Treasurer is directed to provide standards of bushels, gallons, and weights, of brass; and to send them into every county; and all measures are to be made according to the King's standard; the assize whereof is established by several laws (5), as follows: 'The English penny, called a sterling round, without clipping, to weigh 32 grains of wheat dry, and taken from the midst of the ear. 20 pence make an ounce. 12 ounces a pound. 8 pounds make a gallon of Wine (6). 8 gallons of wine make a London bushel (7), which is the eighth part of a quarter.' And by other laws (8) it is declared, 'That the tun of wine, oil, and honey, should contain, of the English measure, according to the antient assize, 252 gallons; the pipe or butt 126; the tertian 84; the hoghead 63; and every barrel 31½, according to the old assize, and to be gaged by the King's Gager.'

In the reign of Edward III. (9) an act passed to take away the weight called *ancell* (10), whereby, and by subsequent statutes, it is directed, that every sale and buying should be by the even balance.

In the 11th year of Hen. VII. complaint being made to the parliament, that the ancient statutes and ordinances of the realm relating to weights and measures had not been observed and kept, it was therefore enacted, 'That there should be delivered to the Knights and Citizens of every shire and city, one of every weight and measure, which the King had caused to be made of brass, according to his standard in the Exchequer, to be delivered to the respective places mentioned in the act; and that the inhabitants of all cities, boroughs, and market-towns, should make and use weights and measures made according to

(1) Cap. 25.

(2) Bishop Fleetwood says, it was a good law of King Edgar, that there should be the same weight and the same measures throughout the realm, but it was never well observed. *Chron. Preciosum*, p. 34. — And, 2 *Inst.* p. 41. says, 'This law was grounded upon the law of God, Deut. xxv. ver. 13, 14. — and that there were good laws for weights and measures made before the Conquest by Canute. See *Custum de Norm.* c. 16.'

(3) See p. the contents of a quarter.

(4) 51 H. III. St. 6. 14, 25, and 27 Ed. III. 13, 15, and 16 R. II. 9 H. VI. 11 H. VII. c. 4. 16 Car. I. and 22 Car. II. cap. 8.

(5) 51 H. III. St. 1. c. 3. 31 Ed. I. 12 H. VII. c. 5.

(6) The 12th H. VII. c. 5. says wheat.

(7) 9 H. VI. c. 8. says, — Buyers of corn in London bought by a vessel called a fat, containing 9 bushels of corn; which is forbid by the act.

(8) 2 and 18 H. VI. 1 R. III. c. 13. 5 Ann. c. 27. § 19. 23 H. VIII. c. 7. 2 H. VI. c. 11.

(9) 25 Ed. III. St. 5. c. 9. 34 Ed. III. c. 5. 8 H. VI. c. 5. 9 H. VI. c. 8.

(10) King Stephen (says Knighton) settled measures of length and of land, and made appointments *de Ansulis, Bilancibus, &c.* *Decem Scriptores*, p. 2391.

‘ the weights and measures so delivered as aforefaid.’ In the next year another act passed, reciting, ‘ That the King had made such weights and measures of brass, according to the *old* standard thereof remaining within his Treasury; which weights and measures, upon more diligent examination, had been *approved* defective, and not made according to the statutes and *old laws*, and were therefore recalled, and ordered to be broken, and other new bushels and gallons were directed to be made and *sised*, according to a new bushel and gallon to be made according to the *assize*, to remain in the King’s Exchequer:’ where we now find a *bushel* in the custody of the chamberlains called the *Winchester* bushel (11), and a gallon agreeing thereto: upon the bushel there is the following inscription; *Henricus septimus Dei gratia Rex Angliæ et Franciæ.*

In the last mentioned act, the *assize* for weights and measures is in substance the same as in the *old* statutes, only the pound is said to be the *pound Troy* of 12 ounces. But since by this and the former assize laws the pound is directed to be raised from 240 sterling penies, it follows, that the gravity of the assize pound was always the same; but the dimensions of measures of capacity respectively raised from a pound of wine, and a pound of wheat, will be in proportion to each other as the specific gravity of wheat to that of wine or water.

Thus continued the laws relating to the *English* standard of weights and measures till after the Restoration; when a duty of excise (12) being laid upon beer, ale, and other liquors, 36 gallons taken by the gage, according to the standard of the ale-quart, (4 whereof made the *gallon* remaining in the *Exchequer*) were to be reckoned as a barrel of beer, and 32 such gallons a barrel of ale; and afterwards (13) 34 such gallons of vinegar (and of beer or ale stronger or small without the bills of mortality) were declared to be a barrel; and all other liquors liable to the excise-duty were to pay according to the *wine gallon*.

We now find the officers of the revenue determining the contents of our measures of capacity with great exactness: for, on the 25th of May 1688, two general officers of the excise, in the presence of the Lord-Mayor, the commissioners of excise, Mr *Flamstead*, and others, upon an exact trial found that the *old standard wine gallon*, kept in *Guildhall*, did contain but 224 cubic inches; nevertheless, at that time it was thought convenient to continue the former supposed content, being 231

(11) The first time I find it so called by any law, is in an act 22 *Car. II. c. 8.*: and afterwards it is called by this name in several acts of parliament; but in the act just now mentioned, it is said to be *commonly* called the *Winchester* measure. Perhaps it first took that name from the statute made at *Winchester* 16 *R. II.* which directs the clerk of the market to have all his weights and measures ready, and marked and signed according to the standard of the Exchequer.

(12) 12 *C. II. c. 24. § 20.*

(13) 1 *W. and M. c. 24. § 5.* 10 *W. III. c. 21.* 11 and 12 *ditto, c. 15.*

cubic inches, as the *standard wine gallon*, and which has since been established by a law (14).

In the year 1696, an experiment was made, in order to fix the true and exact contents of the brass *standard bushel* of Henry VII. which being filled with common spring-water, and the water measured out with great nicety and exactness; the bushel (15) was found to contain 2145,6 solid or cubic inches; and the water being weighed by the standard weights in the exchequer (and by a beam, which would turn with six grains put into either scale, with 30 pounds in each scale) was found equal to 1131 ounces 14 pennyweights *Troy*; and at the same time and place the standard *Troy* weights were compared with the standard *Avoir-depois*, and 15 pounds of the latter were found equal to 18 pounds 2 ounces 15 pennyweights *Troy*; which fixes the pound *Avoirdepois* at 7000 (16) such grains, as the *Troy* pound weighs 5760; and upon three several trials, made by the gentlemen of the *Council* of the *Royal Society*, at the Exchequer, upon a medium the *Avoirdepois* pound was found equal to 7000,25 *Troy* grains.

By the first (17) *malt* act, which passed soon after the making the experiment upon the *Winchester* bushel, it is declared, that every bushel 18 inches and $\frac{1}{2}$ wide, and 8 inches deep, should be esteemed a legal *Winchester* bushel: and the *coal* bushel is directed (18) to be made 19 inches and $\frac{1}{2}$ diameter, and to contain the last bushel and one quart of water. The first contains 2150,42 cubic inches, the last 2217,47.

We now see different measures established by law (19); and under the excise laws, two different gages or measures, used for taking the dimensions of *wine* and *ale* vessels. The *wine* gallon contains 231 cubic inches, and the *ale* gallon 282; but upon what foundation this last measure was established is difficult to determine.

Troy weights had for some time been established and used for the money affairs in the *Mint*, and for weighing gold, silver, and some few commodities; and the *Avoirdepois* were in general use for weighing all heavy and gross commodities. *Wine* measure was generally looked upon as equal to *Troy* weight: from hence the managers of the *excise* duty were perhaps led to fix the standard of the *ale* gallon, bearing the same proportion to the *wine* gallon as the *Avoirdepois* pound did to the *Troy*; and according to this conjecture, the two gallons answer pretty

(14) 5 *Ann* c. 27. § 17. — This act says, Any cylinder 7 inches diameter, and 6 inches deep, or any vessel containing 231 cubical inches, and no more, shall be a lawful wine-gallon.

(15) *Everard's Stereometry*, p. 193.

(16) *Ward*, in his *Young Math. Guide*, says, 6999 $\frac{1}{2}$ grains. *Phil. Trans.* N°. 465. p. 181. and N°. 470. — *Bishop Hooper* 10. *Pharmacopœia Londin.* says, — The *Avoirdepois* pound is said to be about 7000 grains.

(17) 13 *W.* III. c. 5. § 28. and 1 *Ann.* St. 2. c. 3. § 10.

(18) 12 *Ann.* St. 2. c. 17. § 11.

(19) Though contrary to *Magna Charta*, and several other laws not repealed.

exactly (20); the *ale* gallon exceeding the proportion by somewhat more than one cubic inch and one quarter; but it exceeds the *Winchester* gallon, or 268,2 cubic inches by very near 14 cubic inches: and not one of these measures is agreeable to the words of the *assize*, which directs, (21) 'That the *buskel* shall contain 8 gallons of wheat, the gallon ' 8 pound of wheat of *Troy weight*, the pound 12 ounces of *Troy weight*,' &c. according to the *old* laws of this land.

It is very plain the law-makers in *Henry* the VIIth's time took the *Troy* weight for the standard; and most authors who have written upon this subject have followed their example.

The great difficulty we are under in fixing upon a *standard pound*, agreeable to the *assize*, arises from the uncertainty of the rule laid down in our laws of *assize* for raising the *pound* from 7680 grains of *wheat*; as these grains differ in weight, in different countries, and in different years, I might have said in the same field, and in the same year.

The uncertainty of a pound so raised might with great probability occasion the variety in our weights and measures, so often complained of in our *ancient* laws, and for the prevention whereof *Edward* III. in his 14th year, ordered 'Standard weights and measures to be made of ' *brass*, and sent into every city and town in the kingdom.'

The laws of *assize* never received any alteration, except by the 12th of *Hen.* VII. when the pound is declared to contain 12 ounces of (22) *Troy* weight, and the gallon 8 pounds of *wheat* of *Troy* weight; and since the laws have received no change, we have great reason to conclude, that the standard weights themselves never suffered any addition or diminution; but however this be, we (23) now find in the custody of the proper officer of the *Exchequer* 2 sets of weights, kept there as standards; one called *Troy*, the other *Avoirdupois* weight.

As there is no account handed down to us by our ancestors, shewing at what time, and upon what occasion, these weights, differing considerably in gravity from each other, were there first deposited, we

(20) For, as 144 : 175 :: 231 : 280, 729—And as 144 : 175 :: 224 : 272, 222. This last comes very near the vulgar dry gallon.

(21) 12 H. VII. c. 5.

(22) This is the first time the standard weights are called *Troy* weights. But in an act 2 H. V. St. 2. c. 4. and 2 H. VI. c. 13. relating to *Goldsmiths*, there is mention made of *The Pound of Troy*.

(23) *Phil. Trans.* N°. 470.—The *Avoirdupois* weight of 14 pounds is marked with a crowned E. and inscribed

XIIII POUND AVERDEPOIZ.

ELIZABETH. REGINA.

1582.

The *Troy* weights marked with a crowned E. are ounces, from 256 down to the 16th part of an ounce: and there are no whole pounds *Troy*, penny weights, or grain weights, at the *Exchequer*. There not being pounds, or greater weights, seems to be a proof that these weights were never designed or used for determining the weight of large bodies, or heavy goods.

are at a loss to determine which is the *ancient standard* weight described by the laws of *assize*.

The act in the 12th of *Hen. VII.* has called the standard weight by the name of *Troy* weight; this is the first time the weights are so called in any of our *assize* laws; and notwithstanding this authority, it will be found very difficult, if not impossible, to reconcile the *Troy weight* and *measure* raised therefrom with the words of the *assize*, and any measures now in being; for the natural and most ready way to determine this question is to compare both the *Troy* and *Avoirdepois* weight with measures raised from each, according to the rule laid down in the *assize*, and with such measures as are or have been used by authority.

‘ The most exact (24) and geometrical way of expressing the capacity of any vessel or measure is by expressing in known terms the solidity of a body which will precisely fill it: the fittest will be water. The solidity of all bodies is best expressed by the help of a cube, whose equal sides we know by a standard measure of length; and it appears, that this way of determining measures of capacity is not only the most geometrical, but also exceeding ancient (25).’ By this rule some gentlemen at *Oxford*, in the year 1685, determined the weight of a cubic (26) foot of spring water, or 1728 solid inches, to be 1000 ounces *Avoirdepois*; and by the same rule the capacity and contents of the standard bushel in the *Exchequer* was determined in the year 1696, with great care and exactness: by the same rule the contents of other vessels of capacity have been settled; and in the following table p. 71. I have inserted the names of such measures as are of any authority, whose contents are known; by which the proportion they bear to each other, and to measures raised according to the *assize*, as well from the pound *Troy* as the pound *Avoirdepois*, will be readily observed.

In the next place let us compare the experiment made upon the cubic foot of spring water with that upon the *Winchester bushel*, and we shall find an uniform and perfect agreement between them; and that, upon each trial, a cubic vessel, the sides whereof were equal to an *English* foot, did contain (27) 1000 ounces *Avoirdepois* of spring water. From

(24) *Bishop Cumberland's Essay*, p. 60. — who also says, The *Egyptians* made their *ardob* the cube of their known standard the *cubit*: — and that the *Romans* made their *quadrantal* the cube of their standard the *foot*.

(25) *Measures of bodies* are either determined by their solid contents, or weight. Measures of content are formed from cubes of assigned lengths. *Bishop Hooper*, p. 2.

(26) *Phil. Trans.* N^o. 169.

| | | | | |
|-----|------|---------|-----|---------|
| oz. | pwt. | cu. in. | oz. | cu. in. |
|-----|------|---------|-----|---------|

(27) For as 1131. 14 *Troy* : 2145,6 :: 1000 *Avoir.* : 1728,041. Some writers upon this subject say, that a cubic foot of spring water is equal to 76 pounds *Troy*; which is 10 pennyweights 20 grains more than the 1000 *Avoirdepois*. See *Arbuthnot's Tables explained*, p. 80, 283. *Bishop Hooper's State*, &c. p. 11. — But the *Explainer* of *Arbuthnot's* tables seems to have been quite ignorant of any experiment since *Sir Jonas Moore's* time; and to have disregarded the due proportion between the *Avoirdepois* and *Troy* pound; and for 175. to 144. his tables, he says, are calculated at 17. to 14.

hence

hence we are led to the discovery of a *natural and universal standard* for the *English weights and measures*; and such an one as is, in every respect, agreeable to the words of the *assize* recorded in our most *ancient laws* *.

Magna Charta points out the *quarter* of *London* as the only standard for measures and weights of that time; but we are left to guess of what measure or weight it was the quarter part. If we suppose it the quarter of a *ton*, or 2000 pound weight, then the *quarter* was 500 pounds, and the eighth part of that, or a *buschel*, was equal to a *cubic foot*, or 62½ pounds; from whence less measures and weights were easily deduced. Subsequent *assize laws* direct the greater measures to be raised from the less; that 8 pounds should make a gallon; 8 gallons a buschel; which was to be the eighth part of a quarter; and by this rule the *quarter* is raised to 512 pounds, and the *ton* to 2048 pounds. These measures and weights are raised with ease from known parts of the *foot*. For a cubic vessel, whose sides are equal to $\frac{1}{10}$ of a foot, will contain a cube of spring water equal to an *ounce Avoirdepois*; and from hence, by a regular *geometrical progression*, we shall obtain cubes equal to (28) 8—64—512 ounces, or to 4—32—256—2048 pounds *Avoirdepois*: and from a cubic vessel containing one such pound, we shall have other cubic vessels, equal in weight 8—64—512 pounds; and in measure to the *gallon*, *buschel*, and *quarter*, according to the *assize*.

The (29) *gallon*, *buschel*, and *quarter*, are called dry measures; and are used for ascertaining the quantity of corn, and other dry goods; the *gallon* is also a liquid measure raised from a pound, in liquids now called a *pint* (30); from whence all the other liquid measures are raised; but with this difference in the proportion, that the *liquid buschel* is not 64, but 63 pounds or pints; eight whereof make the *hogshead* equal to 63 gallons; from whence the contents, as well of the larger as smaller vessels or measures of capacity are settled.

The *measures of capacity* thus raised, are sufficiently convenient for common use, and are generally retained at this time; but for *weights*, there has been some variety from time to time, in the composition of the larger sort, used for determining the weight of merchandize and heavy goods, as will appear from the following extract from several old acts of parliament.—The *stone* for weighing *lead* was (31) settled at 12 pounds; for wax, sugar, spices, and allom, at 8 pounds; of which

* The discovery of this Analogy was published by the Rev. Mr William Barlow, in these Transactions, N°. 458. p. 457, Anno 1740. C. M. See Vol. IX.

(28) Eight ounces are equal to a *mark*, whereof two, or twice the contents of that cube make a pound *Avoirdepois*.

(29) The half-buschel, peck, gallon, pottle, and quart, are directed by 25 Ed. III. St. 5. c. 10. to be made according to the King's standard.

(30) See note (35) of the next page—The *pint* is not mentioned in the *assize laws*; but Bishop Hooper has given a long and learned dissertation upon that measure, and calls it the *pint of old*, p. 458.

(31) *Cay's Abridgment Title Weights*, § 9.

last, 13¹/₂, or 108 pounds, made the *hundred weight*: the *sack of wool* (32) was to weigh but 26 *stone*, 14 *pounds* to each *stone*: a *weye* (33) of cheese 32 *cloves*, each *clove* 7 *pounds*. And for many years past, the *hundred weight* has been fixed (34) at 112 *pounds Avoirdepois*, and that by a general consent, and without any particular law to establish it.

These weights have been universally and immemorially (35) used in *England*, with an exception to the weighing of gold, silver, and some very few commodities, for which the *Troy weight* has been used for a great many years. When it was first introduced into this kingdom, does no-where appear; but Mr *Folkes*, in his tables of the *English silver coins* (36), tells us, it was not established or used at the *Mint* before the 18th of *Hen. VIII.*

By reducing the *liquid bushel*, or one eighth of the *hogsheaf*, from 64 to 63 pints, it seems plain that our ancestors took the *cubic foot* for their *model*; the contents of such a vessel being $62\frac{1}{2}$ pints or pounds: and from hence, and from what has been shewn before, it is not very unnatural to conclude, that at first our ancestors fixed and established as well their weights as measures from known parts of this model; taking always a whole number for each *primary weight* or *vessel*; and from thence proceeding, by a regular *geometrical* proportion, to raise the greater weights or measures: so that the *English foot* (the undoubted and universal *standard* of all *measures of length* within this realm) is also the standard for the *Avoirdepois* (37) weights, and all measures of capacity.

Upon

(32) 25 *Ed. III. St. 5. c. 9.* 13 *R. II. c. 9.*

(33) 9 *H. VI. c. 8.* The weye equalled 224 pounds.

(34) That is, 14 *stone* at 8 pounds, or 8 *stone* at 14 pounds each, according to the *old laws*, and *present usage* of the *stone weight*. The 112 pound is a very convenient weight for a standard, because it is divisible into more even parts than any less number.—And it is compounded from the *assize bushel*, it's half and quarter; that is to say, 64.32, and 16 pounds.

(35) The *Apothecaries* (who, next to the *Goldsmiths*, are supposed to make the most use of *Troy weights*) seldom keep weights adjusted to the *Troy pound* heavier than two drams; but for all above buy and sell by *Avoirdepois*: and with them, by the term *libra* in measure is meant the *wine pint*; though this measure is not, say they, so denominated from it's containing an exact pound-weight of any liquor, and the term *uncia* in measure does not denote a twelfth part of the pint, but the sixteenth; though in weight, agreeable to it's signification, it is used to express one twelfth part of a pound; so that an ounce in measure is scarce more than $\frac{3}{4}$ of an ounce in weight. See *Pemberton's Dispensary*, p. 44.

(36) *Page 4.* Mr *Folkes* says, The pound used at the *mint* before that time, called the *Tower* or the *Moneyers pound*, was equal to 5400 *Troy grains*: and, p. 13, 14, that the weight of the groat, from 13 *Hen. IV.* to 4 *Edw. IV.* was equal to 60 such grains. Which is agreeable to what is said in an act of parliament of 2 *Hen. VI.* that the pound *Troy* of coined money was worth 32 shillings; for 32 shillings, or 96 groats, at 60 grains each, weigh 5760 grains, or a pound *Troy*. Though, by the same act, by reason of the scarcity of silver money, and in order to bring bullion into the mint, it was enacted, That silver uncoined, of the same goodness as the money, should be sold only for 30 shillings the pound *Troy*.

(37) The very name *Avoirdepois*, by which our common weights are known, has by some been looked upon as a proof that they were of foreign extraction. The first time

I find

Upon the whole therefore, I think it is sufficiently proved, that a *cubic vessel*, whose sides are equal to an *English foot*, will contain 1000 ounces *Avoirdepois*, or very near that weight of spring-water: that weights and measures, deduced by a regular *geometrical progression* from such a vessel, or from cubic vessels, whose sides are equal to known parts of an *English foot*, bear an exact analogy to each other, and to weights and measures raised from a *pound*, according to the words of our most ancient *assize laws*. This being considered, and that the *Avoirdepois* weight is now in common use for determining the gravity of all heavy bodies, that this weight now is, and immemorially has been, used for settling the ancient duty of *tonnage* and *poundage* upon all goods and merchandize taken by weight (except some few drugs, which are charged in the book of rates by the ounce *Troy*); and that there is not the least proof, either in our *ancient* or *modern laws*, to induce a belief that this duty was ever generally taken by the *Troy* weight, or that *Troy* weights were ever in general and common use in this kingdom, it must surely be allowed, that the weight mentioned in our *old laws*, or acts of parliament, was the *Avoirdepois* weight.

The learned Bishop *Cumberland*, in his (38) Treatise, says, ‘ That *Postscript.*
 ‘ our *English Avoirdepois* ounce is the same as the *Roman* ounce; and
 ‘ was probably introduced into this kingdom by the *Romans*, when
 ‘ they gave laws and planted colonies here, and hath thence continued
 ‘ unchanged to this day; which is not commonly observed, because
 ‘ we use the *Avoirdepois* weights only about heavier commodities; not in
 ‘ weighing silver and gold, which are weighed by the *Troy* ounce; which
 ‘ I suppose was introduced by the *Normans*, because it takes it’s name
 ‘ (39) from a *French* town, *Troyes* in *Champaigne*.’ Most authors (40)
 have been of this opinion. This leads me to compare our *English foot*
 with the *Roman foot*, which Mr *Greaves* takes as equal to 967 such

I find the word used in our laws, is in an act of *Ed. III. St. 1.* where it is applied to *wines* as well as *corn*; as it is afterwards in *25 Ed. III. St. 3. c. 2.* and *16 R. II. c. 1.* And in an act *27 Ed. III. St. 2. c. 10.* there is the following clause:—‘ Because we have
 ‘ perceived some merchants buy *Avoirdepois* merchandizes by one weight, and sell by
 ‘ another, we will and establish, that *one weight, one measure, and one yard*, be through
 ‘ all the land; and that *wools*, and all manner of *Avoirdepois*, be weighed by even *balance*.’ This King, in his 14th year, had directed *standard weights* to be made of *brass*, and sent into every *city and town*; and I conjecture, that those *standards*, from the words of the foregoing clause, took the name of *Avoirdepois*, and were the weights by which the merchants used to *buy*. What were the lighter weights by which they *sold*, does not appear; perhaps the *pound Troy*. That the former were the *lawful* weights, appears by an act *24 H. VIII. c. 3.* where they are so called; and Butchers, who before that time sold their meat by hand, were thereby obliged to provide themselves with beams, scales, and weights *sealed*, called *Haberdepois* (for *Avoirdepois*); and in the next reign the *Avoirdepois* weights, now remaining as *standards* in the *Exchequer*, were deposited there, as appears from the name and inscription thereon.

(38) See p. 11, 103, 107.

(39) Bishop *Hooper*, p. 432, is of another opinion as to the derivation of the name.

(40) See *Hooper’s Inquiry*, 10, 14, 92; and *Arbuthnot’s Tables explained*, p. 16, and 283.

parts, as ours is 1000. The *Roman amphora* or *quadrantal* is generally allowed (41) to be equal to a *cubic Roman foot*; and to contain 80 pounds, or 960 ounces. Then the side of the *amphora* is equal to (42) ,986 parts of the *English foot*; agreeing exactly with the foot deduced by *Villalpandus* from the *congius* of *Vespasian*; and a cubic vessel, whose sides are equal to ,967 parts of the *English foot*, will not contain (43) quite 904 $\frac{1}{4}$ ounces; which, if true, reduces the *Roman ounce* to near 412 $\frac{1}{2}$ grains *Troy*.

| The Table of | Bushels in Cube inch. | Gallons in Cu. Inch. | Pints in Cu. Inch. | Weight of the gallon in <i>Avoirdupois</i> pounds. |
|---|-----------------------------|----------------------------|--------------------------|---|
| By the coal act | 2217,47 | 277,183 | 34,648 | 10,025 |
| By the malt act | 2150,42 | 268,8 | 33,6 | 9,722 |
| Winchester bushel | 2145,6 | 268,2 | 33,525 | 9,6 |
| From, the Wine Gallon | 1848 | 231 | 28,875 | 8,354 |
| The Guildhall gallon | 1792 | 224 | 28 | 8,101 |
| 16 oz. <i>Avoirdupois</i> | 1769,472 | 221,184 | 27,648 | 8 |
| 12 oz. <i>Troy</i> | 456,0224 | 182,0028 | 22,75035 | 6,5826 |
| The following are not supported by any law or authority : | | | | |
| (44) The vulgar dry meas. | 2178 + | 272,25 | 34,0625 | 9,8468 |
| (45) The Ale measure | 2256 | 282 | 35,25 | 10,1995 |

An account of
two Chinese
paper-money
bills; by F.
Anth. Gaubil,
to Dr Mortimer.
No. 494.
p. 330. Jan.
&c. 1750.
Read Feb.
1749. Dated
Peking,
Nov. 9. 1748.

VI. Two paper money-bills of the reign of *Hongvou*.

The year of Christ 1368, was the first of the empire of *Hongvou*, founder of the dynasty of *Ming*. During the dynasty of *Yuen* (who were *Mogul Tartars*) which *Hongvou* destroyed, there was a great deal of paper-money. There had also been some, 140 years before, under the dynasty of *Kin* (oriental *Tartars*) who reigned in the northern provinces of *China*, and in *Tartary*. The *Yuens* destroyed this dynasty, as well as that of the *Song's*, who were *Chinese*, that reigned in the southern provinces.

(41) See Bishop *Hooper*, p. 152, 175. *Greaves's Mis. Works*, p. 198, 199, 297, 303.

(42) For the cube root of 960 is 986,1. And
oz. Parts.

(43) The cube of 967 is but 904,231063. And Mr *Greaves* himself says, an *amphora* made by the *Pes Colotianus* held only $7\frac{1}{2}$ *congi*, equal to 900 *Roman ounces*; which comes as near the cube of 967, as can be expected from the uncertain method he took to determine the contents of that *amphora*, which was by filling it with 7 *congi*, and, as he guessed, about an half, of *milium*. See his *Miscellaneous Works*, (1737) p. 225.

(44) Dr *Arbuthnot* gives a table of the vulgar dry measure, as the contents of the *Winchester* measure. And he had so little regard for the *Avoirdupois* weight, that he does not give any table thereof.

(+) See the note (20)

(45) The *ale* measure even exceeds the *coal* measure. And the excess of the *ale* measure above the *Winchester* is more than one in 20 of the last measure.

vinces of *China*. We find no paper-money of the dynasties of the *Xuen's* and *Kin's*; and that of *Hongvou* is scarce. The *Bonzes* and *Chinese* empirics superstitiously say that this paper-money laid upon children brings them good luck.

These two bills are the same with those, the figures and explanations of which are to be seen in *Father du Halde's Description de la Chine*, Tom. II. pag. 168.

I send you the two,* because probably you have none of that sort. The late *Father Dentrecolles* formerly sent to *France* an account of the *Chinese* money, ancient and modern: and some notes and remarks on this sort of money were of late years sent hence to *Petersburg*.

VII. 1. Though an inquiry into the manner how bodies are resolved by putrefaction, with the means of accelerating or preventing that process, has been reckoned not only curious, but useful (a), yet we find it little prosecuted in an experimental way: nor is it to be wondered at, considering how offensive such operations are: wherefore, as I have been led to make some experiments and remarks on this subject, from the accident of having had an uncommon number of putrid distempers under my care in the hospitals of the army, I shall venture to lay before the *Society* what I have found somewhat different from the common opinion, as well as some facts, which, as far as I know, have not been mentioned before.

Some experiments on substances resisting putrefaction; by John Pringle, M.D. F. R. S. N^o. 495. p. 480. Apr. &c. 1750. Read June 28. 1750. here printed with additions.

1. Finding it a received notion, that bodies by putrefaction became highly alkaline, I made the following experiments, to inquire how far this was true in fact.

The *serum* of human blood putrified, made, with a solution of sublimated mercury, first a turbid mixture, and afterwards a precipitation. This is one of the tests of an *alkali*, but scarce to be admitted here; since the same thing was done with recent urine (of a person in health), which is never accounted alkaline. The same *serum* did not tinge the syrup of violets green; and made no effervescence when the spirit of vitriol was poured upon it. I made the experiment twice upon portions of different *serum*, both highly putrid; and once on water, in which corrupted flesh had been some time infused; and the most I could find was, that, having given the syrup previously a small reddish cast with an acid, this colour was rendered fainter, but not destroyed by the putrid humours; and as to the effervescence, having dropped the spirit of vitriol into these liquors unmixed, and also diluted with water, the mixture was quiet, and only a few air-bubbles appeared on shaking the glasses. Upon the whole, though there were some marks of a latent *alkali* in the putrid *serum*, they were so very faint, that one drop of spirit of hartshorn in a

* One of which I presented to the *R. S. C. M.*

(a) Lord *Bacon* calls, "the inducing or accelerating putrefaction a subject of very universal inquiry;" and says, "that it is of excellent use to inquire in o the means of preventing or staying putrefaction; which makes a great part of Physick and Surgery." See *Nat. Hist. Cent. IV.*

quantity

quantity of water equal to that of the putrid liquors, shewed more of an *alkali* than 20 drops of any of the other.

2. It has been a maxim, that all animal substances, after putrefaction, being distilled, send forth a great quantity of volatile salt in the first water; but Mr *Boyle* found that this held good only in urine; and that in the distillation of the *serum* of human blood putrefied, the liquor which first came over had little strength, either as to it's smell or taste, and did not at first effervesce with an acid. And here it may be observed, that the Chemists have generally applied those properties they discovered in urine, to all the humours indifferently; whereas, in fact, there is a great diversity.* For some animal substances, such as urine and bile, *soon* putrefy; the *saliva* and the white of an egg *slowly*. Yet those that soonest corrupt do not always arrive at the highest degree of putrefaction. Thus the bile is soon corruptible, but the rankness of it is not to be compared to that of flesh; and the white of an egg is not only much less disposed to putrefy than the yolk, but, when corrupted, yields a different and less offensive smell. And it seems particular to stale urine to contain an alkaline salt, which, without distillation, makes a strong effervescence with acids: whereas most other animal humours putrefied, though of a more intolerable *fætor*, yet contain less volatile salt, less extricable, and not effervescing with acids. But what makes the difference between stale urine and other putrid substances still more specific, is, it's inoffensiveness with regard to health; whilst the steams of most other corrupted bodies are often the cause of putrid and malignant diseases*.

Now, upon finding in urine a much greater quantity of volatile salt, and that more easily separable than in any other humour, and that stale urine is the least noxious of putrid animal substances, so far then from dreading the volatile *alkali* as the deleterious part of corrupted bodies, from this instance we may rather infer it to be a sort of corrector of putrefaction.

3. Daily experience shews how harmless the volatiles are, both when smelled to, or taken in substance; but still there remains a prejudice, as if these salts, being the produce of corruption, should therefore hasten putrefaction; not only in distempers where these salts are unwarily taken, but also in experiments out of the body.

Now, as to the effects arising from the internal use of them, little can be said, unless the kind of disease was precisely stated. For, supposing they were by their nature disposed to promote putrefaction; yet if that is already begun, from a languor of circulation, and obstruction, then may the volatiles, by their stimulating and aperient quality be the means of stopping it's progress: and, on the other hand, though they were really antiseptic, yet if the humours are disposed to corrupt from excess of heat or motion, these very salts, by adding to the cause, may

* Nat. Hist. of human blood, Vol. IV. p. 178. fol.

augment the disease. So that, upon the whole, it will be the fairest criterion of the nature of these volatiles to enquire, whether out of the body they accelerate or retard putrefaction.

In order to decide this question, I have made repeated experiments of joining both the spirit and salt of hartshorn to various animal substances; and have constantly found, that, so far from promoting putrefaction, they have evidently hindered it; and that with a power proportioned to their quantity. The trials have been made with the *serum* of the blood, and also with the *crassamentum*, after it had been dried by keeping. I once separated the thick inflammatory crust of pleuritic blood from the rest of the mass; and, dividing it, I put one portion into distilled vinegar, the other into spirit of hartshorn; and after keeping the infusions above a month in the middle of summer, I found the piece which lay in alkaline spirit as sound as that in the acid.

Another time I put in one phial about an ounce and a half of an equal mixture of ox's gall and water, with 100 drops of spirit of hartshorn; and in another as much of the gall and water without any spirit. The phials, being corked, were set by a fire, so as to receive about the degree of animal heat; whereby, in less than two days, the mixture without the spirit became putrid, but the other was not only then, but after two days longer untainted.

I afterwards infused 3ij of the lean of beef with 3ij of water and 3ss. of salt of hartshorn. Another phial contained as much flesh and water with a double quantity of sea-salt: in a third was the flesh and water only to serve by way of index. These phials were placed on a lamp-furnace, in a heat varying between 94 and 104 degrees of *Fahrenheit's* scale. About 18 hours after infusion, the contents of that phial which served as an index, were rank; and in a few hours more that with the sea-salt was also putrid; but the flesh with the volatile alkali was sound, and continued so after standing 24 hours longer, in the same degree of heat: and that the smell of the hartshorn might occasion no deception, the piece of flesh was washed from the salt, and still smelled sweet.

About the same time I took 3 pieces of fresh beef, of the same weight as above; and laying 2 of them in gallypots, I covered one with saw-dust, and the other with bran: but the third piece being strewed with salt of hartshorn powdered I put into a four ounce phial which had a glass stopper. They were all three placed in the outside of a window exposed to the sun; and the weather being warm, on the third day the flesh in the gallypots began to smell; on the fourth were putrid. Next day the phial was examined; when the flesh was washed from the salt, and found quite sweet. It was then dried and salted again with hartshorn; and having stood in the house some weeks longer in sultry weather, it was looked at a second time, and observed to be as sound as before; neither was the substance at all dissolved, but was of such a consistence.

consistence as might be expected from common brine *. And lest it might be suspected, that the flesh in the gallypots, by being more exposed to the air than that in the phial, became sooner putrid, I have since inclosed flesh in phials, as that with the hartshorn and found the confinement rather hasten the putrefaction.

Now, by these and many other experiments of the kind, finding volatile alkaline salts not only do not dispose animal substances to putrefaction out of the body, but even prevent it, and that more powerfully than common sea-salt, we may presume that the same taken by way of medicine, will, *cæteris paribus*, prove antiseptic; at least we cannot justly suppose them corrupters of the humours more than fermented spirits or sea-salt; which taken in immoderate quantities may raise a fever, and thereby accidentally be the occasion of corruption.

4. I have likewise made several experiments with the fixed alkaline salts, which have no less antiseptic power than the volatile. The trials were made both with the lee of tartar and salt of wormwood. But here we must not confound a disagreeable smell of such mixtures with one that is really putrid; nor the power those lixivials have of dissolving animal substances with putrefaction.

5. From these experiments it was natural to conclude, since acids by themselves were amongst the most powerful antiseptics, and the alkaline salts were likewise of that class, that the mixtures of the two to saturation would resist putrefaction little less than the acid alone. But in the trials I have made upon flesh with a *spiritus Mindereri* composed of vinegar saturated with salt of hartshorn, and also with the juice of lemons saturated with the salt of wormwood, I found the antiseptic virtue considerably less than when either the acids or *alkali's* were used singly.

6. As for the comparative virtues of these salts upon flesh, I found ʒss of lemon-juice saturated with a scruple of the salt of wormwood resisted putrefaction nearly as much as gr. xv. of nitre; but, when the trial was made with ox's gall, ʒij of this mixture were more antiseptic than ʒi of that salt. Again, nitre compared with the dry neutral salts, weight for weight, is more antiseptic than any in preserving flesh I have yet tried. Crude *sal ammoniac*. came next to it, and even exceeded it in the experiment with ox's gall. After these the *sal diureticus*, *tartarus solubilis*, and *tartarus vitriolatus*, seemed to have nearly the same power.

I have mixed vinegar with a large quantity both of chalk and crabs-eyes, in order to neutralize it; but, though seemingly saturated by the effervescence ceasing, it still retained an acidity, and was found much more antiseptic than lemon-juice neutralized with the salt of wormwood; tho' this last acid be considerably stronger than vinegar.

7. Thus far have we considered the common neutral salts; which, however powerful in resisting putrefaction, are inferior to some resinous

* The same piece has been since kept dry a twelvemonth, and is still untainted, and as firm as at first.

substances, and even some vegetables which I have tried. Thus myrrh, in a watry *menstruum* was found at least 12 times more antiseptic than sea-salt. Two grains of camphire mixed with water preserved flesh better than gr. lx. of that salt: and I imagine, could the camphire be kept from flying off, or concreting to the sides of the phial, that gr. ls or even less, would have sufficed. An infusion of a few grains of *Virginian* snake-root in powder exceeded 12 times it's weight of sea-salt. Camomile-flowers have nearly the same extraordinary quality. The Jesuit's bark has it also; and if I have not found it so strong as the two substances last mentioned, I impute that in part to my not being able to extract it's embalming parts in plain water.

Now vegetables possessing this balsamic quality are the more valuable, in that, being usually free of acrimony, they may be taken in much greater quantities than either spirits, acids, resins, or even the neutral salts. And as in the great variety of substances answering this purpose, there may be also some offensive or useful qualities annexed, it may not be amiss perhaps to review some part of the *materia medica* for this end. I shall add, that, besides this extraordinary power in preserving bodies, I have discovered in some of these substances a sweetening or correcting quality after putrefaction had actually begun.

2. Three pieces of the lean of fresh beef, each weighing 3ij, were put separately into wide-mouthed phials. 3ij of cistern-water were added to each; in one were dissolved gr. xxx. of sea-salt; in another 60; but the third contained nothing but flesh and water. These bottles were little more than half-full; and, being corked, were placed in a lamp-furnace, regulated by a thermometer, and kept about the degree of human heat.

The same continued N^o. 196 p 525. Nov &c. 1750. Read Nov. 1. 1750.

About ten or twelve hours after, the contents of the phial without salt had a faint smell; and in 3 or 4 hours more were putrid*. In an hour or two longer the flesh with the least salt was tainted; but that which had most, remained sweet above 30 hours after infusion. This experiment was often repeated with the same result, making allowance for variations of the degree of heat.

The use of this experiment was for making standards, whereby to judge of the septic or antiseptic strength of bodies. Thus, if water with any ingredient preserved flesh better than without it, or better than with the additions of the salt, that ingredient might be said to resist putrefaction more than water alone, or with 30 or 60 grains of sea-salt. But if, on the other hand, water, with any addition, promoted corruption more than when pure, the substance added was to be reckoned a septic, or hastener of putrefaction.

* It is to be observed, that these pieces were all intire; but when they are beat to the consistence of a pap, with the same quantity of water, the putrefaction then begins in less than half the time mentioned here.

The following experiments were therefore all made in the same degree of heat with the quantity of flesh, water, and air, as above specified; together with such septic or antiseptic substances, as shall be afterwards mentioned, and were all compared with the standards. But whereas the least quantity of salt preserved flesh little longer than plain water, I shall always compare the several antiseptic bodies with the greatest quantity of salt; so that whenever any substance is said to oppose putrefaction more than the standard, I mean, more than gr. lx. of sea-salt.

2. I began with examining other salts, and compared them in the same quantity with the standard; which being of all the weakest, I shall suppose it equal to unity, and express the proportional strength of the rest in higher numbers in the following table.

| | | |
|--|----------------------------|-----|
| <i>A Table of the comparative powers of salts in resisting putrefaction.</i> | Sea-salt | 1 |
| | <i>Sal gemmæ</i> | 1+ |
| | Tartar vitriolated | 2 |
| | <i>Spiritus mindereri</i> | 2 |
| | <i>Tartarus solubilis</i> | 2 |
| | <i>Sal diureticus</i> | 2+ |
| | Crude <i>sal ammoniac.</i> | 3 |
| | Saline mixture | 3 |
| | Nitre | 4+ |
| | Salt of hartshorn | 4+ |
| | Salt of wormwood | 4+ |
| | Borax | 12+ |
| | Salt of amber | 20+ |
| | Alum | 30+ |

In this table I have marked the proportions by integral numbers; it being hard, and perhaps unnecessary, to bring this matter to more exactness; only to some I have added the sign +, to shew, that those salts are stronger than the number in the table by some fraction; unless in the 3 last, where the same sign imports that the salt may be stronger by some units*. The tartar vitriolated is rated at 2; though more than gr. xxx of it was taken to equal the standard: but perceiving all of it was not dissolved, an allowance was made accordingly. On the other hand, as part of the hartshorn flies off, it's real force must be greater than what appears by the table. The salt of amber is likewise volatile; and as gr. iii. of it were found more preservative than gr. lx. of sea-salt; it may therefore be much more than 20 times stronger. This is indeed

* Five grains of borax was the smallest quantity compared with sea-salt; but holding out so much longer, I suspect three grains would have been sufficient; in which case the force of this salt was to be estimated at 20: a singular instance of the strength of a salt not acid. gr. i. of alum was weaker than 60 of sea-salt; but 2 were stronger. The power therefore of alum lies between 30 and 60; but, as I could judge by the experiment, nearer the first number.

an acid salt; but as the acid part of it is inconsiderable, this high antiseptic power must be owing to some other principle. The *spiritus mindereri* was made of common vinegar and salt of hartshorn; the saline mixture of salt of wormwood saturated with lemon-juice. The alkaline part in either of these mixtures with water only would have resisted with a power of 4 +; so that the acid added rendered these salts less antiseptic; viz. the *spiritus mindereri* by $\frac{1}{2}$, and the saline mixture by $\frac{1}{3}$: which was a circumstance very unexpected.

3. Next I proceeded to try resins and gums, and began with myrrh. As part of this substance dissolves in water, gr. viii. were made into an emulsion; but most of it subsiding, I could not reckon on a solution of more than gr. i or ii; which nevertheless preserving the flesh longer than the standard, we may account the soluble part of myrrh perhaps about 30 times stronger than sea-salt.

Aloes, *asa fetida*, and the *terra Japonica*, dissolved in the same manner as myrrh, like it subsided, and with the same antiseptic force. But gum ammoniac and *sagapenum* shewed little of this virtue. Whether it was that they opposed putrefaction less, or that all the antiseptic principle fell with the grosser parts to the bottom. Three grains of opium dissolved in water did not subside, and resisted putrefaction better than the salt. But I observed that more air than usual was generated, and the flesh became tenderer than with any of the stronger antiseptics.

Of all the resinous substances camphire resisted most: two grains dissolved in one drop of spirit of wine, gr. v. of sugar, and 3ij of water exceeded the standard: though, during the infusion, most of the camphire flew off, swam a-top, or stuck to the phial. Suppose only the half lost, the remainder is at least 60 times stronger than salt; but if, as I imagine, the water suspended not above a tenth part, then camphire will be 300 times more antiseptic than sea-salt. That nothing might be ascribed to the minute portion of the spirit, used in this experiment, I made another solution of camphire in a drop or two of oil, and found this mixture less perfect, but still beyond the standard.

4. I made strong infusions of camomile-flowers, and of *Virginian* snake-root; and finding them both greatly beyond the standard, I gradually lessened the quantity of these materials, till I found gr. v. of either impart a virtue to water superior to gr. lx. of salt. Now as we cannot suppose these weak infusions contained gr. fs. of the embalming part of these vegetables, it follows, that this must be at least 120 times more antiseptic than common salt.

I also made a strong decoction of the bark, and infused a piece of flesh in 3ij of it strained; which flesh never corrupted, though it remained 2 or 3 days in the furnace, after the standard was putrid. In this time the decoction became gradually limpid, whilst the grosser parts subsided: by which it appears, that a most minute portion of the bark intimately mixed with water (perhaps less than of the snake-root, or camomile-flowers) is possessed of a very extraordinary antiseptic force.

Besides these, pepper, ginger, saffron, contrayerva-root, and galls, in the quantity of gr. v. each, as also gr. x. of dried sage, of rhubarb, and the root of wild valerian *, separately infused, exceeded gr. lx. of salt. Mint, angelica, ground-ivy, fenna, green tea, red roses, common wormwood, mustard, and horse-radish, were likewise infused, but in larger quantities, and proved more antiseptic than the standard. And as none of these can be supposed to yield in the water above gr. i. or ii. of the embalming principle, we may look upon them all as very powerful resisters of putrefaction. Farther, I made a trial with a decoction of white poppy-heads, and another with the expressed juice of lettuce, and found them both above the standard.

By these specimens we may now see how extensive antiseptics are ; since, besides salts, fermented spirits, spices and acids, commonly known to have this property, many resins, astringents, and refrigerants, are of the number ; and even those plants called anti-acids, and supposed hasteners of putrefaction ; of which class horse-radish is particularly antiseptic. And indeed after these trials, I expected to find all dissolvable substances endowed with some degree of this quality ; till, upon further experiments, I perceived some made no resistance, and others promoted corruption. But before I enter upon that part of my subject, it will be proper to relate some other experiments more nearly connected with the preceding.

5. Having seen how much more antiseptic these infusions were than sea-salt, I then tried whether plants would part with this virtue without infusion. For this purpose, having 3 small and thin slices of the lean of beef, I rubbed one with the powder of the bark, another with snake-root, and a third with camomile-flowers. It was in the heat of summer, yet, after keeping these pieces for several days, I found the flesh with the bark but little tainted, and the other 2 quite sweet. The substance of all the three was firm ; particularly that with the camomile, which was so hard and dry, that it seemed incorruptible. Why the bark had not altogether the same effect, was probably owing to its close texture.

6. I have also made some attempts towards the sweetening of corrupted flesh, by means of mild substances ; because distilled spirits, or strong acids, the only things known to answer this intention, were of too acrid and irritating a nature to be thoroughly useful, when this correction was most wanted. As for salts, besides their acrimony, it is well known, that meat once tainted will not take salt.

A piece of flesh weighing 3ij, which in a former experiment had become putrid, and was therefore very tender, spongy, and specifically lighter than water, was thrown into a few ounces of the infusion of camomile-flowers, after expressing the air, to make it sink in the fluid :

* Though the experiment was only made with gr. x. of the powder of this root, yet, considering how long that quantity resisted putrefaction, we may reckon the valerian among the strongest antiseptics.

the infusion was renewed twice or thrice in as many days; when, perceiving the *fætor* gone, I put the flesh into a clean bottle, with a fresh infusion; and this I kept all the summer, and have it still by me, quite sweet, and of a firm texture*. In like manner I have been able to sweeten several small pieces of putrid flesh, by repeated affusions of a strong decoction of the bark; and I constantly observed, that not only the corrupted smell was removed, but a firmness restored to the fibres.

Now, since the bark parted with so much of it's virtue in water, it was natural to think it would still yield more in the body, when opened by the *saliva* and bile; and therefore it was by this antiseptic virtue it chiefly operated. From this principle we might account for it's success in gangrenes, and in the low state of malignant fevers, when the humours are so evidently putrid. And for intermittents, in which the bark is most specific, were we to judge of their nature, from circumstances attending them in climates and seasons most liable to the distemper, we should assign putrefaction as a principal cause. They are the great endemic of all marshy countries, and rage most after hot summers, with a close and moist state of air. They begin at the end of summer, and continue thro' autumn; being at the worst, when the atmosphere is most loaded with the *effluvia* of stagnating water, rendered more putrid by vegetables and animal substances that rot in it. At such times all meats are quickly tainted; and dysenteries, with other putrid distempers, coincide with these fevers. The heats dispose the humours to acrimony; the putrid *effluvia* are a ferment; and the fogs and dews, so common to those climates, stop perspiration, and bring on a fever. The more these causes prevail, the easier it is to trace this putrefaction of humours. The *nausea*, thirst, bitter taste of the mouth, and frequent evacuations of putrid bile, are common symptoms and arguments for what is advanced. We shall add, that in moist countries, in bad seasons, the intermittents not only begin with symptoms of a putrid fever, but, if unduly managed, easily change into a putrid and malignant form, with livid spots and blotches, and mortification of the bowels. But, as a thorough discussion of this question might carry us too far from our present subject, and be unseasonable here, I shall refer it to it's proper place, and only remark, that whatever medicines (besides evacuations and the bark) have been found useful in the cure of intermittents, they are, so far as I know, all highly antiseptic; such are, myrrh, camphire, camomile-flowers, wormwood, tincture of roses, alum with nutmeg, vitriolic or strong vegetable acids with aromatics.

3. Decoctions of wormwood and of the bark, also infusions of camomile-flowers, and of snake-root, preserved yolks of eggs, not only several days longer than water did alone, but also when a good quantity of sea-salt was added to it. I likewise found that salt of hartshorn preserved this substance better than four times it's weight of sea-salt.

The same continued. Ibid. p. 550. Read Nov. 15. 1750.

* This piece has been kept a twelvemonth in the same liquor, and is still firm and uncorrupted.

2. Ox's gall was kept some time from putrefaction by small quantities of lee of tartar, spirit of hartshorn, crude *sal ammoniac*, and the saline mixture, and still longer by a decoction of wormwood, infusions of camomile-flowers, and of snake-root; by solutions of myrrh, camphire, and salt of amber: all were separately mixed with gall, and found more antiseptic than sea-salt; and seemingly in proportion to their effects upon flesh. Only nitre failed; which, tho' four times stronger than sea-salt in keeping flesh sweet, is inferior to it in preserving gall; and remarkably weaker than crude *sal ammoniac*; which again is somewhat less powerful than nitre in preserving flesh. The nitre was soon opened by the gall, and emitted a vast quantity of air, which rose as from a fermenting liquor; and when this happened, the gall began to putrefy. But the saline mixture generated no air, and opposed the putrefaction of gall more than it did that of flesh.

3. The last trial was with the *serum* of human blood, which was preserved by a decoction of the bark, and an infusion of snake-root, nor with less efficacy than flesh. But saffron and camphire were not here above $\frac{1}{4}$ so antiseptic as before; whether it be that they are less preservative of this humour, or, as I suspect, that they were not well mixed. Nitre acted nearly with it's full force, being about four times stronger than sea-salt: it generated some air, but much less than it did with the gall. No other humour was tried; but, from these specimens added to the former experiments, we may conclude, that whatever is preservative of flesh will be generally antiseptic, though perhaps not always with equal force.

4. Having already shewn how putrid flesh might be sweetned, I shall conclude this part of my subject with a like trial upon the yolk of an egg. A portion of this, being diluted with water, stood till it corrupted; when a few drops were put into a phial with \mathfrak{z} ij of pure water, and about twice as many drops were mixed with a strong infusion of camomile-flowers. At first both phials had some degree of a putrid smell; but being corked, and kept a few days near a fire, the mixture with plain water contracted a strong *fætor*, whilst the other smelled only of the flowers.

Thus far have I related the experiments made of antiseptics; by which it appears, that, besides spirits, acids, and salts, we are possessed of many powerful resisters of putrefaction, endued with qualities of heating, cooling, volatility, astringency, and the like, which make some more adapted than others to particular indications. In some putrid cases, many proper antiseptics are already known; in others they are wanting. We are yet at a loss how to correct the *sanies* of a cancerous ulcer; but, from such a multitude of antiseptics, it is to be hoped some may be found at last adequate to that intention. It may be farther remarked, that, as different distempers of the putrid kind require different antiseptics, so the same disease will not always yield to the same medicine. Thus the bark will fail in a gangrene, if the vessels are too full, or the blood

blood fizy : but, if the vessels are relaxed, and the blood resolved or disposed to putrefaction, either from a bad habit, or the absorption of putrid matter, then is the bark a good specific. With the same caution are we to use it in wounds ; viz. chiefly in cases of absorbed matter, which infects the humours, and induces a hectic fever. But, when inflammatory symptoms prevail, the same medicine increasing the tension of the fibres, and fizziness of the blood, a state directly opposed to the other, has such consequences as might be expected.

By the success of the bark in so many putrid cases, it should appear that astringency had no small share in the cure. And indeed the very nature of putrefaction consists in a separation or disunion of the parts. But as there are other cases, in which astringency is less wanted, we may find in contrayerva-root, snake-root, camphire, and other substances, a highly antiseptic power, with little or none of the other quality. And since several of these medicines are also diaphoretic, their operation is thereby rendered more successful.

I come now to the last thing proposed, which was, to give an account of some observations made on substances hastening or promoting putrefaction ; an inquiry not less useful than the former. For, setting aside the offensive idea commonly annexed to the word, we must acknowledge putrefaction to be one of the instruments of nature, by which many great and curious changes are brought about. With regard to medicine, we know, that neither animal nor vegetable substances can become aliment, without undergoing some degree of putrefaction. Many distempers proceed from a deficiency of this action. The *crises* of fevers seem to depend upon it ; and perhaps even animal heat, according to a late ingenious theory *.

But, in the prosecution of this subject, I have met with very few real septics ; and found many substances, commonly accounted such, of a quite opposite nature. The most general means of accelerating putrefaction is, by heat, moisture, and stagnating air ; which being sufficiently known and ascertained, I passed over, without making any particular experiment on those heads. Lord *Bacon* †, as well as some of the Chemists, has hinted at a putrid fermentation, analogous to what is found in vegetables ; and this having so near a connexion with contagion, I made the following experiment, for a further illustration of this matter.

5. In the yolk of an egg, already putrid, a small thread was dipped, and a small bit of this was cut off and put into a phial, with half of the yolk of a new-laid egg diluted with water. The other half, with as much water, was put into another phial, and both being corked, were set by the fire to putrefy. The result was, that the thread infected the

* An Essay on the cause of animal heat, by *J. Stevenson, M. D.* Vide Medical Essays, Vol. V.

† Vide *Nat. Hist.* Cent. IV. Exper. 330.

fresh yolk ; for the putrefaction was sooner perceived in the phial that contained it, than in the other. But this experiment was not repeated.

In this manner the putrefaction of meat advances quicker in a confined than a free air ; for, as the most putrid parts are also the most fugitive ; they incessantly issue from a corruptible substance, and disperse with the wind ; but in a stagnation of air, they remain about the body ; and by way of ferment excite it to corruption.

6. As for other septics, recited by authors, I found none of them answer the purpose. The alkaline salts have been considered as the chief putrefiers. But this is disproved by experiments. Of the volatiles it may be indeed observed, that, though they preserve from the common marks of putrefaction, with a force four times greater than that of sea salt ; yet, in warm infusions, a small quantity of these salts will soften and resolve the fibres, more than water does by itself. They also hinder the coagulation of blood ; and when taken by way of medicine, thin and resolve it, but are not therefore septics. For, so little do these salts putrefy, or even resolve the fibres, when applied dry, that I have kept, since the beginning of *June* last, notwithstanding the excessive heats, a small piece of flesh in a phial, preserved only with salt of hartshorn, at present perfectly sound, and firmer than when first salted.

7. From the specimens we had of the antiscorbutic plants, it is likewise probable none of that tribe will prove septic. Horse-radish, one of the most acrid, is a very powerful antiseptic. And though carrots, turneps, garlick, onions, celery, cabbage, and colewort, were tried (as *alcalescents*) they did not hasten, but somewhat retarded, the putrefaction.

8. The case was different with such farinaceous vegetables as were examined ; *viz.* white bread in infusion, decoctions of flour, barley, and oatmeal ; for these did not at all retard putrefaction ; but, after it was somewhat advanced, they checked it, by turning sour. By a long digestion the acidity became considerable ; which, by conquering the putrescency of the flesh, and generating much air, did not ill represent the state of weak bowels, which convert bread, and the mildest grains, to such an acid, as prevents a due resolution and digestion of animal food*.

9. I examined *cantharides*, dried vipers, and *Russian castor*, all animal substances, and therefore most likely to prove septic. The flies were tried both with fresh beef, and with the *serum* of human blood ; the vipers only with the former ; but neither of them hastened putrefaction. And as for the *castor*, so far from promoting this process, that an infusion of gr. xii. opposed it more than the standard salt.

* It is to be remarked, that, in making this experiment, I did not then attend to a fermentation that ensued, and which was the cause of the acidity. This kind of fermentation between animal and vegetable substances, being hitherto overlooked, shall be therefore set forth in my next paper.

10. After finding no septics where they were most expected, I discovered some which seemed the least likely; viz. chalk, the *testacea*, and common salt.

Twenty grains of crabs-eyes prepared, were mixed with 6 drachms of ox's gall, and as much water; into another phial was put nothing but gall and water, in the same quantity with the former; and both being placed in the furnace, the putrefaction began much sooner, where the powder was, than in the other phial. I infused afterwards in the lamp-furnace 30 grains of prepared chalk, with the usual quantity of flesh and water; and observed, that the corruption not only began sooner, but went higher by this mixture; nay, what had never happened before, that in a few days the flesh resolved into a perfect *mucus*. The experiment was repeated with the same effect; which being so extraordinary, I suspected some corrosive substance had been mixed with the powder: but, for a trial, a lump of chalk being pounded, 30 grains of it proved fully as septic as the former. The same powder was compared with an equal quantity of salt of wormwood, and care was taken to shake both the mixtures alike: but, after three days warm digestion, the salt had neither tainted nor softened the flesh, whilst the chalk had rotted and consumed that which was joined to it. Nor were the effects less of the testaceous powders of the Dispensary. Eggshells in water resisted putrefaction, and preserved the meat longer firm than plain water*.

11. To try whether the *testacea* would also dissolve vegetable substances, I infused them with barley and water, and compared this mixture with another of barley and water, without the *testacea*. After a long maceration by a fire, the plain water swelled the barley, became mucilaginous and sour; but that with the powder kept the grain to its natural size, though it softened it, made no mucilage, and remained sweet.

12. Nothing could be more unexpected than to find sea-salt a hastener of putrefaction. But the fact is thus. 3i of salt preserves 3ij of fresh beef, in 3ij of water, above 30 hours, uncorrupted, in a heat equal to that of the human body; or, what amounts to the same, this quantity of salt keeps flesh about 20 hours longer sweet, than pure water; but 3ss of salt does not preserve it above 2 hours longer. This experiment has been already mentioned. Now I have since found, that gr. xxv. have little or no antiseptic virtue; and that 10, or 15, or even gr. xx manifestly both hasten and heighten the corruption†. It is moreover to be remarked, that in warm infusions with these smaller quantities, the salt, instead of hardening the flesh, as it does in a dry form, in brine, or even in solutions, such as our standard, it here softens and relaxes

* The trial was made with a coarse powder, of this substance, but not repeated.

† The most putrefying quantity of salt, with this proportion of salt and water, is about 10 grains.

the texture of the meat, more than plain water ; though much less than water with chalk, or the testaceous powders.

Many inferences might be made from this experiment ; but I shall only mention one. Salt, the indispensable seasoner of animal food, has been supposed to act by an antiseptic quality, correcting the too great tendency of meats to putrefaction. But, since it is never taken in aliment beyond the proportion of the corrupting quantities in our experiment, it would appear that salt is subservient to digestion, chiefly by a septic virtue ; that is, by softening and resolving meats ; an action very different from what is commonly believed.

A remark on
Father Hardouin's
amendment
of a passage
in Pliny's
Nat. Hist.
lib. ii.
§ lxxiv. edit.
Paris. folio,
1723. by
Martin
Folkes, Esq;
Pr. R. S. N.
482. p. 365.
Jan. & Feb.
1747. Read
Jan. 22.
1746-7.

VIII. *Vasque horoscopa non ubique eadem sunt usui, in trecentis stadiis, aut ut longissime, in quingentis, mutantibus semet umbris solis. Itaque umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, æquinoctii die, paulo plusquam dimidiam gnomonis mensuram efficit. In urbe Roma nona pars gnomonis deest umbræ. In oppido Ancone superest quinta. Decima in parte Italiæ, quæ Venetia appellatur, eisdem horis umbra gnomoni par fit.*

The geographical reader cannot but observe here immediately, that somewhat is faulty in this passage as it stands ; since the equinoctial shadow of the gnomon being made shorter at *Ancona* than at *Rome*, the latitude of *Ancona* will consequently be made lesser than that of *Rome* ; whereas it is known to be considerably greater ; *Ancona* standing on the *Adriatic*, about 2° to the N. of that capital.

But, upon turning to Father *Hardouin's* observations upon this passage, I find the text to have been altered by him in a very remarkable manner, from all the former printed editions. His observation is as follows :

Hactenus editum est, in oppido Ancone superest quinta XXX. In parte Italiæ, &c. MSS. hic variant. Nos ex certissima conjectura edicavimus, Ancone superest quinta, decima in parte Italiæ, &c. In decima regione Italiæ Venetiam statuit in geographicis libro sequente. Neque simile veri est, in tot particulas, hoc est, in quintas tricesimas, ab homine πλακωτέρως ubique agente, gnomonem umbramve dividi.

Upon which words it may be noted, that although the Rev. Father acquaints us he had met with some variation in the manuscripts, yet he appeals to none ; nay, he even tells us expressly, that his amendment was purely made upon conjecture ; whence we may safely conclude, that it stands unsupported by any various reading or authority whatsoever. He at the same time also acknowledges, that all the printed editions conspire in another reading ; which I have found to be true in several I have had occasion to look into, with this only variation, that whereas the first edition 1469, and several of the following ones, print the word *quinta* at length, and XXX only in figures ; some of the later ones, and that by the *Elzevirs* particularly in 1635, print both the words at length ; the whole passage running thus :

Umbilici

Umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, equinoctii die, paulo plus quam dimidiam gnomonis mensuram efficit: in urbe Roma nona pars gnomonis deest umbræ: in oppido Ancone superest quinta trigesima: in parte Italiæ quæ Venetia appellatur, iisdem horis umbra gnomoni par fit.

The plain meaning of these words is only this, that the length of the shadow of a *gnomon* or upright style at noon, on the day of equinox, is, in *Egypt*, little more than $\frac{1}{2}$ the height of the *gnomon*; that the same at *Rome* wants $\frac{1}{9}$ part of that height; that at *Ancona* the height of the *gnomon* exceeds the length of it's shadow, by $\frac{1}{35}$ part, or is in proportion to it as 35 is to 1; and that, in the part of *Italy* which is called *Venetia*, the length of the shadow, and the height of the *gnomon*, are equal to each other.

The particulars here mentioned are respectively true, in the 4 following latitudes, $26^{\circ} 34'$, $41^{\circ} 38'$, $44^{\circ} 10'$, and 45° . The first of which is the latitude of the middle parts of *Egypt*, and the last that of several places in the territories of *Venice*, the city itself standing, according to *Manfredi's* table, in lat. $45^{\circ} 33'$, and *Padua* in $45^{\circ} 28'$. The lat. of this last place is given by *Ptolemy* $44^{\circ} 30'$, and that of *Aquileia* at the head of the *Adriatic* in the *Friuli* exactly 45° .

The lat. of *Rome*, according to *Ptolemy* $41^{\circ} 40'$, only exceeds that collected from the text before us by $2'$; and his last again falls short $15\frac{1}{2}'$ of that delivered by *Manfredi* and *Bianchini* $41^{\circ} 54\frac{1}{2}'$. Besides which it may be noted, that the very fact here mentioned is also spoken of by *Vitruvius*, as *F. Hardouin* has himself in another of his notes observed. *Sol æquinoctiali tempore ariete libraque versando, quas ex gnomone partes habet novem, eas umbræ facit octo, in declinatione cæli, quæ est Romæ.*

We come last to the latitude of *Ancona*, which is given by *Manfredi* $43^{\circ} 54'$, or $16'$ less than that above collected from *Pliny*; but which is set down by *Ptolemy* $43^{\circ} 40'$, half a degree less than the same. This may however be looked upon as no bad observation, considering the time when, and the manner how, it is supposed to have been made, as we are ignorant of the hour when the sun really crossed the *equator* on the days of observation, and especially as it comes, with all it's imperfections, as near to the truth, as that reported both by our author and *Vitruvius* to have been made at *Rome* itself; and as it only exceeds the true latitude by about the same quantity, which that given by *Ptolemy* 70 or 80 years afterwards fell short of the same.

It therefore appears, upon the whole, that this text needed no correction; and for the observation, that 35 parts were too many for a *gnomon* to be divided into, it will be found to have very little weight, when it is considered, that the Antients made use of very large *gnomons* upon these occasions; that one of the obelisks now standing at *Rome*, that of *St John's Lateran*, is in height 108 *English* feet without the pedestal; and that the other, still buried under the *Campo Marzo*, which was formerly used for this very purpose, wanted but little of the same

height. The thirty-fifth part therefore of the height of such a stone, did not fall short of 3 *English* feet; a much less quantity than which would easily discover itself in the shadow, whose length, notwithstanding all difficulties arising from the *penumbra*, might certainly be determined to less than a foot.

I shall just add to this remark the description given by *Pliny* of this *gnomon*; who, speaking in his xxxvi. book, § xiv. of the obelisks that were at *Rome* in his time, adds, in the beginning of § xv. *Ei, qui est in campo, divus Augustus addidit mirabilem usum, ad deprehendendas solis umbras, dierumque ac noctium ita magnitudines, strato lapide ad magnitudinem obelisci, cui par fieret umbra, brumæ confectæ die, sexta hora, paulatimque per regulas (quæ sunt ex ære inclusæ) singulis diebus decresceret, ac rursus augesceret.* From which description I understand, that there was laid down, from the foot of the obelisk northward, a level pavement of stone, equal in breadth to the breadth of the obelisk itself, and equal in length to it's shadow at noon upon the shortest day; that is to say, that it's length was to the height of the obelisk almost as 22 are to 10; and that into this pavement there were properly let in parallel rulers of brass, whose distances from the point, directly under the *apex* of the obelisk, were respectively equal to the lengths of the shadow thereof at noon, on the several days of the year; as the same lengths decreased from the shortest day to the longest, and again increased from the longest day to the shortest.

After which the author mentions in a passage greatly corrupted, and therefore now almost unintelligible; that one *Manilius*, or *Manlius*, had added to the top of the obelisk a gilded ball, whose use was to make the shadow of the extremity the more observable, as the middle part of the shadow of that globe could readily be estimated; whereas the shadow of an *apex* would, at so great a distance, be intirely imperceptible.

*The elements
of a Short
Hand; by
Samuel Jeake,
Esq; N^o 487.
p. 345. Apr. &c.
1748. Read
May 26. 1748.*

IX. 1. A succession of new short-hands published without the reason of their construction, having put me on forming a method founded on nature, the only guide to perfection, I settled an alphabet in the following manner.

Having taken in a book that lay by me a paragraph as clear of the principal idea of the book as any I could find, consisting of near 1000 letters, I enumerated the repetitions of each of them, and wrote them down; and thereby made the following table of the number of times each letter was repeated in 1000. 'Tis true, it cannot be said the repetitions will be exactly the same in every 1000 letters that may be taken either in the same book or another; but whoever will enumerate them will not find difference enough to be of consequence.





The TABLE.

| a | b | c | d | e | f | g | h | i | k | l | m | n | o | p | q | r | s |
|----|----|----|----|----|----|----|----|----|---|----|----|----|----|----|---|----|----|
| 81 | 20 | 23 | 45 | 99 | 18 | 18 | 54 | 78 | 3 | 36 | 15 | 66 | 83 | 12 | 0 | 50 | 61 |
| t | u | w | x | y | z. | | | | | | | | | | | | |
| 95 | 50 | 25 | 0 | 23 | 1. | | | | | | | | | | | | |

After

After having made this table, I considered with myself, that there were in nature no more than 8 simple characters; 4 whereof are right, and the other 4 are crooked lines.

The 4 right lines are first the perpendicular line |, and secondly the line of level —; which make the two sides of a square. Secondly the oblique line / ascending from the left to right, and the oblique line \ descending from left to right, making the two sides of the rhomb; which is the figure of the diamonds on the cards.

The four crooked lines are only the semicircle when the diameter is either above or below it, or on the right or left hand of it as,    .

All characters whatever must be made up of these, and from their composition, which introduces ambiguity of signification, arises the difficulty of reading a short-hand, which uses the simple characters for some letters, and compound characters for other letters; or, which is as bad, for words.

This difficulty, being unavoidable in a short-hand of more than 8 letters, making it appear that 8 was the number of letters a short-hand ought not to exceed, I considered it in the following light.

1. If *a, e, i, o*, and the aspirate *h*, be suppressed, there will be 19 letters only remaining to be represented by 8 marks.
2. If *c s x z*, which have a sound much alike, be represented by one character, there will remain 15 letters to be represented by the other 7 marks.
3. If *c g k q*, which have a sound not very different, be represented by one character, there will remain 12 letters to be represented by 6 marks.
4. If *b p f* be represented by one mark, there will remain 9 letters to be represented by 5 marks.
5. If *d t* be represented by one mark, only 7 letters remain to be represented by 4 marks.
6. If *l n* be represented by one mark, only 5 letters remain to be represented by 3 marks.
7. If *m, n*, are represented by one mark, only 3 letters remain to be represented by 2 marks.
8. If *u, w*, are represented by one mark, there will remain one mark to represent *y* the only letter hitherto unmentioned.

Writing with suppression of the vowels hath been always admitted into short-hands of all sorts, because the consonants are looked upon as radical letters, which indeed they ought to be. I shall suppress *h*, as being not radical.

All short-hands are subject to ambiguity; for there being but 8 marks to represent 24 letters; and those 8 being used for 8 of them in the short-hand alphabets, the other letters must be described by characters compounded of these 8.

The ranging of the letters into classes, as is done here, will hardly introduce a greater ambiguity than all short-hands are subject to. So that this method cannot be reckoned more puzzling to a reader than any of the rest.

1. The repetitions of *d* being 45, and of *t* 95, amount to 140, for the repetition of this class.

2. The repetitions of *l*, being 36, and of *r* 50, amount to 86, for the repetition of this class.

3. The repetitions of *m*, being 15, and of *n* 66, amount to 86, for the repetition of the third class.

4. The repetitions of *u*, being 50, and of *w* 25, give 75, for the repetition of the fourth class.

5. The repetitions of *e*, when of the nature of *s*, being about half it's number in the table, may be reckoned 10, those of *s* 61, those of *x* 0, and those of *z* 1, give 72, for the repetitions of the fifth class.

6. The repetitions of *b*, being 20, of *f* 18, and of *p* 12, give 50, for the repetitions of the sixth class.

7. The repetitions of *c* before *a*, *o*, *u*, being about 13, of *g* 18, of *k* 3, and of *q* 0, give 34, for the repetition of the 7th class.

8. The repetition of *y*, being 23, gives 23 for the repetition of the 8th class.

By a little reflection it will appear, that the marks applicable to these classes are in some measure determined. For a right line taking up less time than a crooked line in it's description, it is plain the 4 first classes must be referred to the 4 right lines; and the 4 circular parts to the remaining 4 last classes.

But the right lines are indifferent to all the first 4 classes, and the circular parts to the 4 last classes, for the reason just mentioned. So that so much as relates to the fixing the particular right line to represent the particular class, is at the liberty of the inventor of a short-hand, to adjust agreeable to his own fancy: and the same is true of the circular parts. Thus any one may perceive how far the fancy of a short-hand maker is properly bounded or at liberty.

I shall take notice of one shortening rule; which is that of increasing the dimensions of a line, when the letter must be repeated successively; as in *man*, *rare*, and the like cases. This is a good rule of Mr *Weston*.

An alphabet according to the classes.

dt. lr. mn. uw. csxz. bfp. cgkq. y.

/ — \ | C O O

A practice

A practice on the Lord's Prayer.

Handwritten shorthand symbols for the Lord's Prayer, consisting of various strokes and loops.

Which being expressed in letters of the common alphabet will certainly convince the reader how easily a language may be read though the vowels are omitted. e. g.

ur ftr wc rt n vn, llwd b ty nm, ty kn dm cm, ty wll b dn n rt s t
 s n vn,
 gu s ts dy ur dly brd, nd frgu s ur dts, s w frgu ur dtrs, nd ld s
 nt nt tmpttn, bt
 dlor s frm vl, fr tn s t kn dm, t pwr, nd t glry, fr vr nd vr, mn.

As to the introduction of marks for representing words in a short-hand, I shall not now say any thing more about it, than all short-hands, this as well as any other, is equally susceptible of them.

The advantages of this short hand in the state exhibited, when perfectly learned, so as to be written readily, will appear to be.

1. That, by suppression of *a e i o b*, or $\frac{4}{10}$, only $\frac{6}{10}$ of the time of writing ordinary long-hand is necessary to write this.

2. That the simple strokes representing the consonants, not taking up above $\frac{1}{2}$ the time of writing the consonants, only half of $\frac{6}{10}$, or $\frac{3}{10}$ of the time of any thing written in long-hand is necessary for writing this.

3. Right lines not taking up more than $\frac{2}{3}$ of the time of description of crooked lines, as the diameter is $\frac{2}{3}$ of the semiperiphery it appears, if only right lines were used, these $\frac{3}{10}$ would be reduced to $\frac{2}{10}$, by the subtraction of $\frac{1}{3}$ of $\frac{3}{10}$. But, because the number of right lines, all things considered, should not be reckoned but about double the number of crooked ones, only $\frac{2}{3}$ of $\frac{2}{10}$ can be taken from the $\frac{3}{10}$; that is to say, the time taken up in writing this hand will be $\frac{9}{30} - \frac{2}{30} = \frac{7}{30}$ of the time taken up in writing of the common long-hand, or less than the $\frac{1}{4}$ of the time.

As I have shewn all the principles on which short-hands can be constructed to advantage, I have no need to compare this with any other: because I have enabled every reader to judge of them, by shewing within what limits all improvements are bounded.

2. As it has been suggested to me, that I should take some notice of the plan for a short-hand by S. Jeake Esq; which was lately read before us at the R. S. I take the liberty of addressing to you the following remarks.

A letter from
 John Byrom:
 M. A. F. R. S.
 to the President,

containing
some remarks
on Mr Jeake's
plan for short-
hand. No.
488. p. 388.
June 1748.
Read June 23.
1748.

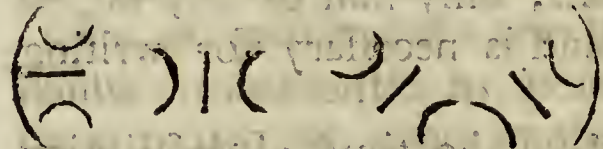
remarks upon it; being obliged to thank you for the public testimony which you were pleased to give, on that occasion, in favour of the method which I had the pleasure of communicating to you; and which in your judgment, confirmed by the experience of many other Gentlemen who have learned it, appears sufficiently to be perfected to demonstration.

His plan, as far as it goes, might have a plausible appearance to a Gentleman, at the first turn of his thought towards short-hand; but a little practical attention must have shown him how liable it was to the very objection that he intended to remedy, viz. ambiguity.

The first mark, for instance (L), in this short specimen, stands for these 4 several words which occur in it, viz. *our, will, evil, ever*; and forty more that one might enumerate, must, whenever they occur, be represented by it; not to mention how often it must occasion ambiguity in the beginning, middle, or end of a longer word, or marks, whereof it is a constituent part only.

Now, though in the Lord's Prayer it is easy, or in casual writing one of his learning and sagacity might be able, by a long familiarity with the characters, to determine the sense of what was written in them, yet it is evident, that, to common learners, a difficulty so perpetually occurring must appear insuperable.

The postulatium, likewise, which this plan for short-hand is grounded on, is taken up too expeditiously; for, there being, in nature, 4 rectilinear strokes, the horizontal, the perpendicular, and the acute, and grave (if I may so call them); it is manifest, by inspection, that from these 4 directions there will arise, at least, 8 curvilinear characters, as each of the strait ones admits distinctly of 2 opposite curves.





and there is no absolute necessity that any of them should be always semicircular; a shape that, for the most commodious combination of simple characters, is in fact much oftener inconvenient than otherwise.

The alphabet then of simple characters may be fairly enlarged by; and room be also left for the fancy of an inventor to extend it farther, if he should find it convenient upon the whole.

I say, upon the whole; for the worst short-hand may happen to express a few particular words better than the best; and arbitrary marks for words or sentences may be often shorter than regular ones: but this is no inducement to write, in one case by a bad method, and in the other, by none at all.

Another oversight, in this plan, is the neglect of beauty and linearity; though the simplicity of it's characters does not, perhaps, admit of such enormous scrawling as others may.

For, to instance again in the specimen; suppose the mark for the word temptation  which expresses a vast variety of different combinations of consonants, to be limited, by a previous knowledge of the language, to that word only, yet, after all, it is a very awkward one; and ought by a common short-hand rule of leaving out such consonants as are not sounded (as the *p* is not in *temptation*) to have been formed in another manner () wherein the beauty and linearity, and of course, the brevity of the mark would have been preserved.

But emendations of this nature would, I doubt, in many cases, which continued writing upon this hypothesis must exhibit, be utterly impracticable.

In short, this Gentleman set out upon right principles, which many hap-hazard undertakers have but little considered; but he had not leisure enough, perhaps, to examine them to the bottom; as was the case with Dr *Green* of *Cambridge* (he that wrote the *Greenian Philosophy*, as he calls it), who formed a short-hand for his own private use, upon much the same plan and principles. He gave me one of his sermons in it; and, upon suggestion of the advantages that he might have taken, he said, that for want of time to consider of his scheme more thoroughly, when he first adopted it, he had overlooked them.

A perfect short-hand, I suppose, would be a solution of some such problem as this: “A language being given, to assign the most compendious method of expressing it readily, and legibly, by an alphabet, and rules, the best adapted to that purpose.”

How easy soever the general principles of such a method may appear to be, there is a deal of nicety required to put them in execution: an exact attention to continual trials and amendments is necessary to ascertain the preference amongst an infinite variety of dispositions, which inventors may select, and throw their characters into. So that it is no wonder, that so many publishers of new short-hands content themselves merely with newness, or at most with some still imperfect meliorations of foregoing attempts.

This Gentleman proceeds no farther than to make an alphabet for his plan*; but must be sensible, that, were it never so complete a one, many compendious applications of it might be obtained by a proper enquiry into the nature of our language (the most happily susceptible of this art of any) and the abbreviations which it admits of, very intelligibly, in writing.

And, in his alphabet, he entirely omits the letter *b* (which is often wanted), and the vowels *a*, *e*, *i*, *o*, and yet retains the vowel *u*, which is certainly as needless as any of the rest: but as a single point, in 5

* Mr *Jeake* only offers his plan as the mere elements of a short hand, leaving it to every practitioner to build upon his foundation, as they shall judge necessary from practice: He retains the *u*, because it often stands for *v* or *ve* or *w*. C. M.

distinct situations, would have provided for them all alike, he might as well have added that to his plan, in order to express any particular vowel, upon occasion; because it would not have hurt his alphabet; and because the reading of his short-hand without any vowels at all, is so extremely difficult.

For, as one of his strait strokes (V) must stand for the words *am, an, in, on, no, me, him, home, &c.* and one of his crooked ones (C) for *as, is, us, so, has, his, ease, ice, use, ax, ox, &c.* and so of the rest; he would himself, in all probability, be often at a loss to distinguish what he had written, upon his own plan*.

The consonants *j* and *v* he has taken no notice of; as if the common way of repeating 24 letters did really give a just idea of an alphabet; which it does not; nor can a perfect short-hand for our language (or any other respectively) well be planned, without considering the real alphabet, or table of every particular sound, or modification of sound; that is to say, vowel or consonant which occurs in it; and then adjusting the proper characters to them, and taking all the advantages that either nature or custom may afford.

I do not recollect, that any author has ever entered into such a disquisition with a view to short-hand; but, with a view to other advantages, many different accounts have been given of a real or universal alphabet; several of them by eminent members of the *R. S.* that proposed by Mr *Lodwick*, in particular, is published in the *Trans.* N^o. 182. but might, I think, be reduced into less compass, and set in a plainer light; and if it be agreeable, I shall, on this occasion attempt to do it.

In the mean time, it will not, I hope, be thought impertinent in me to offer these remarks upon the plan that has been laid before us, of an art which I have taken so much pains to cultivate, and bring to that perfection which my first and last intention of introducing one common standard, for the general practice of it, required.

If I have succeeded, Gentlemen will, I persuade myself, concur to facilitate the design; which though the accidents of life, at a distance from this place, have hitherto retarded, I am intent upon accomplishing to the utmost of my power.

A letter from the same to the Pres containing some remarks on

Mr Lodwick's
alphabet Ibid.
p. 401. Read
June 30. 1748.

3. Mr *Lodwick* premises first the advantages of such an alphabet; which I may as well refer to as repeat; they all center in acquiring, describing, or perpetuating the true sounds of any language, by a standard character for all.

He then defines what a single sound, what a compounded one, a vowel, consonant, diphthong, and triphthong is; all which is likewise sufficiently obvious, and needs no repetition.

* Vowels may be known to be antecedent or consequent, by the mark being written above or below the line of level; e. g. C *as*, C *sa*; V *am*, V *ma*: the ambiguities in many of these words are not important, viz. *as, has, is, his, use, us.*

To proceed directly, therefore, to his alphabet, or collection of all the single vowels or consonants which are used in any language, the number of vowels is, according to him, 14, which are, all but 3, expressed in *English* words in the following table.

| | |
|-------------------------------------|--|
| 1. <i>a</i> ——— <i>tall</i> | 8. <i>ui</i> ——— <i>mais.</i> Low-Dutch. |
| 2. <i>a</i> ——— <i>tallow</i> | 9. <i>y</i> ——— <i>tile</i> |
| 3. <i>a</i> ——— <i>tale</i> | 10. <i>o</i> ——— <i>tone</i> |
| 4. <i>e</i> ——— <i>tell</i> | 11. <i>u</i> ——— <i>tunne</i> |
| 5. <i>ea</i> ——— <i>teal</i> | 12. <i>u</i> ——— <i>une.</i> French. |
| 6. <i>i</i> ——— <i>till</i> | 13. <i>oo</i> ——— <i>tool</i> |
| 7. <i>u</i> ——— <i>dure.</i> French | 14. <i>ou</i> ——— <i>tould.</i> |

These, he says, are the vowels, *each of which is long, and short: short, as in the words God, man, sin; long, as in ball, demand, seen, &c.*

As any vowel sounded by itself is naturally long, I take it for granted that he intended the 14 in this table to be all such; and yet, in the words *tallow, tell, till, tunne*, the vowels, as we now pronounce them at least, are all short; and in the words *tile* and *tould*, a diphthong (or composition of 2 vowels pronounced in the time of one) is sounded: so that there are but 5 long vowels accounted for in our language by the words *tall, tale, teal, tone, tool*. The foreign words, as it would be nice and endless to dispute about, so it comes not within the compass of my present design; which is to give a list of vowels, whereby to discriminate, as conveniently as may be, all the instances of vocality that occur, distinctly, in the *English* language; for which I apprehend that half the number in his catalogue, or 7 characters, would be, to all attainable purposes, sufficient.

As we commonly reckon but 5 vowels in our alphabet, *a, e, i, o, u*; two of which, *viz. i* and *u*, are really diphthongs, I must denote the single vowels by 2 apiece, as custom sometimes does, to shew which I mean; and, beginning from the smallest aperture of the organs to the most dilated, they are these.

| | | | |
|--------------|---------------|---------------|-----------------|
| 1. <i>oo</i> | <i>ooze</i> | <i>stool</i> | <i>too</i> |
| 2. <i>oa</i> | <i>oat</i> | <i>stole</i> | <i>to</i> |
| 3. <i>ee</i> | <i>eel</i> | <i>steel</i> | <i>see</i> |
| 4. <i>ea</i> | <i>eat</i> | <i>steal</i> | <i>sea</i> |
| 5. <i>ai</i> | <i>aid</i> | <i>stale</i> | <i>say</i> |
| 6. <i>aa</i> | <i>aaron</i> | <i>stamen</i> | (sol) <i>fa</i> |
| 7. <i>au</i> | <i>autumn</i> | <i>stall</i> | <i>saw</i> |

Though vocal sounds, like instrumental, may in speculation, admit of numberless distinctions, yet, as experience shews that 7 notes, flattened or sharpened, upon occasion, suffice for a practical gamut,

or scale of music; so I incline to think, that 7 vocal notes or vowels, varied in some correspondent manner, or struck, as one may say, in diphthongal or triphthongal chords with each other, may well enough account for the sounds of our language; or possibly of any other, if it be considered, that different voices, as well as instruments, have somewhat so peculiar in them, that nothing but the ear itself is able to distinguish.

However, as far as these 7 vowels extend, if they were denoted by any common characters, as, suppose at present, by the 7 first numeral figures, the absurd variety which custom has introduced of expressing the same vocal sounds, amongst different nations, even using the same alphabetical characters, would, in a great measure, be immediately corrected.

One instance may serve for many; the word *we* in *English*, and the word *oui* in *French*, however differently written, have the same sound, or composition of sound, from our vowels *oo*, and *ee*, or their *ou* and *i*; if then the figure 1 was always to denote the sound that we express by *oo*, the *French* by *ou*, the *Italians* by *u*, &c. and the figure 3 was to denote our *ee*, and their *i*, in like manner, the combined character, or diphthong 13, would be sounded alike by all readers of any nation, who should previously be agreed upon such a common character.

And allowing Mr *Lodwick's* notion, that there are 3 vowels in other languages, which ours has not, there will remain the figures 8, 9, 0, to express them by ——— or any other characters may be pitched upon. What I aim at is to shew, that, through an over pursuit of accuracy, he has multiplied his vowels, without any apparent necessity.

And the case is the same with respect to his consonants, which he thus ranges into 11 files, and 6 ranks.

| 1. | 2. | 3. | 4. | 5. |
|-----------------|------------------------|---------------------|----------------|----------------------|
| 1 B <i>bond</i> | D <i>dark</i> | J <i>jest</i> | G <i>game</i> | = |
| 2 P <i>pond</i> | T <i>tart</i> | Ch <i>chest</i> | K <i>came</i> | = |
| 3 M <i>mind</i> | N <i>name</i> | gn <i>seign</i> Fr. | ng <i>song</i> | = |
| 4 = | dh <i>this</i> | j <i>jean</i> Fr. | g <i>gaen</i> | Low V <i>Valley</i> |
| 5 = | th <i>thing</i> | sh <i>shall</i> | ch <i>dack</i> | Dutch F <i>Folly</i> |
| 6 | n <i>danse</i> French. | | | |

| 6. | 7. | 8. | 9. | 10. | 11. |
|---------------|-----------------|---------------|---------------|---------------|---------------|
| = | L <i>lane</i> | H <i>band</i> | Y <i>yarn</i> | R <i>rand</i> | W <i>wand</i> |
| = | | | | | |
| = | | | | | |
| Z <i>zeal</i> | lh <i>Welsh</i> | | | | |
| S <i>seal</i> | | | | | |

By this distribution one is led to think, at first, that he would hint that there were in language 11 times 6, or 66 consonants; though experience had taught him to complete the 6 ranks of his second file only.

But, as the mark (\equiv) by which he signifies that there are indeed analogous consonants that might be expressed in those places, but with *a difference that would be too nice for common discernment; and such as he had never heard expressed in any language*; as this mark, I say, occurs but 8 times, I will suppose him to take the real number of consonants in nature to be 37, whereof 29 may answer the purposes of an universal alphabet.

To reduce both files and ranks into less compass, and plainer order, I would take the real consonants of his first and fifth files into one file or rank; and place them in this manner

P. B. M. F. V.

dismissing the 5 unexpressed (and, for any thing that appears, inexpressible ones) as imaginary.

And to this order of consonants all such as are used in our language may be adjusted; for, beginning thus with the labial and labio-dental consonants, and so proceeding to the gutturals, they will stand, in rank and file, after this manner.

P. B. M. F. V.

S. Z. R. Sh. Zh.

T. D. L. Th. Dh.

K. G. N. Ch. J.

wherein their mutual analogy and correspondence seems to appear in the plainest light that one can possibly put them in; as a little attention will better discover, than a prolixity of particulars explain.

If not, I shall be glad to be set right, if I am mistaken in supposing, 1. That here is no consonant omitted, which is really used in our common pronunciation; or, 2. That here are none superfluous, or compounded; or, 3. That, in this view of them, their relation to each other is the most discernible.

I except the letter *H*, which may have it's place amongst the guttural consonantal aspirates, which some foreign nations are accustomed to, but ours, in general, is not: and these, as I conceive, will not be found to be distinctly more than what the addition of a fifth rank to the foregoing may exhaust.

The power or force of this peculiar letter *b* is so capable of intermixture with that of others in this table, and that of the naso-guttural *N*, of sliding, without it's full expression, into a following consonant (as it does particularly in *French* pronunciation), that they have led the writers on this subject to imagine consonants in nature, which they endeavour to express by *N French*, *gn*, *ng*, and by divers changes of the:

the letters *b, n, g*, that give an attentive examiner no clear idea of any distinct consonant, but rather perplex the matter by unintelligible niceties, and hinder the prospect of an universal alphabet, by seeming impossibilities of arriving at it.

T and *W*, which constitute Mr *Lodwick's* 9th and 11th file of consonants, are equal to the vowels *ee* and *oo*: his two *Dutch* consonants may have their place in the 5th rank above-mentioned.

So that omitting such of his 29 ought, if these observations are just, to be omitted, there will remain the 20 that are here digested into 5 files and 4 ranks, for the real alphabet of consonants in the *English* language; together with the *H*, which whether it may be reckoned one or not, I shall leave to the Grammarians.

The 5 files, for an assistance to the memory, may by the help of intermediate vowels, be comprised in 5 technical words of an hexameter verse, such as,

PaSTiKa, BoZDaGo, MeRLiN, FiShThoChe, VeZhaDhJo.

And the 4 ranks, as they begin with the letters *P, S, T, K*, may, for the same reason, be called *prima, secunda, tertia, quarta*, (= *kvar-ta*) *classis*.

These 20 consonants may appear to be too few; but I much question whether the real alphabet of any language has either more in number or better fitted for the purposes of speech than this of ours: the difference lies chiefly in the gutturals, which the *Orientals, Welsh, Germans, &c.* pronounce differently from us.

Our neighbours the *French* (which is very remarkable) few as these 20 are, have but 16 of them; and yet they are able to express themselves with greater fluency and precision than we can well be masters of, till we shall imitate their care to polish and to propagate their language, by some attention to the improvement of our own.

They have none of our *th, dh, ch, or j*; and if a man's name, suppose, were *thatch th' edge*, they would not be able, without previous practice, to pronounce any one of these 4 consonants, which help to compose it, and which custom obliges us to denote, so absurdly, by 9 letters that have not the alphabetical force of any one of these 4 amongst them.

The most important reflection upon the subject is this, that whereas we have in our language but seven distinct sounds or vowels, and thrice the number of stops or modifications of them; if we had accordingly 28 letters or types appropriated to them, and always wrote or printed what we spoke, the theory of reading might be acquired in as few hours, as it costs at present months or years to acquire it in.

A, Is a copper with a still-head. *B*, Is a lead or copper-pipe fixed to the head of the copper, through which the steam from the boiling water heats: in it's passing through the 8 rooms the pipe is fixed to the wall or side of the room in the place of the chimney. *C*, *C*, Are stop-cocks, by which the steam may be suffered to pass fast or slow, as you please. *D*, Is the vent for the steam to pass out at. *E*, Is a cistern of water to replenish the copper as it boils away.

steam of boiling water conveyed in pipes along the walls: and a method of preventing ships from leaking, whose bottoms are eaten by worms: by Colonel W. Cook. (N^o. 476. p. 370. Apr. &c. 1745. Read May 2. 1745.

When your ship's bottom is so eaten by the worms, that she is no longer fit for service, try the following method; viz. first calk well the inside planks or lining; then fill the vacant spaces between the timbers and the out and inside planks, with boiling pitch or resin, so high as the main gun-deck.

The pitch, being put in very hot, will run in every the smallest cranny, and make the ship as tight as a bottle. By this the ship is ballasted; there will be no room left for vermin, as rats, &c. and the pitch will serve for other uses when taken out; therefore it will be but little expence.

Fig 52.

XI. 1. I placed on two garden-pots a dry fir-board which was $\frac{1}{2}$ an inch thick, and 9 inches broad; and covered 9 inches length and breadth of it with an inch depth of damp garden-earth; fencing this earth on each side with two course of bricks, in order to make a fire-place to contain the wood-fuel and live coals; which were frequently blown with bellows, in order to keep the fire to a vigorous heat: this was done for 2 hours continuance, before the fir-board was burnt through; when there was only a weak lambent flame at the under part of the board; for it could not flame out for want of proper fuel; because the substance of the board was reduced to a brittle charcoal, by the heat of the inch-depth of earth which lay on it, which hindered the burning board from flaming. And it was observable, that the edges of the board burnt only with a live coal like a match: being hindered from flaming, by the earth which lay on the board.

A proposal for checking in some degree the progress of fires; by the Rev Stephen Hales, D. D. & F. R. S. N^o. 487. p. 277. Apr. &c. 1748. Read April 21. 1748.

May it not hence be reasonably inferred, that, when a house is on fire, it may be a probable means considerably to retard the progress of the fire, to cover with earth the floors of the adjoining and more distant houses, which stand in the course of the progress of the flames?

The thicker the earth is laid, so much the better: but if time will not permit to lay it more than an inch thick, then supposing 27 men to carry each a cubic foot of earth, which will be a cubic yard of earth, then that cubic yard of earth will cover 36 square yards of flooring; which repeated several times, would soon cover all the floors of a house. And as the fire probably mounts with great fierceness up the stair-case, it will be well to lay much earth on the stairs; which will help to give some check, especially as the earth on the floor and stairs may be wetted by the fire-engine; which moisture will be much the longer

longer retained by means of the earth; whereas water, when not thus retained, soon glides away.

And as fires often catch from house to house at their upper parts, an upper floor covered with earth, with the rafters burning on it, will be longer in burning to such a degree as to fall on the next floor, so, when fallen there, it will also be the longer in burning, and will flame the less, on account of the earth on that next floor; and, consequently, will not be so apt to fire the next house, as in the common case of floors without earth, which must needs therefore burn the more fiercely.

Earth may be had either in back yards, or cellars, or streets.

These hints, from one who never saw a house on fire, will, 'tis hoped, be farther improved by those who have more experience and skill in these affairs.

*An addition
to the preced-
ing paper; by
C Mortimer,
Ibid. p. 382.*

2. Two days after the fire-works had been played off in the *Green-Park* on account of the late peace, I went all over the building erected for that purpose, and was greatly pleased to see the Doctor's scheme confirmed by the practice of the engineers upon that occasion; for the room, in which the trains were fired, and which was immediately under the gratings upon which the 6000 rockets rested and were fired from, had the floor covered over with fine sifted gravel about an inch deep, and the walls were whited over with a dirty sort of white-wash, which I took for lime finely powdered and mixed up with size and water, and done 2 or 3 times over. Both floor and wall were of deal.

*The great be-
nefit of venti-
lators; by the
Rev. Steph.
Hales, D. D.
F. R. S. N^o.
488. p. 410.
June 1748.*

XII. I gave an account in the *General Evening Post* of September last of the great benefit of ventilators in *Newgate*, and in the *Succeſs* frigate for *Georgia*, which lay five months wind-bound in our chanel with the transports for *Cape-Breton*. the rest of which were all very sickly; but in the *Georgia* frigate, in which were about 300 men, all were in good health; and last week I was informed that they got all in health to *Georgia*.

*Extract of a
letter from
Mr Arderon
to Mr Baker,
F. R. S. giv-
ing an account
of the Wea-
ver's Alarm,
vulgò LARUM.
N^o. 477. p.
555. Aug. &c.
1745. Read
Dec. 12. 1745.
Fig. 53.*

XIII. The materials necessary to compose it, are a small candle, of 14 or 15 inches in length, a piece of thread or packthread, a graduated board, and a common stone, or any other ponderous body: but the drawing added hereto (*Fig. 53.*) will fully explain my meaning.

A Represents a board, which hangs commonly against a wall, divided and figured according to the size of the candle made use of *. *B*, A little shelf to place the candle on. *CC*, A thread or packthread, tied fast at *D*, and hanging over a pulley at *E*, whereto a weight is hung at *F*.

* For want of such a board a common ruler is frequently used, to set the number of hours between the flame of the candle and the thread.

By

Fig. 52.

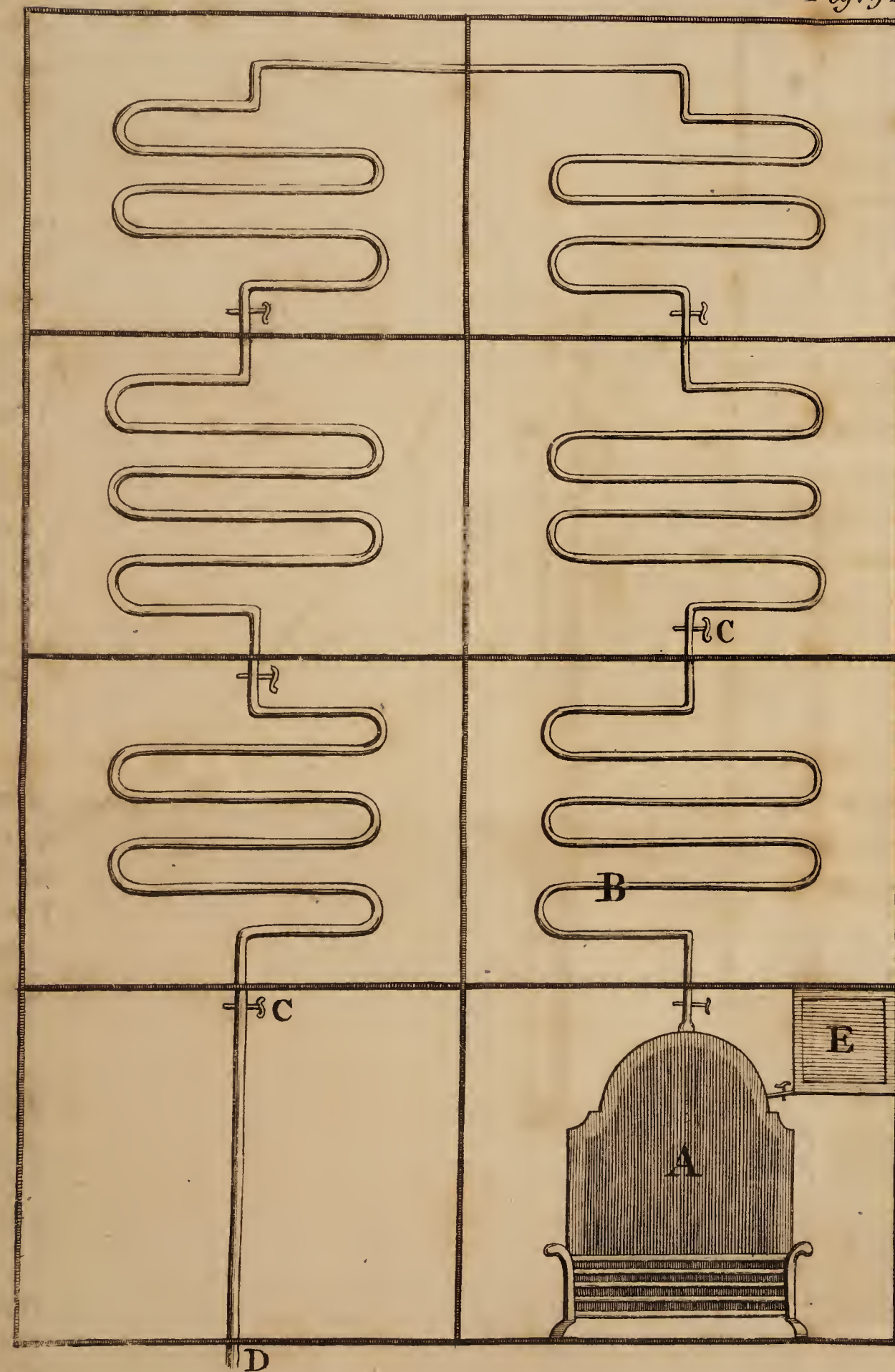
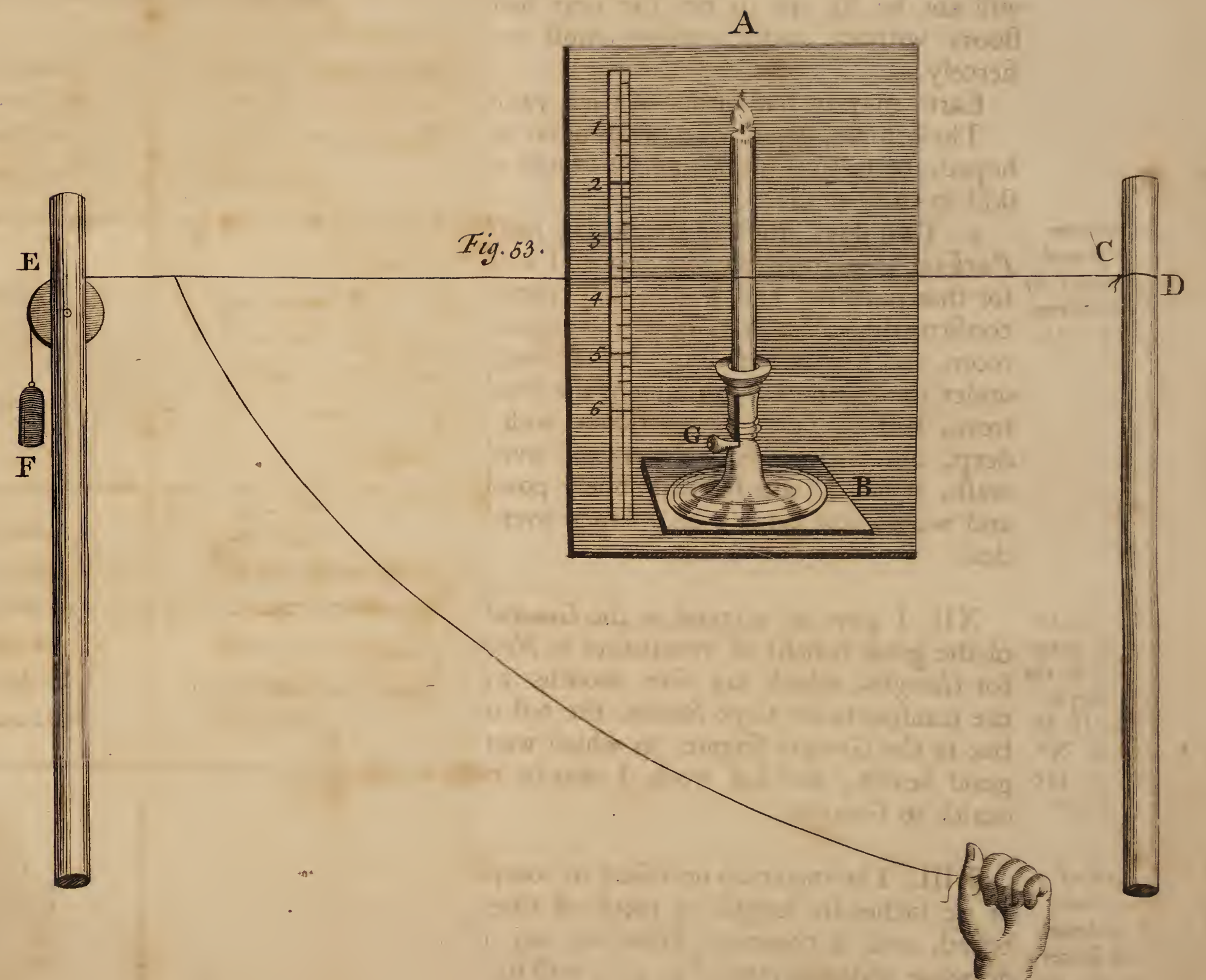


Fig. 53.



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By sliding the spring of the candlestick *G*, up or down, as occasion requires, the flame of the candle is raised as many hours above the thread as the person that adjusts it designs to lie before he is called up. At the desired hour the candle burns the thread in two, the weight falls and, by it's noise seldom fails to wake the person.

But if the man who makes use of this contrivance happens to be of a more than commonly sleepy disposition, in such case another thread is tied to that part of the line *CC* which is next the pulley, and it's other end is twisted round the thumb or wrist of the sleepy person, whereby when the candle burns the line, and the weight falls, he receives such a sudden pull as can hardly fail to wake him, as the drawing will easily explain.

If the line for a few inches on each side the candle be wire, with a short thread only just in the middle where the candle is placed, there can be no danger of doing mischief by the fire's running along the line.

And thus may the poorest mechanic provide himself with an useful servant at a very small expence.

The End of the Tenth V O L U M E.

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